Credit Constraints During the Financial Crisis: A Firm-Level Analysis

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Introduction

Access to finance is a key driver of economic growth.[1] It allows firms to fund both long-term investments and working capital, thereby spurring economic activity. In addition, well-functioning financial markets ensure that funding is allocated to the most profitable investment opportunities in an economy, which leads to a maximisation of social welfare. History, however, has shown that failures in financial markets come at enormous costs. During the financial crisis of 2007-09, for example, many banks ceased to lend and researchers, politicians, and practitioners bemoaned that a credit crunch was hampering economic growth.[2] Governments responded immediately with different policy measures to secure banks' liquidity and ensure that credit continued to flow to the real sector; for instance, bail-out funds for banks were set up and the levels of deposit insurance coverage were increased. Drawing these measures effectively requires a deep understanding of a credit crunch and its impact on the economy.

Empirical research on the causes and consequences of credit constraints provides a solid foundation for designing the right policy measures in response to a credit crunch. In particular, firm-level data allows in-depth analyses along several lines. Which kinds of impairments of credit finance do firms face due to a credit crunch? Even if credit is granted to firms and credit volumes remain stable, do firms suffer from a deterioration of terms and conditions under which they can borrow (e.g., higher interest rates or collateral requirements)? Does firm behaviour alleviate impairments of credit finance? Do firms

[1] See King and Levine (1993), Jayaratne and Strahan (1996), Rajan and Zingales (1998), Beck, Levine, and Loayza (2000), Demirgüç-Kunt and Maksimovic (1998), and Beck, Demirgüç-Kunt, and Maksimovic (2005).

[2] In April 2008, the International Monetary Fund (IMF) estimated that a credit crunch in subsequent quarters could slow down year-on-year GDP growth in the U.S. by 1.4 percentage points (International Monetary Fund (2008), p. 35).

react to credit constraints by using other sources of finance? What are the consequences of credit constraints with respect to real economic activity? Are these real effects caused by credit supply-side factors (e.g., banks ceasing to lend because of liquidity constraints) or by credit demand-side factors (e.g., firms' deteriorating creditworthiness)?

This dissertation addresses these questions by providing three empirical analyses of credit constraints at the firm-level. Chapter 1 shows which kinds of impairments of credit finance firms experienced due to the financial crisis of 2007-09 and whether the risk of facing them was lowered by relationship banking (i.e. the concentration of bank business on a small number of closely connected banks). Chapter 2 assesses firms' behaviour under credit constraints by testing whether they use "Family & Friends" (F&F) finance in response to unsuccessful bank credit negotiations. Finally, Chapter 3 analyses whether the link between credit constraints and real economic activity is caused by credit supply-side factors, or if constrained credit supply is just a mirror picture of firm-side factors that actually hamper real economic activity.

When analysing a credit crunch, it is crucial to understand which symptoms firms could face and how firms' behaviour can help alleviate these. For example, firms can resolve asymmetric information by establishing a close bank relationship to secure access to bank credit (Sharpe, 1990; von Thadden, 2004). This could be of particular importance during times of financial crisis, when high levels of uncertainty keep banks from lending at all or make them do so according to terms and conditions that are less favourable to the firm.

Chapter 1 provides evidence of different kinds of impairments of firms' credit finance due to the financial crisis of 2007-09. The data stems from a survey that was conducted among German firms in September 2011. In the survey, firms indicate which impairments of credit finance they have faced and they report their number of main bank relationships as a measure of relationship banking. Estimations show that relationship banking lowers a firm's risk of facing higher information requirements by banks. It also lowers the risk of facing worse non-price terms of credit (i.e. shorter maturities and higher collateral requirements). However, relationship banking does not affect the probability of experiencing constrained credit availability. Finally, the results are mixed with respect to the impact of relationship banking on the risk of facing higher interest rates due to the financial crisis. The diverging effects of relationship banking on different impairments suggest that soft information, which banks gather over the course of a customer relationship, influences the negotiation of price-terms and non-price terms of credit, but not a bank's credit granting decision.

The key contribution of Chapter 1 stems from the fact that the survey data captures all kinds of impairments within one analysis. In contrast, estimating the impact of relationship banking on loan contract data restricts analyses to firms to which credit was granted and neglects situations in which no credit contract is completed. The analysis of data on rejected credit applications as a measure of credit availability solves this problem, but is limited to firms that have applied for credit. This approach does not account for firms that do not apply for credit because they expect negotiations to be unsuccessful. The analysis in Chapter 1 does not face any of these limitations. Asking firms in a perception-based approach which kinds of impairments of credit finance they have faced allows a broad analysis of the effects of relationship banking.

Besides analysing the symptoms of a credit crunch, the question of how firms' behaviour changes in response to credit constraints must be raised. For example, firms could have a tendency to move into other sources of finance. One of these could be informal finance when sources of formal funding dry up. This is particularly worrying because it keeps potential welfare gains of financial intermediation from being realised and increases firms' borrowing costs (Djankov, Lieberman, Mukherjee, and Nenova, 2003). The use of informal finance has been analysed in previous empirical studies for developing economies and for start-up businesses in developed countries. However, the question of whether established firms in highly developed economies also turn to informal finance when facing credit constraints has been widely neglected in empirical research.

Chapter 2 fills this gap by using a sample of mostly established firms from Germany, a highly developed economy, to analyse whether firms use "Family & Friends" (F&F) finance as a substitute for unsuccessfully negotiated bank credit. In the survey that is also used for the analysis in Chapter 1, firms report whether they receive funding from F&F related to the business ("F&F Business") and from F&F privately connected to the entrepreneur ("F&F Private"). Firms also indicate if their most recent negotiations with banks on a line of credit and/or a loan led to credit being granted. Estimations show that firms use both kinds of F&F finance in response to unsuccessful bank loan negotiations. They do not, however, use it as a substitute for unsuccessfully negotiated lines of credit. Therefore, F&F finance is more important in financing particular investment projects than in working capital finance.

Apart from containing seminal evidence on the use of F&F finance among established firms in Germany, Chapter 2 provides key contributions in the empirical analysis of firms' behaviour under credit constraints. Endogeneity issues are resolved using an instrumental variables estimation approach and sample selection issues are addressed. This stands in contrast to previous literature, which primarily provides evidence on the correlations between the use of informal finance and different firm characteristics.

Knowing that firms' credit finance is impaired, the question arises as to how this affects real economic activity. It is commonly acknowledged that real economic activity and credit market conditions are intertwined (e.g., Bernanke, Gertler, and Gilchrist (1996)). At the firm-level, a correlation between business activity and the experience of credit constraints has two potential explanations. First, credit supply shocks (e.g., a dry-up in the interbank market) could make potential lenders unable to meet firms' credit demand. Without access to credit, firms must postpone investments or reduce their business activity, which leads to lower real economic activity. Second, firm-side factors (e.g., failed business models) could contribute to the slowdown of real economic activity, and banks' reluctance to lend to firms could be a mirror picture of the firms' lack of creditworthiness. Therefore, economic activity could in fact not be hampered exclusively by credit supply-side factors, but also by firm-side factors.

Chapter 3 analyses the question of how to rule out bias from firm-side factors in the estimation of the real effects of credit supply using firm-level data for Germany between 2003 and 2011. The results show that a failure to control for firms' current business situation and future expectations leads to a significant overestimation of credit supply-side effects on real economic activity. In the analysis, a treatment variable indicating constrained credit supply at the firm-level is derived from monthly survey data measuring a firm's perception of bank lending supply. To estimate the treatment effect of constrained credit supply, firms' monthly changes in production and annual employment growth are used as outcome variables. When controlling for balance sheet variables only, estimations suggest that constrained credit supply reduces real economic activity. This effect, however, is significantly reduced when adding survey-based control variables that explicitly capture firms' current business situation and future expectations. In several specifications, the effect even turns insignificant, which suggests that there is no evidence for credit supply-side effects on real economic activity in the data analysed here.

Chapter 3 therefore provides insights into how survey data contributes to the estimation of real effects of credit supply. The results underline that the sole reliance on balance sheet data could lead to incorrect conclusions about the causes and consequences of credit constraints. The shown importance of holding constant contemporaneous and forward-looking credit demand-side factors should be considered in empirical studies estimating the real effects of credit supply.

In summary, this dissertation provides valuable insights into how firm-level data allows the application of advanced econometric methods to draw the correct conclusions with respect to policies in response to credit constraints during times of financial crisis. For example, firm-level data can be used to assess how the resolution of asymmetric information between banks and firms helps alleviate credit financing impairments. This could affect the decision on regulatory measures affecting the flow of information between firms and banks. For instance, requiring banks to base their credit granting decisions on hard facts about the firm could crowd out positive effects of soft information gathered from relationship banking. Furthermore, firm-level data offers unique opportunities to analyse how firms behave under credit constraints. This allows policy makers to react to behaviour that could lead to welfare losses such as firms widely borrowing from unregulated financial institutions or using informal finance as a substitute for bank credit. Finally, the decision whether to take measures addressing banks or firms depends on whether credit constraints are driven by credit supply-side or firm-side factors. For this purpose, firm-level data provides crucial control variables in the analysis of credit constraints and their impact on real economic activity. This dissertation contributes to research along these lines by applying different micro-econometric approaches to seminal firm-level data.

Chapter 1

Relationship Banking During the Financial Crisis: Through Which Channels Did It Work?*

1.1 Introduction

Firms' access to credit finance is impeded by asymmetric information. Banks face problems of adverse selection and moral hazard when they lend to firms. These problems were aggravated by the recent financial crisis when firms operated under increasing uncertainty. In some countries, this arguably contributed to a severe credit crunch.[1] Especially during times of tight credit supply, it is crucial to understand whether firms have less problems with credit finance when they concentrate their bank business on a small number of closely connected banks ("relationship banking") instead of spreading it over a larger number of banks without establishing close ties ("transactional banking"). In particular, we raise the question which kinds of impairments of credit finance due to the financial crisis can be alleviated by relationship banking.

*This chapter is based on joint work with Christa Hainz.

[1] For empirical analyses of banks' credit supply during the financial crisis, see, for example, Ivashina and Scharfstein (2010), Puri, Rocholl, and Steffen (2011a), Popov and Udell (2012), deYoung, Gron, Torna, and Winton (2014), and Jimènez, Ongena, Peydro, and Saurina (2012).

So far, empirical studies have analysed different channels of relationship banking in isolation. Its impact on interest rates and non-price terms of bank credit can be analysed using data from credit contracts. Such data, however, inhibits the analysis of credit availability because it only considers situations in which credit is granted. Estimating the effects of relationship banking on the probability of credit approvals addresses this issue, but it does not account for firms that did not apply for credit in the first place because they expect the application to be rejected, for example.

To capture all potential channels of relationship banking, we use survey data to estimate the effect of relationship banking on four different kinds of impairments of firms' credit finance: higher information requirements by banks, constrained credit availability, increased interest rates, and deteriorated non-price terms of credit. To this end, we shed light on the channels through which relationship banking works. Our analysis is based on data from the Ifo "Financing of the German Economy" survey, which encompasses 1,139 firms from the German manufacturing sector. Using German data is beneficial for this analysis because relationship banking is widespread among German firms and the German manufacturing sector relies heavily on funding from banks. In the survey, each firm reports which kinds of impairments of credit finance it faced due to the financial crisis between 2007 and 2009. A firm's number of main banks serves as a measure of relationship banking, with firms that have only one main bank being considered as following the concept of relationship banking in the narrowest sense.

Our analysis first shows that implementing relationship banking by having only one main bank significantly lowers the probability of higher information requirements by banks and the deterioration of non-price terms (i.e. shorter maturities and higher collateral requirements) due to the financial crisis. Second, the effect of relationship banking on the likelihood of increased interest rates is ambiguous as the risk of this impairment is even lower for firms with two main banks than for those with only one. Finally, relationship banking does not reduce the probability of a firm facing constrained credit availability. To estimate these effects without bias from unobserved heterogeneity, we use a large set of variables to control for other firm-side and bank-side factors.

Although contradictory at first sight, these results can be explained by the role of hard and soft information at different stages of bank credit negotiations. Over the last decades, technological progress and bank regulation induced banks to base their lending decisions primarily on credit scores, which are based on hard information.[2] Therefore, the provision of soft information through relationship banking is no longer affecting a firm's credit availability. If credit is granted, however, the negotiation of the terms and conditions of credit is still affected by the soft information from relationship banking.

Our analysis is most closely related to studies using the number of bank relationships as a measure of relationship banking. In line with our results, Petersen and Rajan (1994) find that a large number of bank relationships increases quoted interest rates. Their analysis is based on U.S. data from the 1987 National Survey of Small Business Finances (NSSBF). Harhoff and Körting (1998) ran a survey similar to the NSSBF among German firms in 1997. They do not find that the number of bank relationships has an impact on interest rates, but provide evidence that a larger number of bank relationships increases the probability that a firm has to pledge collateral, which is in line with our findings. Petersen and Rajan (1994) and Harhoff and Körting (1998) both find that the availability of credit is improved if a firm maintains a smaller number of bank relationships. They measure credit availability indirectly using late payments of trade credit (Petersen and Rajan, 1994) and fast payment discounts not taken (Harhoff and Körting, 1998), arguing that trade credit, the most expensive source of finance for a firm, will only be used if no other source (i.e. bank credit) is accessible. Cole (1998) and Cole, Goldberg, and White (2004) use the 1993 NSSBF to show that a large number of bank relationships lowers the probability that credit is granted by a bank, which constitutes a more direct measure of credit availability.

Other studies use different measures of relationship banking to analyse its impact on credit availability, interest rates, and collateral. Several studies use the length of a rela-

[2] For loans exceeding €750,000, banks in Germany are obliged to make a firm disclose its balance sheets. Furthermore, Basel II bank regulation provided incentives for banks to use ratings to assess the borrower's creditworthiness in order to reduce capital charges (Behn, Haselmann, and Vig, 2014).

tionship,[3] others use the scope of products that the firm receives from a bank[4] to measure relationship banking. In addition, Cole (1998) and Bharath, Dahiya, Saunders, and Srinivasan (2011) use a dummy variable indicating a pre-existing business relationship and Lehmann and Neuberger (2001) use bank-level data to identify relationship banking by asking the bank whether it considers itself to be the main bank of a firm. Overall, these studies do not show a clear effect of relationship banking on credit finance. All these measures of relationship banking focus on features of an individual bank relationship. In contrast, the number of main bank relationships captures a firm's whole portfolio of bank relationships and therefore provides a broader measure of a firm's attitude towards relationship banking.

Furthermore, our analysis relates to studies that analyse the impact of relationship banking on firms' credit finance during times of financial crisis. For the financial crisis of 2007-09, Jimènez, Ongena, Peydro, and Saurina (2012) show that a longer bank relationship increased a firm's probability of getting a loan during the crisis in Spain. Bolton, Freixas, Gambacorta, and Mistrulli (2013) confirm this for Italy and show that relationship banks charge lower interest rates in crisis times. Using data for Korea during the period of 1997-98, Bae, Kang, and Lim (2002) show that a firm's market value is affected by the deteriorating financial health of their main banks. Similarly, Yamori and Murakami (1999) examine the failure of a Japanese bank and its effect on the stock prices of the bank's client firms, thereby evaluating the economic value of the main bank relationship.[5]

We contribute to the literature in important ways to estimate the effects of relationship banking on credit finance. First, we use a perception-based approach to measure impairments of firms' credit finance, an approach that is also used by Campello, Graham, and Harvey (2010), for example. This allows us to analyse the effects of relationship banking

[3] See Petersen and Rajan (1994), Berger and Udell (1995), Harhoff and Körting (1998), Cole (1998), Degryse and van Cayseele (2000), Lehmann and Neuberger (2001), Bharath, Dahiya, Saunders, and Srinivasan (2011), and Santikian (2014).

[4] See Petersen and Rajan (1994), Harhoff and Körting (1998), Degryse and van Cayseele (2000), Cole, Goldberg, and White (2004), and Santikian (2014).

[5] Several other studies estimate the effects of bank health on client firms during financial crises, but do not look at the particular role of relationship banking in this concern (see, for example, Brewer, Genay, Hunter, and Kaufman (2003), Kang and Stulz (2000), and Ongena, Smith, and Michalsen (2003)).

on different aspects of credit finance within one study. In particular, the impact on the probability of higher information requirements by banks has not been analysed in previous studies, although this shows most directly whether relationship banking influences the flow of information between a firm and its banks. Furthermore, firms' perceptions provide comprehensive measures of impairments of credit finance because firms' decisions in response to these impairments (e.g., the cancellation of investment projects) may depend on their perceptions, and not just on factual outcomes of credit negotiations. Second, we amend existing literature by using a firm's number of main banks as a measure of relationship banking, because this is a more precise measure than the number of business relationships to banks in general.

This chapter is organised as follows. In Section 1.2, we derive four testable predictions about how relationship banking could affect a firm's credit finance. Section 1.3 provides information about the data set, our measures of relationship banking, and impairments of credit finance. We show estimation results in Section 1.4 and robustness checks in Section 1.5 before presenting our conclusions in Section 1.6.

1.2 Hypotheses

Based on theoretical models, we build hypotheses about the impact of relationship banking on four kinds of impairments of a firm's credit finance: higher information requirements by banks, constrained credit availability, increased interest rates, and deteriorated non-price terms. For the design of policy measures, it is important to figure out which of these impairments relationship banking alleviates.

First, we expect that banks are less likely to require further information when they are closely tied to the firm through relationship banking. During ex ante screening and interim monitoring, banks often require firms to provide information, which induces costs. For example, the provision of interim financial statements and business plans to a bank is highly burdensome, especially for small firms. This may be particularly prevalent in times of a financial crisis, when banks require more information as uncertainty increases considerably. The theoretical literature argues that a flow of information about the firm is adjunct to loan contracts (Fama, 1985; Sharpe, 1990), and more generally to all kinds of interactions. If this was the case and if such information helped a bank monitor the firm, relationship banking should imply the provision of information, thereby lowering the probability of additional information requirements by banks. Furthermore, as banks also gain from relationship banking, they may put more effort into processing existing information instead of requiring additional ones from their customers.

Hypothesis 1: Relationship banking lowers the probability of higher information requirements by banks due to the financial crisis.

Second, we expect relationship banking to improve a firm's credit availability. Stiglitz and Weiss (1981) present a theoretical model in which asymmetric information leads to credit rationing. Consequently, the resolution of asymmetric information through relationship banking should improve a firm's access to bank credit. Thakor (1996), however, argues that approaching a large number of banks for credit leads to ambiguous effects on overall credit availability. Although the likelihood that at least one bank will grant credit is higher when many banks are approached, each bank's expected profit from screening declines. Therefore, each bank, knowing a firm's optimal application strategy, is more likely to save the screening effort and ration credit when a firm approaches a large number of banks. During the financial crisis, we expect another aspect to affect a firm's credit availability: banks with constrained lending capacities might first lend to firms about which they are best informed. The risk of ending the relationship and destroying "informational capital" accumulated over the course of the relationship may keep the bank from constraining credit to a relationship borrower.

Hypothesis 2: Relationship banking lowers the probability that the availability of bank credit is constrained due to the financial crisis.

Third, with respect to interest rates, the predictions for the impact of relationship banking are somewhat ambiguous. Relationship banks clearly have an informational advantage compared to outside lenders, which gives them an incentive to extract an informational rent from proprietary information ("hold-up problem"). According to Sharpe (1990) and Petersen and Rajan (1995), banks grant lower rates to young firms, and increase them later in the course of the relationship. Thereby, loan contracts do not necessarily break even in every period because both parties agree on (implicit) long-term contracts. Boot and Thakor (1994), however, argue that interest rates decrease over time as banks learn about the quality of a firm and can commit to granting lower rates. Assuming limited monopoly power of the relationship lender, the model by Bolton, Freixas, Gambacorta, and Mistrulli (2013) predicts that relationship banks charge higher interest rates than transactional banks in good times and lower interest rates in bad times.

Hypothesis 3: Relationship banking lowers the probability that interest rates increase due to the financial crisis.

Finally, relationship banking should render the non-price terms of credit less stringent for the firm, although non-price terms differ in nature from the interest rate.[6] With respect to collateral, Inderst and Müller (2007) obtain this result in a model in which a local relationship lender has an informational advantage vis-a-vis transactional lenders. They show that this advantage lowers the lender's collateral requirements. Another aspect of collateral is that it can only fulfill its role of mitigating moral hazard and adverse selection problems if the value of the collateral can be observed by the bank (Rajan and Winton, 1995). Relationship banking leads to proximity between a bank and the firm, which improves the bank's ability to assess the value of the collateral (Boot, 2000). Less uncertainty about the collateral value could lower the overall requirement.

Hypothesis 4: Relationship banking lowers the probability that non-price terms of credit contracts are impaired due to the financial crisis.

[6]While the interest rate induces a pecuniary transfer from the borrower to the bank, non-price terms (such as maturity and collateral requirements) are an outcome of the risk-sharing between the contract parties.

1.3 Data

1.3.1 The Data Set

The following analysis tests the four hypotheses concerning the channels of relationship banking based on data from the Ifo "Financing of the German Economy" survey, the Bureau van Dyk (BvD) Amadeus, and the BvD Bankscope database. The Ifo "Financing of the German Economy" survey was based on a written questionnaire, which was sent to a sample of CFOs of German manufacturing firms in September 2011.[7] All firms are part of the address database of the Ifo Investment Survey for which the Ifo Institute continuously ensures representativeness of the sample of addressees for the German manufacturing sector. In total, 1,139 firms participated in the survey. The response rate was close to 25 percent, leading to a sample in which small firms (less than 50 employees), medium-sized firms (50-249 employees), and large firms (more than 249 employees) are evenly represented.

German manufacturing firms represent an ideal environment to test our hypotheses for two reasons. First, they are highly dependent on bank credit. According to Hainz and Wiegand (2013), 72.9 percent of the firms in the survey reported that they use bank credit. Second, the concept of having a main bank is deeply rooted in the bank-based German economy (see Section 1.3.3). The number of main banks therefore provides an accurate measure of a firm's degree of following the concept of relationship banking.

The Ifo "Financing of the German Economy" survey data is particularly suited to analyse the channels of relationship banking because it contains both a firm's number of main bank relationships and perception-based information on which kinds of impairments of credit finance that firms have experienced due to the financial crisis. In addition, firm and bank balance sheet data from the BvD Amadeus and the BvD Bankscope database provide a large set of control variables. All variables used in the following analysis are described in Table 1.1.

[7] During the six months prior to the survey, personal meetings with executives were scheduled to conduct pre-tests to rule out that the survey design was subject to problems arising from response behaviour. The pre-test talks were also used to ensure that the possible answers listed in the questionnaire captured the firms' reality.

Table 1.1: Variable descriptions

Variable	Description
Impaired	Firm answered the following question with yes: "Was the credit finance of your firm impaired by the financial crisis between 2007 and 2009?"
Information	Higher information requirements from banks
Reduction	Reduction of existing lines of credit
Availability	Constrained availability of new loans or lines of credit
Interest	Increase in interest rates for existing loans or lines of credit
Maturities	Bank credit was offered only at shorter maturities
Collateral	Banks required more collateral
Main bank	Number of main banks
Employees	Number of employees (in heads) at the end of 2010
Assets	Total assets
Equity	Equity/Assets
Long-term debt	Long-term debt/Assets
Cash	Cash / Assets
Return	EBIT / Turnover
Earlypay	Cash discounts drawn / Cash discounts offered to firm
Age	Firm age in years, based on year of foundation
Incorporated	Firm is a corporation by its legal status
Ext. rating	Firm has external rating besides banks' internal ones
Group	Firm is part of a group company
Family	Largest shareholder is a single person or family
Export	Firm generates turnover abroad
Concentration	Share of business that is conducted with the three most important customers

Sources: Ifo "Financing of the German Economy" survey, BvD Amadeus database.

1.3.2 Impairments of Credit Finance

The survey question about impairments of credit finance due to the financial crisis started by asking firms: *"Was the credit finance of your firm impaired by the financial crisis between 2007 and 2009?"*. The descriptive statistics for the dummy variable *Impaired* in Table 1.2 indicate that about 22 percent of the firms in the sample answered this question with "Yes".

Table 1.2: Impairment of credit finance due to the financial crisis

	N	Freq.	Perc.	Min	Max
Impaired	1,062	235	22.13%	0	1
Information	1,046	153	14.63%	0	1
Reduction	1,046	75	7.17%	0	1
Availability	1,046	114	10.90%	0	1
Interest	1,046	112	10.71%	0	1
Maturities	1,046	27	2.58%	0	1
Collateral	1,046	103	9.85%	0	1
Other	1,046	50	4.78%	0	1

Note: The table shows descriptive statistics for all kinds of impairments of credit finance due to the financial crisis; *Impaired* equals one if a firm answered the following question with yes: *"Was the credit finance of your firm impaired by the financial crisis between 2007 and 2009?"*, and zero if a firm answered the question with no; all other variables equal one if a firm has reported the respective impairment and zero if not; firms could report more than one impairment.

To shed light on the channels through which relationship banking works, firms with impaired credit finance were asked what kinds of impairments they experienced due to the financial crisis. The possible answers are listed in the lower part of Table 1.2.

First, the dummy variable *Information* indicates that a firm faced higher information requirements by banks (Hypothesis 1). This impairment was reported by over 14 percent of the firms, which makes it the most frequent of all. Second, variables indicating a reduction of existing lines of credit (*Reduction*) and constrained availability of new loans or lines of credit (*Availability*) are used as measures of constrained credit availability (Hypothesis 2). These impairments were reported by 7 percent and 11 percent of the firms, respectively. Third, *Interest* indicates an increase in the interest rate for an existing loan or line of credit (Hypothesis 3). Almost 11 percent of the firms faced this impairment. Finally, the dummy variables *Maturities* and *Collateral* are used to measure the probability of non-price terms and conditions of bank credit being impaired (Hypothesis 4). The former variable indicates that banks offered credit only for shorter maturities (reported by less than 3 percent of the firms), the latter indicates that banks requested more collateral (reported by almost 10 percent of the firms).

1.3.3 Measuring Relationship Banking

Based on the following theoretical foundation, we use a firm's number of main bank relationships to measure the extent to which it follows the concept of relationship banking. Boot (2000) defines two conditions for relationship banking to be present:[8]

1. Multiple interactions between a bank and its customer over time or across products, through which the bank gathers soft information about the customer.

2. The information gathered is non-public and remains proprietary to the bank.

We argue that these conditions are fulfilled for firms with one main bank, whereas having a larger number of main banks violates the conditions in the following two ways. First, a larger number of main banks indicates that a firm spreads its business among different banks. Consequently, each single bank runs less business with the firm and therefore learns less about it, which violates Condition 1. Second, if there is a larger number of main banks gathering information from interactions with the firm, this information can no longer be considered to be proprietary to each bank, which contradicts Condition 2. Therefore, firms with a larger number of main banks do not apply relationship banking as defined by Boot (2000).

Our focus on main bank relationships amends the approach of using the number of banks from which a firm receives financial services as a measure of relationship banking[9]. The number of main banks is a much clearer signal of relationship banking than business

[8] The "learning theory" of relationship banking contains both elements and is modelled by Bolton, Freixas, Gambacorta, and Mistrulli (2013).

[9] For studies using the number of business relationships to banks as a measure of relationship banking, see, for example, Petersen and Rajan (1994), Harhoff and Körting (1998), Cole (1998), and Cole, Goldberg, and White (2004).

relationships to banks in general and therefore gives us a more stringent view of a firm's attitude towards relationship banking. In addition, our study is based on information provided by the firm. This provides a better insight than data from banks' perceptions on whether they consider themselves to be the main bank of a firm (Elsas, 2005; Lehmann and Neuberger, 2001).[10]

Table 1.3 shows the distribution of the number of main banks of the firms in the sample. The largest share of firms (40.4 percent) follows the principle of relationship banking by having only one main bank, which underlines the importance of the main bank concept for German firms. 37 percent of the firms have two main banks. About 22 percent, however, report that they do not have a main bank at all or that they have three and more main banks. For the following empirical analysis, these two groups of firms are defined as not following the concept of relationship banking.

Furthermore, impairments of firms' credit finance due to the financial crisis are linked to the number of main bank relationships in Table 1.3. The fraction of firms whose credit finance was impaired by the financial crisis is lowest among firms with one main bank (15.7 percent). The fraction is slightly higher among firms with a second main bank (21 percent) and by far highest among firms with zero or three and more (30.6 percent). The probability of higher information requirements by banks is smallest for firms that apply relationship banking. For the reduction of existing lines of credit, however, there are hardly any differences between firms with different numbers of main banks. Increased interest rates are the least likely for firms with two main banks, while the likelihood of deteriorated non-price terms is lowest for firms with one main bank.

These descriptive statistics provide initial evidence showing that the gains from relationship banking seem to outweigh the benefits from creating a competitive situation between a large number of banks. The probabilities of different kinds of impairments of credit finance, however, appear to be not equally affected by relationship banking.

[10] To gain an impression of what exactly a main bank is for a firm, the data set provides information on whether certain features characterise the two most important bank relationships and if the respective bank is a main bank. This leads to a sample of over 1,600 main bank relationships for which features are listed in Table A.2 in the Appendix. The most important criteria for main banks are the long duration of a relationship, personal support by the bank, and the short distance between the headquarter of the firm and the bank. Only 32 percent of the firms report that the main bank is the most important creditor.

Table 1.3: Relationship banking and impaired credit finance

	Main bank (1)	*Main bank (2)*	*Main bank (0/3+)*
N	1,042	1,042	1,042
Freq.	421	386	235
Perc.	40.40%	37.04%	22.55%
Impaired	15.68%	20.98%	30.64%
Information	9.03%	15.28%	23.83%
Reduction	5.94%	8.03%	8.09%
Availability	8.08%	11.92%	14.47%
Interest	8.55%	8.29%	18.72%
Maturities	0.95%	2.85%	5.11%
Collateral	6.65%	11.40%	13.19%
Others	3.33%	4.66%	7.66%

Note: The table shows descriptive statistics for firms' number of main banks and all kinds of impairments of credit finance due to the financial crisis; *Impaired* equals one if a firm answered the following question with yes: *"Was the credit finance of your firm impaired by the financial crisis between 2007 and 2009?"*, and zero if a firm answered the question with no; all other variables equal one if a firm has reported the respective impairment and zero if not; firms could report more than one impairment.

1.3.4 Control Variables for Firm Characteristics

To rule out that the correlation between relationship banking and impairments of firms' credit finance is driven by hitherto unobserved firm characteristics, the following estimations incorporate a set of control variables from survey data and firms' balance sheets. We expect these to determine both a firm's creditworthiness and its attitude towards relationship banking. All variables are listed and summarised in Table 1.4.

First, the variables *log(Employees)* and *log(Assets)* in Table 1.4 show that firms that apply relationship banking are smaller than firms that do not. Firm size is also widely acknowledged to affect a firm's access to credit. Large firms often have demand for large scale funding. Access to large amounts of credit may be difficult when banks are short of lending capacities and want to diversify their risk exposure across several firms, rather than clustering risks by granting large loans to few firms. On the other hand, asymmetric information might be lower for large firms as they are typically older and more transparent, which could lower the risk of impaired credit finance.

Furthermore, a broad set of variables to control for a firm's financial situation constitute the most obvious predictors of impaired credit finance. Table 1.4, however, does not provide a clear picture of the correlation between a firm's financial situation and its attitude towards relationship banking.

Table 1.4: Firm characteristics and relationship banking

	Main bank (1) (N=271)	*Main bank (2)* (N=254)	*Main bank (0/3+)* (N=134)
log(Employees)	4.2	4.4	5.9
log(Assets)	8.5	8.8	10.4
Equity	31.48%	34.39%	34.69%
Long-term debt	28.00%	25.80%	21.40%
Cash	10.22%	12.39%	8.85%
Return (<3%)	42.80%	37.80%	47.01%
Return (3% to <7%)	33.21%	38.19%	35.07%
Return (7% to <10%)	16.24%	14.57%	9.70%
Return (10% +)	7.75%	9.45%	8.21%
Earlypay (0%)	3.32%	3.15%	5.22%
Earlypay (0% to <25%)	18.08%	11.81%	12.6â€š9%
Earlypay (25% to <50%)	11.07%	13.39%	5.97%
Earlypay (50% to <75%)	12.92%	9.45%	14.93%
Earlypay (75% +)	54.61%	62.20%	61.19%
log(Age)	3.9	4.1	4.2
Incorporated	69.74%	64.17%	70.15%
Ext. rating	20.66%	20.47%	21.64%
Group	25.83%	26.38%	42.54%
Family	79.70%	84.65%	76.87%
Export	85.24%	87.40%	90.30%
Concentration (<10%)	15.50%	13.78%	26.87%
Concentration (10% to <30%)	49.08%	46.46%	42.54%
Concentration (30% to <50%)	18.45%	25.20%	16.42%
Concentration (50% +)	16.97%	14.57%	14.18%

Note: The table shows descriptive statistics for all firm characteristics that are used as control variables in the estimations in this study; see Table 1.1 for a description of all variables.

For the balance sheet variables *Equity*, *Long-term debt*, and *Cash*, we calculate the average value of the balance sheets in 2007, 2008, and 2009 in order to capture a firm's financial situation during the entire financial crisis. Data on return on sales (*Return*) and taken early payment discounts (*Earlypay*) are only available in categorical variables so that we have to use the respective variables in 2008 as control variables, instead of calculating average values of the three crisis years.

In addition to firm size and balance sheet variables, survey data provides additional control variables for firm age (*log(Age)*), transparency (*Incorporated*, *Ext. rating*), ownership (*Group*, *Family*), and export status. All these variables are commonly used as predictors of firms' access to credit finance. Table 1.4 shows that firms that implement relationship banking do not differ much in age and transparency from those that do not, but the latter are more likely to belong to a group company, less likely to be family-owned, and less likely to export.

Finally, the concentration of bank business on one main bank could be driven by a firm's risk preferences. We approximate this with the share of a firm's business that is concentrated on the three most important customers (*Concentration*). Table 1.4 shows that firms that concentrate business on one main bank also have a high customer concentration, whereas firms that do not do so also spread their own business across customers.

1.3.5 Control Variables for Bank Characteristics

Estimating the effects of relationship banking on impairments of credit finance raises the need to control for bank-side effects. Empirical research has shown that bank balance sheet channels are an important determinant of firms' access to credit[11] and that banks differ in their business model with respect to being a relationship lender (e.g., Bolton, Freixas, Gambacorta, and Mistrulli (2013)).

To rule out estimation bias from bank-side factors, we use the fact the German banking system consists of different classifications of banks that differ in their financing structure, their lending behaviour during the financial crisis, and their business model with respect to being a relationship bank.[12] First, commercial banks are universal banks engaged in both corporate banking and investment banking. They are privately owned and only to a small extent deposit-financed. Second, savings banks are publicly owned and serve

[11]See, for example, Ivashina and Scharfstein (2010), Popov and Udell (2012), Jimènez, Ongena, Peydro, and Saurina (2012), and deYoung, Gron, Torna, and Winton (2014).

[12]See Hackethal (2004) for a detailed description of the German banking system.

public interest by taking in deposits and providing credit to borrowers within their region. Third, Landesbanken, which are also publicly owned, provide services to federal states and offer large scale funding to companies, which cannot be offered by small savings banks. The fourth classification contains cooperative banks. Like savings banks, they only operate within their region and are largely deposit-financed. Cooperative banks are owned by their members, whose interest they serve.

Table 1.5: Bank classifications, impaired credit finance, and relationship banking

	Commercial	*Savings bank*	*Landesbank*	*Cooperative*	*Other*
Freq.	401	373	63	214	93
Perc.	61.41%	57.12%	9.65%	32.77%	14.24%
Impaired	21.45%	21.18%	23.81%	17.76%	23.66%
Information	15.71%	13.94%	17.46%	12.15%	16.13%
Reduction	6.98%	6.43%	3.17%	5.61%	5.38%
Availability	10.22%	11.26%	7.94%	8.88%	8.60%
Interest	11.22%	10.19%	15.87%	9.81%	13.98%
Maturities	2.74%	2.41%	4.76%	0.93%	3.23%
Collateral	10.47%	9.92%	4.76%	11.68%	12.90%
Others	3.49%	3.22%	4.76%	2.34%	9.68%
Main bank (1)	37.66%	41.82%	30.16%	45.33%	46.24%
Main bank (2)	38.15%	42.36%	38.10%	41.59%	30.11%
Main bank (0/3+)	24.19%	15.82%	31.75%	13.08%	23.66%

Note: The table shows descriptive statistics for bank-side variables; *Commercial*, *Savings bank*, *Landesbank*, *Cooperative*, and *Other* equal one if a firm reports that at least one of its two most important banks belongs to the respective classification of banks.

The data provides the classification of the two most important banks of each firm. We expect this to be a sufficient control variable for bank-side factors for two reasons. First, banks' attitude towards being a relationship lender is unlikely to differ between individual banks within each classification so that no further bias reduction stems from controlling for bank-level data. Second, the majority of firms in our sample have one or two main banks so that it is reasonable to assume that impairments of credit finance are primarily determined by the two most important banks. Nevertheless, we use control variables from bank balance sheets as a robustness check in Section 1.5.3.

Table 1.5 shows that impaired credit finance (*Impaired*) is most likely for firms that have a Landesbank and the least likely for firms that have a cooperative bank among the two most important banks. This was to be expected because Landesbanken were seriously affected by the financial crisis and therefore most likely to cause impairments of firms' credit finance. Savings banks and cooperative banks are expected to be the least likely to do so because they are largely deposit-financed and Ivashina and Scharfstein (2010) show, albeit for U.S. data, that deposit-financed banks reduced lending to firms less drastically in response to the financial crisis than other banks. Furthermore, Table 1.5 provides clear indications that different kinds of impairments are associated with having different classifications of banks among the two most important bank relationships.

Table 1.5 also shows that the fraction of firms that do not implement relationship banking is highest among firms with commercial banks and Landesbanken among the two most important banks. Relationship banking is more widespread among firms for which savings banks and cooperative banks are most important. This could be explained differences in business models between banks. Savings banks and cooperative banks serve customers only within their region, which makes the establishment of close long-term bank-firm relationships likely. In addition, firms that rely on Landesbanken and commercial banks could maintain more bank relationships because they anticipate difficulties in getting credit from these banks. According to the theoretical model of Detragiache, Garella, and Guiso (2000), firms with only one bank relationship face the risk of adverse selection if the relationship bank cannot roll over loans due to liquidity constraints and a firm has to address banks that have not learned about a firm's credit quality before. Establishing multiple bank relationships reduces this risk.

In sum, Table 1.5 shows that bank-side factors affect both a firm's risk of experiencing different kinds of impairments of credit finance and its probability of following the concept of relationship banking. Therefore, controlling for these bank-side factors is crucial to rule out that they induce bias in the estimated effects of relationship banking on the probability of impairments of credit finance.

1.4 Methodology and Results

1.4.1 Methodology

To investigate the channels of relationship banking, we run separate OLS estimations in which the dependent variables indicate the different kinds of impairments as listed in Table 1.2 in Section 1.3.2. For example, to test whether relationship banking affects the probability of facing higher information requirements by banks due to the financial crisis, we estimate the linear probability model

$$Information_i = \alpha_0 + \alpha_1 MB_i + \alpha_2 Firm_i + \alpha_3 Bank_i + \alpha_4 Industry_i + \epsilon_i \tag{1.1}$$

where MB_i is a set of the two dummy variables *Main bank (1)* and *Main bank (2)* so that firms whicht do not use relationship banking by having zero or three and more main banks constitute the baseline category. $Firm_i$ is the set of firm characteristics listed in Table 1.4 in Section 1.3.4, $Bank_i$ is the set of dummy variables for having the respective classification of banks among the two most important banks, and $Industry_i$ is a set of industry dummy variables based on the two-digit WZ 2008 industry classification.

1.4.2 Relationship Banking and Impairments of Credit Finance

Confirming Hypothesis 1, the results in Estimation (1) in Table 1.6 show that the probability of facing higher information requirements by banks due to the financial crisis is about 15 percentage points lower for firms that concentrate on one main bank relationship relative to firms that do not follow the concept of relationship banking. The effect of concentrating business on two main banks is also statistically significant, but substantially smaller. The difference between the two coefficients is not statistically significant.[13] This result supports the view that information provision through relationship banking

[13] Throughout the estimations in this study, the difference between the estimated coefficients of *Main bank (1)* and *Main bank (2)* turn out to be statistically insignificant, which is why test statistics are not reported for the sake of conciseness.

limits asymmetric information, which facilitates the bank's screening process and interim monitoring. This reduces the necessity to require more information from the firm.

Not supporting Hypothesis 2, Estimations (2) and (3) show that relationship banking does not affect a firm's credit availability. The number of main banks does not have a significant impact on a firm's probability of experiencing the reduction of existing lines of credit or constrained availability of new loans or lines of credit. This could be explained by the banks' credit granting decisions being primarily based on credit risk models. Since these models only process hard information and do not account for soft information, it is reasonable that we do not find a statistically significant effect of relationship banking on a firm's credit availability.

Furthermore, Estimation (4) indicates that relationship banking lowers the likelihood of an increased interest rate for existing loans or lines of credit by almost 10 percentage points. The effect of having two main banks, however, is even larger, which is not entirely in line with Hypothesis 3. A potential explanation for this result stems from the fact that interest rates are subject to a bargaining process between a bank and a firm. Having a second main bank may improve a firm's bargaining position and prevent hold-up problems faced by firms with only one main bank.

Finally, we assess how impairments of non-price terms of credit are affected by relationship banking (Hypothesis 4). According to Estimation (5), having only one main bank lowers the probability that banks offer credit only at shorter maturities significantly by almost 5.5 percentage points. When looking at higher collateral requirements in Estimation (6), the effect is even larger at 7.9 percentage points. There is no such effect for firms with two main banks, which indicates that relationship banking affects non-price terms of credit differently than interest rates. In contrast to interest rate payments, these non-price terms are not a pecuniary transfer and do not immediately affect the profit of the bank. Instead, they are part of the risk-sharing process between the bank and a firm, which does not seem to be affected by hold-up problems and their resolution through competition between banks. One could also argue that soft information from relationship banking plays a larger role in the risk-sharing process than in bargaining about interest rates.

Table 1.6: Baseline OLS estimations

	(1) *Information*	(2) *Reduction*	(3) *Availability*
Main bank (1)	-0.1543***	0.0017	-0.0371
	(0.05)	(0.03)	(0.04)
Main bank (2)	-0.1044**	0.0125	-0.0298
	(0.05)	(0.03)	(0.04)
Firm char.	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Bank class.	Yes	Yes	Yes
Adj. R^2	0.0522	0.0164	0.0441
N	652	652	652

	(4) *Interest*	(5) *Maturities*	(6) *Collateral*
Main bank (1)	-0.0990**	-0.0547**	-0.0789**
	(0.04)	(0.02)	(0.04)
Main bank (2)	-0.1122***	-0.0455**	-0.0472
	(0.04)	(0.02)	(0.04)
Firm char.	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Bank class.	Yes	Yes	Yes
Adj. R^2	0.1059	0.0183	0.0275
N	652	652	652

Note: The table shows OLS estimation results for regressions of dummy variables for different kinds of impairments of credit finance due to the financial crisis on the number of main bank relationships, a set of control variables for firm characteristics, bank classification dummy variables, and industry dummy variables; the baseline category for the main bank relationship dummy variables is *Main bank (0/3+)*; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In sum, firms that implement relationship banking have a lower probability of facing impairments of information requirements and non-price terms of credit due to the financial crisis. Credit availability, however, is not affected by relationship banking. For interest rates, we find a positive effect of concentrating business on one main bank, but evidence suggests that having a second main bank is even more advantageous.

1.4.3 Other Determinants of Impairments of Credit Finance

Table A.3 in the Appendix shows the coefficients of all control variables. As expected, the probabilities of all kinds of impairments of credit finance are lower for firms in a better financial situation. Furthermore, export activity and incorporation increase the probability of facing higher interest rates.

In addition, Table A.3 provides the estimated effects of having different classifications of banks among the two most important banks. Most of the effects are statistically insignificant, so that the results provide only weak evidence of bank-side effects on the firms' risk of facing different kinds of impairments of credit finance.

1.5 Robustness Checks

1.5.1 Estimation of Nonlinear Models

Instead of estimating a linear probability model, the effects of relationship banking could be estimated using nonlinear estimation procedures based on the following model:

$$Pr(Information_i = 1) = \Phi(\alpha_0 + \alpha_1 MB_i + \alpha_2 Firm_i + \alpha_3 Bank_i + \alpha_4 Industry_i) \quad (1.2)$$

where $\Phi(\cdot)$ is the cumulative distribution function of a standard normal distribution. Table A.4 in the Appendix shows that the binary probit marginal effects differ somewhat in size from the OLS estimators in Table 1.6. However, they confirm our findings that relationship banking works through information requirements and non-price terms of credit, but does not affect credit availability. The results for interest rates remain ambiguous.

Binary probit estimations, however, induce the problem that many observations are dropped because some control variables (mainly the industry dummy variables) perfectly predict the value of the outcome variable. Table A.4 in the Appendix shows that this is particularly problematic in Estimation (4) when using *Maturities* as the dependent variable. Therefore, linear probability models are preferable as baseline estimations.

1.5.2 Addressing Potential Reverse Causality

Challenging our interpretation of the results in Section 1.4, one could argue that firms have reacted to impaired credit finance by setting up further main bank relationships, which would raise endogeneity concerns. From this perspective, our estimations would suggest that firms with a high probability of impaired credit finance maintain more main bank relationships, instead of vice versa.

First, an argument against reverse causality can be derived from the characteristics of main bank relationships. As Table A.2 in the Appendix shows, a long duration of the bank relationship is a key criterion for a firm to refer to the respective bank as a main bank. Even if firms had added business relationships to banks when experiencing impaired credit finance, these would not constitute long-term relationships. Therefore, it is unlikely that this increased the number of main banks reported in the survey.

Second, we test for the impact of reverse causality by re-running all estimations after excluding firms with potential dynamics in their main bank relationships. The survey data contains the durations of the two most important bank relationships of every firm. We therefore know whether these were established during the financial crisis, or before. This allows us to identify firms with dynamics in the structure of their bank relationships, which may have been caused by the financial crisis and its impact on a firm's credit finance. In total, 153 firms are dropped from the sample because their first or second most important bank relationship was established in 2007 or later.[14]

[14]The reduction in the number of observations used in the estimations is smaller as some of the dropped firms were not considered in the first place because of missing values.

If reverse causality would drive our results, dropping these firms should affect the estimated effects of relationship banking. The comparison of the results in Table A.5 in the Appendix to Table 1.6 shows that this is not the case. For the firms left in the sample, we find that relationship banking affects credit finance by lowering the probability of higher information requirements and impaired non-price terms of credit due to the financial crisis. We do not find any effects of relationship banking on credit availability. The ambiguous results for interest rates do not change either. Therefore, estimations without firms with young important bank relationships allows us to rule out that our findings are driven by reverse causality.

1.5.3 Controlling for Bank Balance Sheet Data

Another problem of the estimations in Section 1.4 could stem from the fact that the bank classification dummy variables do not rule out estimation bias from bank-side factors entirely. If banks within the different classifications differ with respect to their lending during the financial crisis (as shown for German savings banks by Puri, Rocholl, and Steffen (2011a)) and in their attitude towards being a relationship bank, our results could be driven by remaining heterogeneity in bank characteristics.

To test the robustness of our results with respect to bank heterogeneity within bank classifications, we re-run our estimations using bank balance sheet variables as control variables instead of bank classification dummy variables. Unfortunately, data from the Ifo "Financing of the German Economy" survey does not allow the identification of the banks to which firms maintain business relationships. Therefore, we draw this information from the BvD Amadeus database. We then link the balance sheet of each of these banks to the firm-level data set.

We control for banks' total assets, their equity ratio, their liquidity ratio, and their deposit-financing ratio. Each ratio is calculated relative to total assets. We measure banks' financial situation during the entire financial crisis by calculating the average of these balance sheet items over each bank's balance sheets from the years 2007-2009.

Table 1.7: Bank balance sheets and relationship banking

	Main bank (1)	*Main bank (2)*	*Main bank (0/3+)*
N	649	649	649
Freq.	266	251	132
Perc.	40.99%	38.67%	20.34%
$log(Bank\ assets)_{max}$	11.8	12.4	13.5
$log(Bank\ assets)_{min}$	8.2	8.1	8.7
$Bank\ equity_{max}$	6.03	6.06	5.83
$Bank\ equity_{min}$	3.28	2.98	2.34
$Bank\ liquidity_{max}$	27.44%	28.32%	31.91%
$Bank\ liquidity_{min}$	16.99%	14.67%	13.50%
$Bank\ deposits_{max}$	65.53%	67.31%	63.29%
$Bank\ deposits_{min}$	41.58%	38.29%	28.25%

Note: The table shows descriptive statistics for bank balance sheet variables; for each variable, the maximum and minimum value across all banks to which a firm maintains a business relationship according to the BvD Amadeus database are presented.

As different firms work with different numbers of banks, we have to control for the financial situation of the whole portfolio of banks. We do so by using the maximum and minimum values for every variable across the balance sheets of each firm's banks.[15] Table 1.7 provides descriptive statistics for the bank balance sheet variables depending on a firm's number of main bank relationships. We find that firms that follow the concept of relationship banking work with somewhat smaller banks. Their banks also have higher equity ratios and rely more on deposit financing. The picture for banks' liquidity is mixed as minimum and maximum liquidity across a firm's banks show opposing correlations with relationship banking.

When controlling for bank balance sheet variables instead of using bank classification dummy variables, estimation results in Table A.6 in the Appendix confirm most of our previous findings. Concentrating on one main bank relationship still reduces the risk of higher information requirements significantly. The effect of having two main banks is also statistically significant, but substantially smaller. There is, as before, no effect of relationship banking on credit availability and the results with respect to interest rates

[15]Without reporting the results in this chapter, we also run estimations using only maximum values, only minimum values, or mean values across the balance sheets of each firm's banks as control variables. The results for the effects of relationship banking on the different kinds of impairments of credit finance due to the financial crisis are not altered by using different approaches.

remain ambiguous. There is still a significantly lower risk of the impairment of maturities when following relationship banking. However, the risk of higher collateral requirements is no longer significantly affected by relationship banking. Table A.6 in the Appendix also provides the estimated coefficients of the different bank balance sheet variables. However, it is hard to interpret these because using minimum and maximum values leads to problems with multicollinearity between these variables. This, however, does not bias the estimated effects of relationship banking.

1.6 Conclusion

This chapter addresses the question through which channels relationship banking affects firms' credit finance during times of financial crisis. We test whether applying relationship banking by focusing business on one main bank relationship lowered a firm's risk of facing different kinds of impairments of credit finance due to the financial crisis of 2007-09. Our results show that relationship banking lowers the probability that a firm reports higher information requirements by banks due to the financial crisis. However, relationship banking does not significantly affect the impairment of firms' credit availability. For the interest rate, our results are ambiguous. Here, it seems that a firm benefits from maintaining only one main bank relationship, but having a second main bank is even more advantageous. For non-price terms (i.e. maturity and collateral), relationship banking lowers the probability of impairments significantly.

The effect of relationship banking on information requirements is the most direct evidence that banks gather information about firms from a bank-firm relationship. The divergent results for credit availability and the other channels of relationship banking might be due to the fact that banks use hard rather than soft information in their credit risk models on which they base their credit granting decision. If credit is granted, however, soft information from relationship banking affects the negotiation of the credit contract. This holds even more for non-price terms and conditions that determine the risk-sharing between banks and firms than for the pecuniary transfer constituted by interest payments.

These findings underline that relationship banking was beneficial for firms (and thereby for the whole economy) during the financial crisis, although it was perceived as somewhat superseded before the crisis. Therefore, in times of uncertainty, regulators should facilitate the flow and use of information from bank-firm relationships. Many magazines and guidebooks for practitioners also picked up on this topic. For example, a report by Deloitte & Touche GmbH (2012) concludes that: "the quality of the relationship of medium-sized firms and their banks is often underrated and thereby neglected. In particular during a crisis, a long-term and positive relationship is of utmost importance." Our analysis provides strong support for this view.

Chapter 2

Friendship and Money, Oil and Water? Credit Constraints and "Family and Friends" Finance

2.1 Introduction

Facing credit constraints induces firms to seek other sources of funding. Recent empirical evidence shows that many firms used more public market finance[1] and trade credit[2] in response to the financial crisis of 2007-09. Alternatively, firms may seek capital from informal sources like "Family and Friends" (F&F) in response to bank credit constraints.[3] When going informal, welfare gains from financial intermediation through banks no longer materialise. Djankov, Lieberman, Mukherjee, and Nenova (2003) argue that firms may then face higher borrowing costs and that "finance from friends and family is unreliable, untimely and can bear significant non-financial costs".

[1]See, for example, Adrian, Colla, and Shin (2012), Barraza, Lee, and Yeager (2014), and Becker and Ivashina (2014)

[2]See, for example, Carbó-Valverde, Rodríguez-Fernández, and Udell (2013) and Coulibaly, Sapriza, and Zlate (2013)

[3]Other informal sources such as moneylenders are widespread in developing countries, but are not common in developed economies.

Empirical evidence of the use of informal finance has been primarily provided in the context of the capital structure of start-up businesses in developed economies and financial systems in developing countries.[4] However, not much attention has been paid to the question whether informal finance is also used by established firms in highly developed countries. Furthermore, previous studies analyse correlations, but do not estimate the causal effect of bank credit constraints on the use of informal finance.

This chapter analyses the use of F&F finance by non-start-up firms in a highly developed economy and tests whether it is driven by unsuccessful bank credit negotiations. The analysis is based on data from the Ifo "Financing of the German Economy" survey, which was conducted among German manufacturing firms in September 2011. The data distinguishes between F&F finance from sources connected to the business ("F&F Business") and sources privately connected to the entrepreneur ("F&F Private"). Descriptive statistics show that 15.46 percent of the firms in the sample use at least one of the two kinds of F&F finance, which is surprisingly high. The data also contains detailed information on each firm's last bank credit negotiations between 2008 and 2011. The causal effect of unsuccessful bank credit negotiations on the use of F&F finance is estimated using instrumental variables (IV) estimations to deal with endogeneity from a potential signalling effect of F&F finance on a firm's likelihood of receiving bank credit.

IV estimations show that firms use both kinds of F&F finance in response to unsuccessful negotiations of bank loans. This effect, however, is not found for unsuccessful negotiations of lines of credit. Therefore, F&F finance seems to be especially relevant in financing particular investments (substituting loans), but not as much in financing working capital (substituting lines of credit). The comparison of OLS and IV estimators indicates that "F&F Business" serves as a positive signal of a firm's creditworthiness in bank credit negotiations. This is in line with Berger and Udell (1998), who argue that

[4] Allen and Qian (2010), Allen, Qian, and Zang (2011), and Allen, Carletti, Qian, and Valenzuela (2013) provide theories on the finance-and-growth-nexus arguing that informal finance may have advantages over formal finance in supporting economic growth in developing countries. This is supported by the analysis of data from India (Allen, Chakrabarti, De, Qian, and Qian, 2012) and China (Allen, Qian, and Qian, 2005). Also using Chinese data, Ayyagari, Demirgüc-Kunt, and Maksimovic (2010), however, do not find a relationship between the use of informal finance and economic growth. Degryse, Lu, and Ongena (2013) find a positive complementary effect of formal and informal finance on sales growth for small firms in China, but not for large firms.

different financial instruments are interconnected and initial insider finance often serves as a "predicament" for receiving external finance. For "F&F Private", however, evidence of its signalling effect is ambiguous.

This study is most closely related to a strand of research on the use of F&F finance and other kinds of informal finance. The theoretical model by Myers and Majluf (1984) predicts that firms' financing decisions follow a pecking order. Because of asymmetric information, firms preferably rely on internal funding (i.e. retained earnings). If they need external finance, they prefer debt over equity instruments. However, Myers and Majluf (1984) do not explicitly state the role of F&F finance in the pecking order. Filling this gap, Berger and Udell (1998) find that "insider finance" in the form of debt or equity from the start-up team, family, and friends plays a role in the early stages of development, but is phased out as the firm matures. This has been supported by several empirical studies on the determinants of the use of different kinds of informal finance in start-up businesses and small firms.[5] Altogether, these studies find that younger, smaller, less transparent, and less financially sound firms are more likely to use informal finance. However, such studies do not explicitly observe the presence of credit constraints.

In this regard, more direct evidence of the correlation between credit constraints and the use of informal finance is provided by Allen, Chakrabarti, De, Qian, and Qian (2012), who show that Indian firms use informal finance in response to "limited access to institutional finance". Using data on a broad set of firms from 48 countries in the World Business Environment Survey (WBES), Beck, Demirgüç-Kunt, and Maksimovic (2008) further find that there is a correlation between financing obstacles and the use of finance from informal lenders.[6] Such correlations, however, do not allow a causal interpretation.

The following empirical analysis amends this literature by using an IV approach to estimate the causal effect of unsuccessful bank credit negotiations on the use of F&F finance. The comparison of OLS and IV estimators provides evidence of whether F&F finance is

[5] For analyses of start-up businesses, see, for example, Chavis, Klapper, and Love (2011), Sanyal and Mann (2010), Astebro and Bernhardt (2003), Basu and Parker (2001), Romano, Tanewski, and Smyrnios (2001), and Fluck, Holtz-Eakin, and Rosen (1998). For analyses of small firms, see, for example, Denis and Mihov (2003), Berger and Udell (2002), and Bitler, Robb, and Wolken (2001).

[6] Beck, Demirgüç-Kunt, and Maksimovic (2008) define informal finance as financing coming from "informal moneylenders and other traditional sources". They categorise F&F finance as internal finance and do not provide further evidence of its use.

a positive or a negative signal of a firm's creditworthiness in credit negotiations with banks. A particular contribution stems from the use of data on firms of all size and age groups in a highly developed economy without a focus on start-up businesses.

Since the use of F&F finance requires both demand and supply of this kind of funding, the following analysis can also be seen as a test of the theoretical predictions by Giannetti and Yu (2014). They develop a model to investigate lending and borrowing between connected sources, such as F&F finance. Their model predicts that "financiers allocate capital on the basis of prior connections, instead of collecting information on the [...] entrepreneur", depending on the initial capital, transparency, and the quality of investment opportunities in an economy. They show that, even in advanced economies (i.e. those economies with a high level of initial capital), high costs of information acquisition and low average quality of potential borrowers can prompt financiers to "forfeit information acquisition" and lend to "connected entrepreneurs" only.

This prediction is applicable to Germany during the financial crisis of 2007-09, which is captured by the sample used in this analysis. When the German economy was hit by the slowdown of global economic activity, many firms faced a sharp drop in demand for their products and uncertainty about firms' creditworthiness increased (International Monetary Fund, 2009). In the context of the model by Giannetti and Yu (2014) this can be interpreted as a deterioration of borrower quality and increasing information acquisition costs. The model predicts that depositors in Germany preferred to lend to "connected entrepreneurs" rather than acquiring information on potential borrowers. This is in line with data in this study providing evidence of a widespread supply of F&F finance in the German economy.

The remainder of the chapter is structured as follows. In Section 2.2, two hypotheses are derived from existing literature. Section 2.3 provides a description of the data set as well as descriptive statistics on the use of F&F finance and unsuccessful bank credit negotiations. Section 2.4 provides estimation results for OLS and IV approaches. Section 2.6 addresses sample selection issues and the role of discouraged borrowers. Finally, Section 2.7 summarises the findings.

2.2 Hypotheses

Existing empirical literature shows that firms' use of informal finance is correlated with credit constraints (e.g., Allen, Chakrabarti, De, Qian, and Qian (2012), Beck, Demirgüç-Kunt, and Maksimovic (2008)). This is in line with the pecking order theory predicting that firms first borrow from closely connected sources before moving towards arm's length finance. It also raises the question whether credit constraints actually drive firms into F&F finance or if the correlation can be explained by the signalling effect of F&F finance on a firm's probability of receiving bank credit. In particular, it is worth testing the significance of such a causal effect for established firms in a highly developed economy.

Hypothesis 1: Unsuccessful bank credit negotiations cause firms to use of F&F finance, even established firms in a highly developed country.

Resolving endogeneity issues raises the follow-up question whether the use of informal finance facilitates access to credit or if it makes it more difficult. A strand of literature provides evidence of the signalling effect of trade credit on access to bank credit because of trade partners' ability to assess a firm's creditworthiness (e.g., Giannetti, Burkart, and Ellingsen (2011), Engemann, Eck, and Schnitzer (2011)). In a similar manner, such a signalling effect could exist for F&F finance due to the information that connected firms and individuals gather about a firm. Banks could take the fact that firms receive funding from closely related sources as a positive signal of creditworthiness. This may be particularly important when asymmetric information is high, for example, because firms are very young, face high entrepreneurial risk, or do not have a track record of repaid debt.

Hypothesis 2: Using F&F finance is a positive signal of a firm's creditworthiness.

Alternatively, a firm's use of F&F finance could prompt a bank to abstain from granting credit for two reasons. First, the use of F&F finance could be a result of previous unsuccessful credit negotiations, possibly with other financial institutions. Second, from a bank's point of view, the use of F&F finance might carry a whiff of opaqueness, which could threaten the success of bank credit negotiations.

2.3 Data

2.3.1 The Data Set

The following analysis is based on data concerning firms' use of F&F finance, their recent bank credit negotiations, and numerous firm characteristics from the Ifo "Financing of the German Economy" survey, which is described in Chapter 1. The age structure of the firms in the sample is skewed towards older firms (see descriptive statistics in Section 2.3.4), which makes the data set particularly suited to analyse the use of F&F finance in a broad set of firms without a focus on start-up businesses.

Because the survey data contains only a small amount of information on firms' financial situations, it is complemented with 2011 firm balance sheet data from the Bureau van Dyk (BvD) Amadeus database and 2011 credit ratings from Creditreform, a German rating agency. A description of all variables is provided in Table 2.1.

2.3.2 The Use of F&F Finance in Germany

In the following, firms are divided into those that use F&F finance (*F&F firms*) and those that do not (*non-F&F firms*). Considering that Germany is a highly developed economy and that the sample does not focus on start-up businesses, the fraction of F&F firms in the sample is surprisingly high at 15.46 percent. In comparison, capital market finance is used by 3.6 percent and factoring by 10 percent of the firms.

The data distinguishes two kinds of F&F finance. "F&F Business" is defined as a firm receiving capital from a firm or person close to the business (e.g., customers, suppliers). "F&F Private" indicates that capital is received from a person privately connected to the entrepreneur. "F&F Business" is used by 5.79 percent of the firms in the sample. "F&F Private" is about twice as important with 11.81 percent of the firms using it. Analysing both kinds of F&F finance separately provides additional insights as they potentially differ in their degree of informality. While "F&F Business" can be expected to be widely based on formal contracts, informal procedures are more likely for "F&F Private".

Table 2.1: Variable descriptions

Variable	Description
"Family and Friends" finance	
F&F	Firm receives capital from "Family and Friends"
F&F Business	Firm receives capital from F&F close to the business (e.g., customers, suppliers)
F&F Private	Firm receives capital from F&F privately connected to the entrepreneur
Bank Credit Negotiations	
Rejected (line)	Last negotiated line of credit (since 2008) was rejected or only partially granted
Rejected (loan)	Last negotiated loan (since 2008) was rejected or only partially granted
Discretion	Loan officer had a larger impact on the credit granting decision than the bank-internal credit rating
Firm size and age	
Empl	Number of employees
Assets	Total assets
Age	Firm age in years, based on year of foundation
Transparency	
Incorporated	Firm is a corporation by its legal status
Ext. rating	Firm has external rating besides banks' internal ones
Customer	Share of business that is conducted with the three most important customers
Export	Firm is exporting
Ownership	
Group	Firm is part of a group company
Family	Largest shareholder is a single person or family
Control	Percentage share held by largest shareholder
Operating	Largest shareholder active in operative management
Financial condition	
Rating	Score between 100 (sound) to 600 (risky)
Equity	Equity/Assets
Long-term debt	Long-term debt/Assets
Cash	Cash/Assets
Return	EBIT/Turnover
Earlypay	Cash discounts drawn/Cash discounts offered to firm

Sources: Ifo "Financing of the German Economy" survey, BvD Amadeus database, and Creditreform.

2.3.3 F&F Finance and Unsuccessful Bank Credit Negotiations

Data on the use of F&F finance can be linked to a firm's most recent bank credit negotiations. In total, 496 firms negotiated a line of credit between 2008 and 2011 and 510 firms negotiated a bank loan.[7] For this analysis, negotiations are defined as unsuccessful if credit is not granted or granted only at a smaller volume than demanded by the firm. It is important to note that this encompasses rejections of a credit application by the bank, as well as situations in which a firm decides to withdraw the application because of unfavourable terms of credit offered by the bank (e.g., when interest rates are too high). Either scenario can be interpreted as a situation in which a firm faces credit constraints.

Table 2.2: F&F finance and credit negotiations

	Rejected (line)			*Rejected (loan)*		
	Yes	No	$p > t$	Yes	No	$p > t$
N	71	425		64	446	
Perc.	14.31%	85.69%		12.55%	87.45%	
F&F	30.00%	18.75%	0.03**	35.48%	15.67%	0.000***
F&F Business	23.53%	14.56%	0.06*	30.65%	13.02%	0.000***
F&F Private	13.64%	6.47%	0.04**	13.56%	4.71%	0.006***

Notes: The table shows the fraction of F&F firms separately among firms that have successfully negotiated bank credit and among those that have negotiated unsuccessfully; p-values are reported for two-group mean comparison t-tests on whether the two groups of firms differ significantly with respect to the probability of using F&F finance; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.2 shows that F&F finance is significantly more widespread among firms that have experienced unsuccessful bank credit negotiations. This holds for both "F&F Business" and "F&F Private". The difference is more pronounced for unsuccessful negotiations of bank loans than for those of lines of credit, which suggests that F&F finance is more important in financing particular investments than for working capital financing. Table B.1 in the Appendix shows that 72.3 percent of the negotiated lines of credit in the sample were used for working capital finance, but only 57.1 percent served the financing of investments. In contrast, loans were primarily used for investments with enhancements

[7] As firms reported negotiations of lines of credit and loans separately in the survey, each firm could report up to two bank credit negotiations between 2008 and 2011.

being the most frequent purpose. Only 15.1 percent of the loans were used for "other" purposes, which includes working capital finance. The analysis of firms' text-based specification of "other" purposes of loans show that only 1.5 percent of the loans were used for working capital finance. Debt restructuring and liquidity protection were the most prominent "other" purposes of loans.

2.3.4 F&F Finance and Firm Characteristics

A key challenge in the following estimations is to hold constant the firm characteristics which drive both the outcome of a firm's credit negotiations and its use of F&F finance. The data provides a broad set of control variables for a firm's creditworthiness, characteristics that are found in the empirical literature to determine a firm's capital structure in general, and predictors of the use of bank debt and informal finance in particular. Table 2.3 shows how F&F firms differ from non-F&F firms in these variables.[8] Altogether, they are significantly smaller and in a worse financial condition. They also differ in ownership variables. Evidence of differences in firm age and transparency, however, is less clear.

Firm size and age

Firm size and age are widely accepted as determinants of a firm's capital structure. For example, small firms rarely use public market finance because they are less capable of dealing with high transaction costs (Titman and Wessels, 1988). In line with existing empirical literature on firm size and informal finance[9], Table 2.3 shows that F&F firms are smaller in terms of the number of employees and total assets.

Firm age is another determinant of a firm's capital structure as older firms are more likely to have a track record of successful business activity and repaid debt. According to Table 2.3, F&F firms are younger than non-F&F firms, which confirms previous studies.[10]

[8]Table B.2 in the Appendix further provides descriptive statistics separately for firms that use "F&F Business" and for those that use "F&F Private".

[9]See, for example, Allen, Chakrabarti, De, Qian, and Qian (2012), Chavis, Klapper, and Love (2011), Sanyal and Mann (2010), Denis and Mihov (2003), Berger and Udell (2002), Bitler, Robb, and Wolken (2001), Romano, Tanewski, and Smyrnios (2001), and Berger and Udell (1998).

[10]See, for example, Allen, Chakrabarti, De, Qian, and Qian (2012), Chavis, Klapper, and Love (2011), Berger and Udell (2002), Romano, Tanewski, and Smyrnios (2001), Fluck, Holtz-Eakin, and Rosen (1998), and Berger and Udell (1998).

Table 2.3: F&F finance and firm characteristics

	F&F=1 (N=169)			*F&F=0* (N=924)			
	$\bar{X}_{FF}$	X_{FF}^{med}	σ_{FF}	$\bar{X}_{noFF}$	X_{noFF}^{med}	σ_{noFF}	$p > t$
log(Empl)	4.61	4.46	1.75	5.02	4.83	1.85	0.008***
log(Assets)	8.80	8.69	2.18	9.23	9.08	2.27	0.04**
log(Age)	3.95	4.17	1.04	4.05	4.36	0.96	0.22
Incorporated	63.69%	1	48.23%	62.01%	1	48.56%	0.68
Ext. rating	25.00%	0	43.43%	21.25%	0	40.93%	0.28
Customer (< 10%)	18.67%	0	39.09%	20.04%	0	40.06%	0.68
Customer (10% to <30%)	40.36%	0	49.21%	45.26%	0	49.80%	0.24
Customer (30% to <50%)	25.30%	0	43.61%	19.16%	0	39.38%	0.07*
Customer (50% +)	15.66%	0	36.45%	15.53%	0	36.24%	0.97
Export	82.63%	1	37.99%	88.46%	1	31.97%	0.04**
Group	26.63%	0	44.33%	39.65%	0	48.94%	0.001***
Family	82.25%	1	38.32%	74.59%	1	43.56%	0.03**
Control	70.36%	75%	26.46%	73.83%	88%	28.95%	0.15
Operating owner	68.64%	1	46.53%	59.50%	1	49.12%	0.03**
Rating	216.79	208	90.38	197.49	186	90.46	0.01**
Equity	22.78%	27.47%	47.84%	37.55%	38.08%	32.45%	0.000***
Debt	33.92%	22.73%	48.83%	22.82%	13.95%	29.15%	0.000***
Cash	9.21%	3.99%	12.81%	11.37%	5.32%	14.37%	0.11
Return (<3%)	58.33%	1	49.46%	43.13%	0	49.55%	0.000***
Return (3 to <7%)	28.21%	0	45.14%	33.18%	0	47.11%	0.22
Return (7 to <10%)	7.69%	0	26.73%	14.69%	0	35.42%	0.02**
Return (10% +)	5.77%	0	23.39%	9.00%	0	28.64%	0.18
Earlypay (0%)	4.29%	0	20.34%	3.01%	0	17.11%	0.39
Earlypay (<25%)	19.63%	0	39.84%	13.73%	0	34.43%	0.05*
Earlypay (25 to <50%)	6.75%	0	25.16%	9.38%	0	29.16%	0.28
Earlypay (50 to <75%)	16.56%	0	37.29%	9.38%	0	29.16%	0.006***
Earlypay (75% +)	52.76%	1	50.08%	64.51%	1	47.88%	0.004***

Notes: The table shows descriptive statistics for firm characteristics separately for F&F firms and non-F&F firms; p-values are reported for t-tests with H_0: $\bar{X}_{FF} = \bar{X}_{noFF}$; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Transparency

Furthermore, transparency affects a firm's capital structure and the use of (informal) funding from personal sources (Berger and Udell, 1998). First, incorporation measures transparency because it determines a firm's publication obligations, which affect access to bank debt and public debt (Cassar, 2004; Ang, 1992) as well as informal finance (Chavis, Klapper, and Love, 2011; Sanyal and Mann, 2010; Bitler, Robb, and Wolken, 2001). Second, having an external rating indicates transparency and affects a firm's capital structure (Croci, Doukas, and Gonenc, 2011; Faulkender and Petersen, 2006; Denis and Mihov, 2003). Additionally, customer concentration is used as a proxy for transparency as firms with widespread customers have a higher interest in transparency. Finally, export business can increase uncertainty and thereby requires a firm to be transparent. The data in Table 2.3 shows that F&F firms do not differ significantly from non-F&F firms in these variables, except that they are significantly less likely to export.

Ownership

Empirical literature provides evidence that a firm's ownership status also affects its capital structure. First, belonging to a group company reduces the need to use external finance as more internal funds are available (Masulis, Pham, and Zein, 2011). Second, family businesses and non-family businesses differ in their capital structures.[11] Furthermore, capital structure decisions can be affected by the owner's control rights (Croci, Doukas, and Gonenc, 2011; Stulz, 1988) and by whether the largest shareholder of the firm is a member of the operating board (Chavis, Klapper, and Love, 2011).

As Table 2.3 shows, F&F firms are significantly less likely to belong to a group company. They are more likely to be family-owned and the largest shareholder is more likely to be part of a firm's operating board. The share of the largest shareholder is somewhat smaller among F&F firms, but the difference is not statistically significant.

[11] See, for example, Chua, Chrisman, Kellermanns, and Wu (2011), Croci, Doukas, and Gonenc (2011), Ellul (2010), Romano, Tanewski, and Smyrnios (2001), and Coleman and Carsky (1999).

Financial condition

Finally, a set of variables measured in 2011 is used to assess the impact of a firm's financial situation on its capital structure (Denis and Mihov, 2003; Lemmon and Zender, 2010). According to Table 2.3, F&F firms are more risky in terms of the Creditreform rating, have a lower equity ratio, more long-term debt, and less cash. Variables for a firm's return on sales and the fraction of early payment discounts that the firm was able to draw underline that F&F firms are in a worse financial condition than non-F&F firms.

2.4 Methodology and Results

2.4.1 Methodology

Ordinary least squares (OLS) estimations

In a first step, OLS estimations are applied to rule out that the correlation of unsuccessful bank credit negotiations and F&F finance is driven by firm heterogeneity. Although the dependent variables in the following estimations are binary, a linear probability model (LPM) is applied instead of a non-linear estimation to make the results comparable to the linear IV estimations below. Estimations are therefore based on the following linear model:

$$F\&F_i = 1 = \beta_0 + \beta_1 Rejected_i + \beta_2 X_i + \epsilon_i \tag{2.1}$$

where X_i contains control variables for firm characteristics listed in Table 2.3 and industry dummy variables based on the two-digit WZ 2008 industry classification. The latter rule out potential industry effects on the firms' capital structure and the use of F&F finance (Bitler, Robb, and Wolken, 2001; Romano, Tanewski, and Smyrnios, 2001). $Rejected_i$ stands either for *Rejected (line)* or *Rejected (loan)*.

The instrumental variable: Discretionary lending

OLS estimators of the effect of unsuccessful bank credit negotiations on the use of F&F finance do not allow a causal interpretation. As discussed in Section 2.2, receiving capital from a connected source could serve as a positive (or negative) signal of a firm's creditworthiness and increase (or decrease) its chances of receiving bank credit. In both cases, OLS estimations would be affected by endogeneity.

Identification of a causal effect can be achieved by applying an IV approach, which requires an instrumental variable that satisfies three conditions. First, it must be a relevant predictor of the outcome of bank credit negotiations. Second, it must not affect a firm's decision to use F&F finance, except through the outcome of bank credit negotiations. Finally, the instrumental variable has to be independent of the use of F&F finance conditional on the control variables.

In the following, a variable indicating that a bank applies discretionary lending in the decision about granting credit provides an instrumental variable that satisfies all three conditions. In practice, banks can decide about credit applications based on a rules-based and pre-codified lending process or leave the credit granting decision to the discretion of the loan officer. In the Ifo "Financing of the German Economy" survey, firms that have negotiated bank credit report whether the loan officer had a larger impact on the credit granting decision than a firm's credit rating. The variable $Discretion_i$ equals one if this was the case and therefore indicates that the bank applied discretionary lending.

There are two arguments why discretionary lending is a relevant predictor of bank credit negotiation outcomes in line with the first condition of an instrumental variable. First, in rules-based lending, banks decide whether to lend based on credit scores that are derived only from hard information about the firm (e.g., balance sheet information). If discretionary lending is applied, soft information is also considered in the credit granting decision. Therefore, a broader range of information is collected and taken into account, which lowers uncertainty for the bank and improves its ability to price the credit contract. This increases the probability that the bank grants credit given a firm's creditworthiness. Second, discretionary lending provides more room for credit decisions to be based on the flow of information from the personal relationship between the loan officer and the customer. This increases the probability of a firm receiving credit, in particular in times of constrained credit supply (e.g., Sharpe (1990); von Thadden (2004)). How discretionary lending predicts a bank’s credit granting decisions is further discussed by Puri, Rocholl, and Steffen (2011b). Their empirical analysis based on a large data set of loans to retail customers shows that discretion in the loan approval decision increases the number of customers that receive credit, which further supports the relevance of discretionary lending as an instrumental variable in the following estimations.

The second condition is also satisfied by discretionary lending as the decision process is deeply rooted in the organisational structure and operational culture of the bank. Therefore, the presence of discretionary lending in a bank affects a firm’s financing decisions (e.g., about F&F finance) only through the result of bank credit negotiations.

Whether discretionary lending is independent of F&F finance conditional on the control variables remains the key assumption in the following IV estimations. Gropp, Gruendl, and Guettler (2013) show that certain firms select themselves into borrowing from discretionary lenders, for example, depending on their creditworthiness. Such selection behaviour can be ruled out as affecting the IV estimations in this study because firms’ credit risk and their financial situation is controlled for by a large set of control variables. Proving the validity of the instrumental variable beyond factors held constant by control variables, the discussion in Section 2.5 provides further evidence of F&F firms not selecting themselves into negotiations with banks that are likely to apply discretionary lending.

IV estimations: Avoiding the “forbidden” regression

Endogeneity enters the basic model in Equation 2.1 through $Rejected_i$, which is a binary variable. This raises concerns about the “forbidden regression” in which predicted values from a non-linear first-stage estimation are used in an IV approach. Angrist and Pischke (2008), and Wooldridge (2010) suggest the following estimation approach to make use of potential non-linearity in the conditional expectations function.

The first step contains a binary probit estimation of the model

$$Pr(Rejected_i = 1) = \Phi(\alpha_0 + \alpha_1 X_i + \alpha_2 Discretion_i) \quad (2.2)$$

where $\Phi(\cdot)$ is the cumulative distribution function of a standard normal distribution. X_i contains all control variables and industry dummy variables, and $Discretion_i$ is the instrumental variable. Two separate estimations are run for *Rejected (line)* and *Rejected (loan)*.[12] Predicted values for the probability of unsuccessful bank credit negotiations are calculated from both estimations. Following Angrist and Pischke (2008), and Wooldridge (2010), these predicted values are used as instrumental variables for *Rejected (line)* and *Rejected (loan)*, respectively, in two-stage least squares estimations.

2.4.2 Results: Ordinary Least Squares Estimations

According to OLS estimations in Table 2.4, the use of F&F finance is not significantly affected by unsuccessful negotiations of a line of credit. The effect of unsuccessful negotiations of a loan, however, is substantially larger and statistically significant. Therefore, F&F finance is more important to firms that need to finance particular investments than to those that need working capital finance.

Table 2.4 further suggests that the effect is driven by "F&F Private", which is a more important substitute for unsuccessfully negotiated bank credit than "F&F Business". In Estimations (3) and (4), unsuccessful bank credit negotiations do not have a significant effect on "F&F Business". Furthermore, unsuccessful negotiations of lines of credit do not affect the probability that a firm uses "F&F Private" in Estimation (5). Estimation (6), however, confirms that unsuccessful negotiations of a bank loan are associated with a significantly higher probability of using "F&F Private".

[12] The results for the two estimations are presented in Table B.3 in the Appendix. The coefficients of the variable $Discretion_i$ are large and highly statistically significant in both estimations, which underlines the relevance of the instrumental variable in predicting the outcome of bank credit negotiations.

Table 2.4: OLS estimations

	(1) *F&F*	(2) *F&F*	(3) *F&F Business*	(4) *F&F Business*	(5) *F&F Private*	(6) *F&F Private*
Rejected (line)	0.053		0.009		0.072	
	(0.09)		(0.07)		(0.08)	
Rejected (loan)		0.178*		0.097		0.160*
		(0.09)		(0.06)		(0.08)
Firm char.	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.1480	0.1116	0.1360	0.1306	0.1497	0.1354
N	289	308	279	303	284	304

Notes: The table shows results for six separate OLS estimations; firm characteristics comprise all control variables listed in Table 2.3; industry dummy variables are included based on the two-digit WZ 2008 industry classification; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.4.3 Results: Instrumental Variables Estimations

Table 2.5 provides results for IV estimations ruling out endogeneity. Estimation (1) shows that unsuccessful negotiations of a line of credit do not cause firms to use F&F finance. According to Estimation (2), however, firms use F&F finance as a substitute for unsuccessfully negotiated bank loans. Estimations (4) and (6) underline that this substitution effect is driven by both kinds of F&F finance. Therefore, Hypothesis 1 is confirmed for bank loans, but not for lines of credit. This underlines that F&F finance is used by firms as an alternative source of funding for particular investments, but not for working capital finance.

The summary statistics of the first-stage regression in Table 2.5 show that the F-statistic is above ten for all estimations.[13] The test criteria suggested by Stock and Yogo (2005) are satisfied at the ten percent level in all estimations. Therefore, discretionary lending provides a sufficiently strong instrument for unsuccessful bank credit negotiations. Comparing the different estimations, it seems to be a stronger instrument for unsuccessful negotiations of a line of credit than for unsuccessful negotiations of a loan.

[13] A critical value of ten is suggested as a rule of thumb by Staiger and Stock (1997).

Table 2.5: IV estimations

	(1) *F&F*	(2) *F&F*	(3) *F&F Business*	(4) *F&F Business*	(5) *F&F Private*	(6) *F&F Private*
Rejected (line)	0.219		0.137		0.041	
	(0.21)		(0.16)		(0.17)	
Rejected (loan)		0.634**		0.430**		0.480*
		(0.31)		(0.21)		(0.27)
Firm char.	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
F	33.21***	15.16***	34.55***	12.89***	31.49***	15.91***
MES	35.81***	18.08***	35.94***	17.14***	35.34***	18.49***
N	234	225	225	220	230	222

Note: The table shows results of separate IV estimations; firm characteristics comprise all control variables listed in Table 2.3; industry dummy variables are included based on the two-digit WZ 2008 industry classification; robust standard errors are reported in parentheses; F is the F-statistic for the first-stage estimation testing for weak instruments; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; MES provides the minimum eigenvalue statistic for the Stock and Yogo (2005) test for weak instruments with *** indicating significance at the 10 percent level.

2.4.4 Signalling Effect of F&F Finance

The comparison of the estimated effects from IV estimations to those from OLS estimations sheds light on whether F&F finance is taken by banks as a positive or a negative signal of a firm’s creditworthiness (see Hypothesis 2). If it was a positive signal, its use would be negatively correlated with the probability of unsuccessful credit negotiations, which would lead OLS estimators to underestimate the effect of unsuccessful credit negotiations of F&F finance. IV estimations would therefore lead to higher estimated effects than OLS estimations. If F&F finance was a negative signal, the opposite would be the case and IV estimations would reduce estimated effects relative to OLS estimations.

Compared to OLS estimation, the impact of unsuccessful bank credit negotiations on the use of “F&F Business” increases in both IV estimations, which suggests that “F&F Business” is taken as a positive signal of a firm’s creditworthiness. This result supports

Hypothesis 2. For “F&F Private”, however, the picture is mixed with estimated effects decreasing in IV estimations of the effect of unsuccessfully negotiated lines of credit, but increasing in estimations of the effect of unsuccessfully negotiated loans.

This shows that receiving capital from business-related sources (e.g., suppliers or customers) is a stronger positive signal of creditworthiness than receiving capital from privately connected sources. The latter could even be a negative signal. This is reasonable as business-related lenders might be more motivated and better able to assess a firm’s business activity, and therefore its creditworthiness than persons who are only privately connected to the entrepreneur. In contrast, the latter may have the incentive to provide capital for altruistic reasons even if creditworthiness is low. Furthermore, the degree of informality could be higher for “F&F Private”, which could induce banks to abstain from lending to the firm.

2.5 Discussion of the Instrumental Variable

Using discretionary lending as an instrumental variable for unsuccessful bank credit negotiations is valid if discretionary lending and the use of F&F finance are independent conditional on the control variables included in the estimations. In the following, the data is used to test the satisfaction of this condition by analysing under which circumstances discretionary lending is applied by a bank and whether these are more likely to be prevalent for F&F firms.

First, the data allows to test whether F&F firms select themselves into negotiations with banks that are particularly likely to apply discretionary lending. Whether banks do so depends primarily on their business model. As described by Hackethal (2004), the German banking system comprises several classifications of banks, which differ substantially in their business models. Private commercial banks are typically universal banks offering a wide range of financial products. Savings banks are publicly owned and focus on taking in deposits and providing credit to the economy within their region. Landesbanken serve as central banks to savings banks and provide large scale funding that cannot be offered by small savings banks. Cooperative banks have a business model comparable to the one of savings banks, but they are mutually owned by their members, whom they serve. Finally, the data contains a category of "Other banks" that comprises banks that do not fall into either of these categories (e.g., foreign banks).

Table 2.6: Determinants of Discretionary Lending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Commercial	0.0022						-0.0559
	(0.04)						(-0.76)
Savings bank	0.0727						0.0177
	(1.15)						(0.21)
Landesbank	0.1643*						0.1460
	(1.84)						(1.43)
Cooperative	0.0684						0.0074
	(0.96)						(0.08)
log(Length)		-0.0204					-0.0300
		(-1.06)					(-1.42)
log(Distance)			-0.0158				-0.0132
			(-0.80)				(-0.55)
Credit (%)				0.0014***			0.0010*
				(2.62)			(1.72)
Meeting					0.0864		0.0811
					(1.35)		(1.23)
Vol. (0.1 to < 0.25)						0.0955	0.0863
						(1.44)	(1.18)
Vol. (1 to < 5)						0.0084	0.0256
						(0.14)	(0.37)
Vol. (5 to < 50)						-0.0176	0.0836
						(-0.25)	(1.04)
Vol. (50+)						-0.0472	0.0155
						(-0.57)	(0.15)
N	1,125	1,099	1,110	1,073	1,114	1,340	982

Notes: The table shows results for OLS estimations in a sample of all credit negotiations of all firms since 2008; the dependent variable in all estimations is the dummy variable indicating that the bank decided about granting credit based on discretionary lending; the baseline category for the bank classification dummy variables is "Other banks"; relationship length is measured in years; distance is measured in travel minutes from the headquarters of the firm to the bank; the variable credit provides the share of credit a firm has with the bank; the dummy variable meeting indicates that the firm meets with the bank in person on a monthly basis rather than less frequently; "Vol." denotes the volume of credit measured in million EUR with "Vol. (< 100)" being the baseline category; standard errors are clustered at the firm-level and reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To figure out whether different classifications of banks are differently inclined to apply discretionary lending, the firm-level data from the Ifo “Financing of the German Economy” survey can be transformed into a data set of the last bank credit negotiations of all firms. Each firm reports the last negotiations of a line of credit and the last negotiations of a bank loan so that up to two observations per firm are available. For every negotiation, firms could report with which classification of banks these negotiations were held. This information can be linked to whether the credit granting decision was made based on discretionary lending.

The application of discretionary lending may also be driven by certain firm-bank relationship characteristics, which are provided in the data. First, the relationship length and the geographical distance (measured by the travel distance in minutes) could drive discretionary lending because a long relationship and proximity might lead to close personal ties. Other factors could be the fraction of credit that the firm proceeds through the bank and whether a firm’s banker is met in person on a monthly basis or less frequently. In addition to these firm-bank relationship characteristics, the impact of negotiated credit volume on the likelihood of discretionary lending can be assessed.

Based on all credit negotiations of all firms, Estimation (1) in Table 2.6 shows that discretionary lending is significantly more likely for Landesbanken with “Other banks” being the baseline category. When including all variables in Estimation (7), the effect turns insignificant. Furthermore, there is no evidence that savings banks and cooperative banks are particularly likely to apply discretionary lending. For private commercial banks, the probability is somewhat lower, but the difference is not statistically significant. Among the firm-bank relationship characteristics, only the fraction of credit proceeded through the bank seems to drive discretionary lending. The effect of monthly meetings is insignificant, but very stable between Estimations (5) and (7).

Therefore, if F&F firms selected themselves into negotiations with discretionary lenders, they should be more likely to negotiate with Landesbanken, banks through which they proceed a large share of credit, and banks with which they meet on a monthly basis. Table 2.7 shows regressions of variables indicating the classification of the bank with which a firm negotiated on whether a firm uses F&F finance. The results show that F&F

Table 2.7: F&F firms, credit negotiations, and bank classifications

	(1) Commercial	(2) Savings bank	(3) Landesbank	(4) Cooperative	(5) Other bank
F&F	-0.056	0.019	0.028	0.071*	-0.062***
	(0.05)	(0.04)	(0.03)	(0.04)	(0.01)
N	1,113	1,113	1,113	1,113	1,113

Notes: The table shows results for OLS estimations in a sample of all credit negotiations of all firms since 2008; standard errors are clustered at the firm-level and reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

firms are not more likely to negotiate with Landesbanken. They tend to select themselves away from negotiations with "Other Banks" and towards cooperative banks, but these do not differ in their attitude towards discretionary lending.

Furthermore, Table 2.8 shows that the firm-bank relationships in which F&F firms negotiate bank credit are not characterised by a particularly high share of credit held with the bank or a high likelihood of monthly meetings. Therefore, the circumstances under which discretionary lending is applied are not significantly more or less likely to be in place for F&F firms. Hence, neither bank classifications nor firm-bank relationship characteristics show any signs of F&F firms selecting themselves into negotiations with discretionary lenders, which strengthens the view that discretionary lending provides a valid instrumental variable for unsuccessful bank credit negotiations.

Table 2.8: F&F firms, credit negotiations, and bank-firm relationships

	(1) Credit (%)	(2) Meeting
F&F	-0.281	-0.004
	(3.22)	(0.03)
N	1,061	1,097

Notes: The table shows results for OLS estimations in a sample of all credit negotiations of all firms since 2008; standard errors are clustered at the firm-level and reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.6 Robustness Checks

2.6.1 Controlling for Sample Selection Bias

So far, all estimations have been conducted in the sample of firms that have negotiated a line of credit or a loan, respectively, which raises concerns about sample selection bias. This is addressed by applying a two-step estimation procedure suggested by Heckman (1979). From estimations of a probit model for firms' decisions to enter bank credit negotiations, the inverse Mills ratio can be calculated for every observation. The ratio is then included as a control variable in the OLS and IV estimations analogue to Section 2.4. In a comparable setup, Brown, Ongena, Popov, and Yeşin (2011) apply this procedure to control for selection effects when analysing the determinants of credit demand and credit access of firms in Eastern Europe using data from the Business Environment and Enterprise Survey (BEEPS).

In this empirical analysis, however, the second step of such a selection model contains an IV estimation. To the best of my knowledge, there is no standard procedure to estimate the correct standard errors in such a setup. Unfortunately, bootstrapped standard errors are not available due to the small number of observations in the data set. Therefore, the following results can only be considered as a "back-of-the-envelope" analysis to control for sample selection bias. The selection process is modelled as

$$Pr(Negotiation_i) = \Phi(\beta_0 + \beta_1 X_i + \beta_2 Competition_i) \tag{2.3}$$

where $\Phi(\cdot)$ is the cumulative distribution function of a normal distribution, X_i is the set of control variables and industry dummy variables, and $Competition_i$ is a set of dummy variables measuring the level of competition a firm faces.[14] The latter serve as exclusion restrictions in order to ensure identification of the two-step estimator. Hainz and Nabokin (2013) also use competition as an exclusion restriction when estimating how firms select themselves into having credit demand. They argue that "firms may invest more often in order to improve their position relative to other competitors" when facing high levels

[14] The level of competition on a scale from 1 to 11 is reported by firms in the Ifo "Financing of the German Economy" survey.

of competition and that this increases their probability of having credit demand. They further claim that the exogeneity with respect to the success of bank credit negotiations is ensured because banks assess the level of competition at the industry-level only. As industry dummy variables are included in all estimations in this study, any effects of competition on the outcome of credit negotiations should be ruled out.

The argument of Hainz and Nabokin (2013) is directly applicable to the estimation of a model for firms' selection into negotiations of bank loans, which are used to finance investments. It is also applicable to negotiations of lines of credit because some firms use them to finance investments. When facing high levels of competition, firms may also increase their working capital to improve their competitiveness.

From the probit estimations, the inverse Mills ratio is calculated and used as a control variable in the OLS estimations as described in Section 2.4.1. The results in Table B.4 in the Appendix confirm previous OLS estimation results from Section 2.4.2: Unsuccessful negotiations of a bank loan are associated with a higher probability of using F&F finance. This is primarily driven by the significant effect of unsuccessful loan negotiations on the use of "F&F Private". Even after controlling for selection bias, estimations do not show any signs of F&F finance being used as a substitute for unsuccessfully negotiated lines of credit.

As another robustness check, the inverse Mills ratio from the selection model is included as a control variable into IV estimations in comparison to estimations in Section 2.4.3. The results in Table B.5 in the Appendix confirm that firms use F&F finance in response to unsuccessfully negotiated loans, but not as a substitute for lines of credit. Unsuccessfully negotiated loans increase the probability of both "F&F Business" and "F&F Private" being used by a firm.

In both the OLS and IV estimations, the results suggest that the estimation of the effect of unsuccessful bank credit negotiations on the use of F&F finance are not affected by sample selection. The estimated coefficients hardly change compared to the estimations in which selection bias is not accounted for. Furthermore, the estimated coefficients of the inverse Mills ratio are statistically insignificant in all estimations.

2.6.2 The Role of Discouraged Borrowers

The extent to which F&F finance is used as a substitute for unsuccessfully negotiated bank credit could underestimate the true substitution effect in response to credit constraints if firms turn to F&F finance before even entering bank credit negotiations. They could do so, for example, because they expect a rejection of their credit application. Such firms are generally referred to as *discouraged* borrowers.

Whether discouraged borrowers provide further evidence in support of the result that credit constraints drive firms into informal finance can be tested with data from the Ifo “Financing of the German Economy” survey. If firms did not negotiate about bank credit, they could report in the survey whether they did not do so because they expected negotiations with banks to be unsuccessful.

OLS estimations are used to test the significance of the effect of a firm being discouraged on the use of F&F finance. The results in Table B.6 in the Appendix suggest that no further substitution behaviour into informal finance is prevalent because of expected unsuccessful bank credit negotiations. IV estimations that would allow a causal interpretation are not available here because the data does not contain any suitable instrumental variables for a firm being discouraged.

2.7 Conclusion

Based on a novel data set from the Ifo “Financing of the German Economy” survey, this chapter shows that F&F finance is surprisingly widespread among German firms. Considering that Germany is a highly developed country and that the sample does not focus on start-up businesses, this deserves special attention. In particular, it is important to analyse whether bank credit constraints are a cause of the widespread use of this particular kind of informal finance.

Descriptive statistics show that firms that unsuccessfully negotiate about bank credit are significantly more likely to use F&F finance. OLS estimations are used to control for a broad set of firm characteristics that previous literature found to determine a firm’s capital structure, its decision to use informal finance, and its risk of facing bank credit constraints. The results suggest that F&F finance is significantly more likely to be used if a firm has unsuccessfully negotiated a bank loan. Such an effect is not found for unsuccessful negotiations of lines of credit.

Since OLS estimations are affected by endogeneity stemming from the fact that F&F finance may have a signalling effect on banks' decisions to grant or reject credit, an IV approach is applied to estimate the causal effect of unsuccessful bank credit negotiations. Thereby, the dummy variable indicating that a bank follows discretionary lending provides an instrumental variable for unsuccessful bank credit negotiations.

IV estimations show that unsuccessfully negotiated bank loans drive the use of F&F finance, which highlights its importance in financing specific investment projects. For working capital finance, however, F&F finance seems less important as there is no significant effect of unsuccessfully negotiated lines of credit on the use of F&F finance. The results show that the substitution effect exists for both "F&F Business" and "F&F Private". The comparison of OLS and IV estimators further indicate that "F&F Business" serves as a positive signal of a firm's creditworthiness in bank credit negotiations while evidence of the signalling effect of "F&F Private" is mixed. Robustness checks show that these findings are unaffected by accounting for sample selection bias. Furthermore, there is no evidence that discouraged borrowers use F&F finance in response to expectations of unsuccessful bank credit negotiations.

Since turning to informal sources of finance is associated with higher borrowing costs (Djankov, Lieberman, Mukherjee, and Nenova, 2003) and welfare gains from financial intermediation no longer materialise, attention should be paid to the extent to which firms use informal finance in response to credit constraints. This study provides evidence that this phenomenon is not only occurring in developing countries or among start-up businesses, but that it is highly relevant even among established firms in a highly developed economy.

Chapter 3

Are Real Effects of Credit Supply Overestimated? Bias from Firms' Current Situation and Future Expectations*

3.1 Introduction

Designing policy measures in response to credit constraints at the firm-level requires an understanding of whether they are caused by credit supply-side factors (e.g., a bank liquidity shock) or firm-side factors (e.g., a shock to the firms' creditworthiness). During the financial crisis of 2007-09, for example, banks in many countries faced a severe liquidity shock and reduced their lending to non-financial firms. Lacking access to credit, firms postponed investment and reduced their business activity. Therefore, credit supply-side factors caused a slowdown in real economic activity.[1] In other countries, however, banks weathered the financial crisis quite well, yet non-financial firms nevertheless experienced credit constraints as a reflection of their deteriorating creditworthiness when the world-

*This chapter is based on joint work with Michael Kleemann.

[1] This view has been supported by Brunnermeier (2009) and Shleifer and Vishny (2010), whereas Kahle and Stulz (2013) challenge this "bank credit channel" story.

wide economic slowdown hit demand for their products.[2] According to this narrative, credit constraints and the economic slowdown were not caused only by credit supply-side, but also by firm-side factors.

To achieve an unbiased estimation of real effects of credit supply-side factors, these must be disentangled from firm-side factors. For this purpose, existing empirical studies primarily use control variables from firms' balance sheets. These mirror past business, but fail to capture a firm's current business situation and future expectations, which induce estimation bias by affecting both credit supply and real economic activity. Market-based variables can be used to address this issue, but these are available for listed firms only and may not perfectly capture all contemporaneous and forward-looking firm-side factors.

We address the question of whether not controlling for firms' current business situation and future expectations, in addition to balance sheet data, leads to biased estimations of the real effects of credit supply. The analysis is based on data from the "EBDC Business Expectations Panel" for Germany between 2003 and 2011, which combines firms' balance sheets with survey-based assessments of their current business situation and future expectations.[3] The data set provides a time-specific treatment variable indicating the experience of constrained credit-supply at the firm-level based on monthly panel data on firms' perceptions of bank lending supply. Applying ordinary least squares (OLS) and propensity score matching (PSM) estimation approaches, we estimate the treatment effect of constrained credit supply on monthly changes in a firm's production.

We find that the sole reliance on balance sheet data to rule out firm-side factors leads to an overestimation of credit supply-side effects on real economic activity. When controlling for balance sheet variables only, OLS and PSM estimations suggest that credit supply-side factors cause a significant increase in the probability of slowed down firm-level production during the post-treatment year. However, when including control variables for firms' current business situation and future expectations, the OLS estimator is significantly reduced. In PSM estimations, the estimated treatment effect even turns insignificant,

[2]The International Monetary Fund (2009) describes the economic crisis in Germany following this pattern.

[3]German data is particularly suited to analyse the real effects of credit constraints because Germany is a bank-based economy where many firms do not have access to public market finance as a substitute for bank credit and may therefore be particularly affected by credit constraints.

which raises doubts about the importance of credit supply-side shocks in Germany during the financial crisis. These findings are further supported by estimations using annual firm-level employment growth as an alternative outcome variable. From our results, we draw the conclusion that the empirical literature analysing the real effects of credit supply should develop sufficient approaches to rule out firm heterogeneity that is not captured by balance sheets.

This analysis contributes to a strand of research on the estimation of the real effects of credit supply[4] at the firm-level.[5] In particular, we focus on the question of how to rule out firm-side factors in such estimations.[6] Therefore, the control variables used in previous empirical studies are of particular interest here.

First, several studies use a combination of balance sheet and market data to rule out firm-side factors when estimating the real effects of credit supply-side shocks (e.g., on firms' investments).[7] They all control for different sets of balance sheet variables such as firm size, cash flow, cash holdings, leverage, and profitability. Since balance sheets are backward-looking only, all these studies additionally control for at least one market-based variable (e.g., market-to-book value), assuming that market prices contain all relevant contemporaneous and forward-looking information about a firm.[8]

Market data to control for contemporaneous and forward-looking firm-side factors, however, is only available when firms are publicly traded. Using a sample of both public and non-public firms, Chodorow-Reich (2014) therefore does not use market data, but

[4]The link between credit supply and real economic activity was first shown by Bernanke (1983). Theoretical models including such a bank lending channel were developed by Bernanke and Blinder (1988), Bernanke and Gertler (1989), Kashyap and Stein (1994), Bernanke and Gertler (1995), Bernanke, Gertler, and Gilchrist (1996), Holmstrom and Tirole (1997), Kiyotaki and Moore (1997), and Gertler and Kiyotaki (2010).

[5]In addition to firm-level studies, Kroszner, Laeven, and Klingebiel (2007), Dell'Ariccia, Detragiache, and Rajan (2008), and Duygan-Bump, Levkov, and Montoriol-Garriga (2011) analyse the real effects of credit supply at the sector-level following Rajan and Zingales (1998) by using external finance dependence of a sector as a measure for financial constraints. Peek and Rosengren (2000) estimate the impact of the Japanese Banking crisis on the U.S. economy at the macroeconomic level.

[6]Therefore, the paper is not comparable to the analysis by Farre-Mensa and Ljungqvist (2013), who deal with the question of how to measure credit constraints.

[7]Gan (2007), Duchin, Ozbas, and Sensoy (2010), Almeida, Campello, Laranjeira, and Weisbenner (2011), Chava and Purnanandam (2011), and Lin and Paravisini (2012) do so by using financial crises as credit supply-side shocks, whereas Amiti and Weinstein (2013) identify bank shocks directly.

[8]Market data is also used to estimate the impact of bank health on firms' market values, for example by Yamori and Murakami (1999), Kang and Stulz (2000), Bae, Kang, and Lim (2002), and Ongena, Smith, and Michalsen (2003).

controls for firms' borrowing patterns, size, age, access to public bond markets, and a set of variables from the Dealscan database when estimating the effect of bank health on U.S. firm-level employment. In a robustness test, he also applies a within-firm approach as used by Khwaja and Mian (2008), which does not require controlling for a certain set of firm characteristics.

To the best of our knowledge, the study by Campello, Graham, and Harvey (2010) is the only one that includes control variables explicitly measuring firms' current business situation and future expectations in the estimation of real effects of credit supply. Similar to our analysis, they use survey data to show that self-reported financial constraints adversely affect several firm-level outcome variables (e.g., planned employment cuts). At first, they only control for firm size, ownership, industry, and rating category to rule out firm heterogeneity. Their estimated real effects of financial constraints turn out slightly smaller, but still significant, when they account for contemporaneous and forward-looking firm-side factors. They do so by including control variables for profitability, growth prospects, and dividend payer status. The last variable, however, is also a market-based variable that is only available for publicly traded firms.

We contribute to the literature on real effects of credit supply by controlling for a large set of explicit survey-based measures of firms' current business situation and future expectations without the need for market data. We test whether the sole reliance on balance sheet data leads to an overestimation of the real effects of credit supply. Thereby, we amend the approach of Campello, Graham, and Harvey (2010) by using panel data instead of cross-sectional data. Furthermore, the use of monthly survey data allows us to identify a direct and time-specific treatment variable for constrained credit supply and estimate its effect on monthly changes in firm-level production, in contrast to analyses based on annual data and more indirect measures of financial constraints. Finally, we estimate credit supply-side effects on annual employment growth, which is often not possible due to a lack of firm-level employment data.[9]

[9] A notable exception is the study by Chodorow-Reich (2014), who uses firm-level employment data. Campello, Graham, and Harvey (2010) estimate the effects of financial constraints on planned employment cuts without observing whether these plans are implemented. Duygan-Bump, Levkov, and Montoriol-Garriga (2011) estimate employment effects of financing constraints at the sector-level by using workers' employment status from the U.S. Current Population Survey (CPS).

Our results are also of interest for the estimation of credit supply-side effects on loan outcomes (without estimating real effects), in which firm-side factors must be held constant as well. For example, in their analysis of bank-side effects on loan outcomes using Spanish data, Jimènez, Ongena, Peydro, and Saurina (2012) control for firm size, leverage, and liquidity in addition to firm age and credit history. Analysing bank-side effects on credit rejections in Eastern Europe, Popov and Udell (2012) control for firm size, age, ownership, export status, and external auditing. Santos (2011) also includes market-based firm characteristics as control variables in his analysis of loan pricing during the recent subprime crisis. Explicit measures of firms' current situation and future expectations could amend analyses along this line of research.

The chapter is structured as follows. Section 3.2 derives the testable hypothesis. Section 3.3 describes the data set, the treatment definition, and provides descriptive statistics for all outcome and control variables. Section 3.4.1 explains the empirical strategy. Results for OLS and PSM estimation are presented in Sections 3.4.2 and 3.4.3. Section 3.5 contains several robustness checks. Section 3.6 discusses the external validity of the results and Section 3.7 provides concluding remarks.

3.2 Hypothesis

If the treatment of constrained credit supply was randomly assigned to firms, the observed difference in real economic activity between restricted and unrestricted firms could be interpreted as being caused by credit supply-side factors. Theoretical literature provides two explanations for why credit constraints could be randomly assigned to firms. According to Stiglitz and Weiss (1981), one symptom of credit rationing is the possibility that one firm is granted bank credit while the credit application of an identical other firm is rejected. Therefore, credit constraints would be considered to be randomly assigned. Furthermore, the model by Sharpe (1990) explains differences in credit supply between otherwise similar firms by heterogeneity in firms' bank relationships. Empirical evidence lends support to the argument that the health of relationship banks has an impact on firms (e.g., Almeida, Campello, Laranjeira, and Weisbenner (2011), Santos (2011), and

Chodorow-Reich (2014)). This is further supported by studies showing that banks with different characteristics, such as size and capitalisation, are differently inclined to transmit monetary policy changes to the real sector[10] and that they differed in their lending behaviour during the financial crisis of 2007-09.[11]

The empirical identification of the real effects of credit supply, however, is complicated by the fact that a bank's credit granting decision is typically not random, but depends on firm-side factors determining a firm's creditworthiness. If, for example, a firm experiences a product demand shock and its creditworthiness deteriorates, it is more likely to experience constrained credit supply. It is also more likely to reduce its production, but it does so in response to the product demand shock, not due to credit supply-side factors. The observed differences between restricted and unrestricted firms would overstate the real effects of credit supply-side factors. Alternatively, the observed difference could understate credit supply-side effects if firms with strong growth potentials share certain features that make them prone to face constrained credit supply. This could, for example, be the case for start-up businesses that are young, small, and risky.

Therefore, controlling for firm heterogeneity is crucial to avoid biased estimators of credit supply-side effects on real economic activity. For this purpose, control variables from balance sheets provide accurate measures of heterogeneity in firms' financial conditions. Balance sheets, however, mirror only past business and fail to capture firms' current business situation and future expectations. The latter predict a firm's future business activity and when deciding about granting credit, banks consider them (e.g., by looking at order books, interim financial statements, or business plans). Consequently, such contemporaneous and forward-looking firm characteristics, which is not captured by balance sheets, could induce estimation bias.

Hypothesis: Not explicitly controlling for firms' current business situation and future expectations, in addition to balance sheet variables, leads to biased estimations of the effects of credit supply-side factors on real economic activity.

[10]See, for example, Kashyap and Stein (2000), Kishan and Opiela (2000), Gambacorta (2005), Kishan and Opiela (2006), and Jimènez, Ongena, Peydro, and Saurina (2012).

[11]See, for example, Albertazzi and Marchetti (2010), Ivashina and Scharfstein (2010), and Puri, Rocholl, and Steffen (2011a).

3.3 Data

3.3.1 The Data Set

For our analysis, we use data from the German "EBDC Business Expectations Panel". The data set links firms' balance sheets from the Bureau van Dyk (BvD) Amadeus database[12] and the Hoppenstedt database[13] to panel data from the Ifo Business Survey.[14] The latter is a monthly survey asking 3,600 plants from the German manufacturing sector for appraisals of their current business situation and expectations for their future business. It provides the basis for the Ifo Business Climate Index, an indicator of economic activity.

For the calculation of the index, the Ifo Institute continuously ensures that the panel of firms included in the survey is representative of the German manufacturing sector. A more detailed description of the data set is provided by Becker and Wohlrabe (2008).[15] All variables used in this analysis are described in Table 3.1.

The Ifo Business Survey data is well-suited to test our hypothesis for three reasons. First, firms in the survey report their perception of bank lending supply, from which we can derive a month-specific treatment variable indicating constrained credit supply at the firm-level. Second, firms report recent changes in production on a monthly basis, which provides a precise measure of changes in post-treatment production. Third, and most importantly, the survey data contains a broad set of appraisals of firms' current business situation and future expectations. In combination with balance sheet variables, this provides a unique opportunity to test how controlling for contemporaneous and forward-looking firm characteristics affects the estimation of the real effects of credit supply.

[12] The BvD Amadeus database contains balance sheet data and other firm-specific information about European firms, including about 1 million mainly non-listed German firms. Its primary source for Germany is the Creditreform, a German rating agency.

[13] Hoppenstedt is a leading provider of balance sheet data for German firms. The public press and commercial registries are among its main data sources. It has almost full coverage of publicly available financial statements in Germany.

[14] When linking annual balance sheet and monthly survey data, which is done based on the name and postal address of the firms, the following issue arises. The fiscal years of some firms in the sample do not coincide with calendar years. We use the monthly frequency of the data and put the most recently published balance sheet into every monthly observation. Thereby, we assume that a firm's balance sheet is made available in the credit application process immediately at the end of the fiscal year.

[15] Kipar (2011) uses data from the Ifo Business Survey to test the impact of credit constraints on firm-level innovation activity.

Table 3.1: Variable descriptions

Variable	Description	Freq.
Treatment		
Restricted	Change in perception of bank lending from "accommodating" or "normal" to "restrictive"	Monthly
Variables of interest		
Slowdown	Production decreased at least once in the last 12 months	Monthly
Slowdown_avg	Fraction of last 12 months in which production decreased	Monthly
Δ*Empl*	Year-on-year employment growth rate	Annual
Empl_decrease	Negative Δ*Empl*	Annual
Firm size		
Empl	Number of employees at company-level	Annual
Assets	Total assets	Annual
Balance sheet data		
Equity	Equity / Assets	Annual
Cash	Cash holdings / Assets	Annual
Long-term debt	Long-term debt / Assets	Annual
Short-term debt	Short-term debt / Assets	Annual
Cash flow	Cash flow / Assets	Annual
ROA	Operating profit / Assets	Annual
Interest coverage	Operating profit / Interest expenses	Annual
Current business situation		
State	Appraisal: Current business situation (good / satisfactory / bad)	Monthly
Orders	Appraisal: Stock of orders (high / enough / too small)	Monthly
Production	Production during last month (increased / unchanged / decreased)	Monthly
Short-time	Firm currently uses short-time work	Quarterly
Export	Firm is exporting	Quarterly
Future expectations		
State exp	Expected business over next 6 months (improvement / no change / worsening)	Monthly
Empl exp	Expected employment over next 3 months (increase / no change / decrease)	Monthly
Headcount	Employees for demand over next 12 months (too few / enough / too many)	Quarterly
Short-time exp	Expecting to use short-time work during next 3 months	Quarterly

3.3.2 The Treatment of Constrained Credit Supply

Based on the panel structure of the data, we derive a variable indicating that a firm receives the treatment of constrained credit supply in a particular month. Since 2003, firms in the Ifo Business Survey are asked how they perceive "banks' willingness to supply credit". Possible appraisal categories are "restrictive", "normal", and "accommodating". This enables us to directly measure a change in credit supply to a firm. We define a firm as *restricted* or *treated* in month t ($Restricted_{i,t} = 1$) if it reports "restrictive" bank lending in month t after reporting "normal" or "accommodating" bank lending in the previous twelve months. A firm is defined as *unrestricted* or *untreated* in t ($Restricted_{i,t} = 0$) if it reports "normal" or "accommodating" bank lending in twelve subsequent months and does not switch to reporting "restrictive" bank lending in month t. This treatment definition allows the estimation of a treatment effect without bias from possible previous treatments. However, it comes at the cost of using only a fraction of the firms in the panel data set.

Estimating the treatment effect on a monthly basis requires an assumption on the exact treatment month for the time from 2003 to September 2008 in which the survey question on credit supply is asked only twice a year, namely in March and August. If a firm switches to reporting "restrictive" bank lending between two surveys in this time period, we assume that the treatment of constrained credit supply occurs in the first month after a firm has reported "normal" or "accommodating" the last time. This assumption is further illustrated and discussed in Appendix A.

After conditioning on the availability of all control variables in the pre-treatment month *t-1* (see Sections 3.3.4 and 3.3.5), the sample consists of 333 firm-month observations in the treatment group and 5,061 untreated firm-month observations in the control group.[16] Figure 1 in Appendix C shows the distribution of treated observations over time. As expected, the number of treated firms increases sharply in the wake of the financial crisis.

[16]The number of observations used in the following analysis is somewhat lower because of missing data in the post-treatment outcome variables.

Our approach to measuring credit supply is similar to the one used by Campello, Graham, and Harvey (2010), who also use survey data to identify financial constraints at the firm-level. Such a perception-based approach has the caveat that the definition of what firms consider to be restrictive bank lending may differ over time or across industries. In the OLS estimations, this problem is dealt with by the inclusion of time and industry dummy variables. PSM estimation allows an even more stringent solution, namely matching firms exactly on quarter-industry cells.

3.3.3 Measuring Firm-Level Real Economic Activity

The Ifo Business Survey provides a precise measure of firm-level real economic activity. On a monthly basis, firms report whether their production has "increased", "not changed", or "decreased" during the last month compared to the month before. Answers to this question in every month from *t+1* to *t+12* measure changes in firm-level production after the treatment of constrained credit supply is assigned in month t.

The main dependent variable in the following analysis is the variable $Slowdown_{i,t+12}$, which indicates that a firm reports a decrease in production at least once during the twelve post-treatment months. We focus our analysis on this negative outcome because we are primarily concerned about restraining effects of credit supply. As a robustness check, we also run estimations using the outcome variable $Slowdown\_avg_{i,t+12}$, which measures the fraction of the twelve post-treatment months in which a firm reports a decreasing production.

Furthermore, we estimate the treatment effect of constrained credit supply on a firm's annual employment growth rate $\Delta Empl_{i,t+12}$, which is calculated as the symmetric growth rate

$$\Delta Empl_{i,t+12} = \frac{Empl_{i,t+12} - Empl_{i,t}}{0.5(Empl_{i,t+12} + Empl_{i,t})} \tag{3.1}$$

as suggested by Haltiwanger, Jarmin, and Miranda (2013) and applied by Chodorow-Reich (2014). To avoid estimations being affected by extreme values due to survey response behaviour, one percent of the observations are cut off from both sides of the distribution of $\Delta Empl_{i,t+12}$.

A caveat in the employment data stems from the fact that firms report their number of employees in the Ifo Business Survey every year in November. As the treatment variable is defined on a monthly basis, this raises difficulties in the distinction between post-treatment and pre-treatment changes in employment. Appendix B contains a discussion of this issue and presents the conservative approach to deal with it in this study. Nevertheless, the exact level of the employment effects of credit supply estimated should be interpreted with caution. Therefore, we use the dummy variable $Empl\_decrease_{i,t+12}$ indicating that a firm has a negative post-treatment employment growth rate as an alternative dependent variable.

Table 3.2: Firms' post-treatment business activity

	$Restricted_{i,t} = 1$			$Restricted_{i,t} = 0$			
t+12	N	$\bar{X}_R$	σ_R	N	$\bar{X}_U$	σ_U	$p > t$
Production							
Slowdown	316	69.9%	0.46	4877	52.5%	0.50	0.000***
Slowdown_avg	316	23.5%	0.24	4877	13.6%	0.19	0.000***
Employment							
$\Delta Empl$	302	-1.5%	0.12	4739	1.0%	0.10	0.000***
Empl_decrease	314	46.8%	0.50	4827	31.8%	0.47	0.000***

Notes: The table shows the descriptive statistics of post-treatment outcome variables separately for treated and untreated firms; treatment status $Restricted_{i,t}$ is defined as described in Section 3.3.2; p-values are reported for a two-group mean comparison t-test with H_0: $\bar{X}_R = \bar{X}_U$; $\Delta Empl$ is cut by one percent from both sides of the distribution to deal with extreme values; these observations are kept in *Empl_decrease*; the samples contain only observations for which all pre-treatment control variables listed in Table 3.3 and Table 3.4 are available; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

According to Table 3.2, firms that experience the treatment of constrained credit supply in t show significantly lower post-treatment real economic activity than untreated firms in all four outcome variables. They are significantly more likely to report a decreasing production at least once during the post-treatment year. On average, restricted firms face decreasing production in almost a quarter of the twelve post-treatment months while this figure is 10 percentage points lower for unrestricted firms. Employment growth rates show that restricted firms are shrinking by 1.5 percent on average in the post-treatment year while unrestricted firms grow by 1.0 percent. Finally, the probability of a decreasing employment is 15 percentage points higher for a restricted firm.

3.3.4 Heterogeneity in Firms' Balance Sheets

Table 3.3 illustrates how restricted and unrestricted firms differ in pre-treatment size and balance sheet variables $B_{i,t-1}$. These variables are often used in the literature to control for firm-side factors when estimating the real effects of credit supply. Firm size is widely used as a predictor of financial constraints because large firms tend to be older and more transparent, which may facilitate access to credit. In Table 3.3, however, restricted and unrestricted firms do not differ significantly in $log(Empl)$ or $log(Assets)$.

Table 3.3: Firms' balance sheets in *t-1*

	$Restricted_{i,t} = 1$ (N=333)			$Restricted_{i,t} = 0$ (N=5,061)			
t-1	$\bar{X}_R$	X_R^{med}	σ_R	$\bar{X}_U$	X_U^{med}	σ_U	$p > t$
Firm size							
log(Empl)	5.4	5.3	1.3	5.5	5.4	1.1	0.25
log(Assets)	17.1	16.9	1.7	17.2	17.0	1.6	0.38
Balance sheet data							
Equity	33.2%	31.1%	25.4%	39.2%	38.3%	21.9%	0.000***
Cash	10.0%	4.3%	12.8%	11.3%	5.7%	13.8%	0.08*
Long-term debt	15.4%	7.7%	19.7%	13.4%	6.4%	17.5%	0.05*
Short-term debt	32.7%	30.2%	24.1%	30.7%	25.7%	40.6%	0.36
Cash flow	7.9%	7.8%	11.9%	10.5%	9.6%	12.2%	0.000***
ROA	-8.1%	3.0%	38.6%	-8.3%	6.1%	47.7%	0.93
Interest coverage	17.4	1.3	42.3	22.9	4.4	46.5	0.03**

Notes: The table shows descriptive statistics of pre-treatment variables $B_{i,t-1}$ separately for treated and untreated firms; treatment status $Restricted_{i,t}$ is defined as described in Section 3.3.2; p-values are reported for a two-group mean comparison t-test with H_0: $\bar{X}_R = \bar{X}_U$; *Interest coverage* is set to zero if values are negative and is winsorized at the 95 percentile; the samples contain only observations for which all pre-treatment control variables listed here and in Table 3.4 are available; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The other variables in Table 3.3 are in line with existing empirical literature showing that restricted firms are in a worse financial condition than unrestricted ones. They have significantly lower equity ratios, fewer cash holdings, more long-term debt, lower cash flows, and lower interest coverage ratios.[17] Restricted firms also hold more short-term debt and are characterised by a lower operating profitability (*ROA*). Therefore, it is important to rule out heterogeneity in firms' balance sheets in the estimation of real effects of credit supply.

3.3.5 Heterogeneity in Firms' Current Business Situation and Future Expectations

Beyond the information contained in balance sheets, Table 3.4 shows that restricted and unrestricted firms also differ significantly in their pre-treatment current business situation $C_{i,t-1}$ and future expectations $F_{i,t-1}$. All variables stem from the firms' appraisals and assessments reported in the Ifo Business Survey.

First, firms are asked directly how they appraise their current business situation. Restricted firms are significantly less likely to report a "good" situation (*State (+)*), but significantly more likely to report a "bad" one (*State(-)*).[18] Second, firms provide an assessment of their current stock of orders, which restricted firms are significantly more likely to report as being "too small" (*Orders (-)*). Third, restricted firms are significantly more likely to report a decreasing production in the pre-treatment month (*Production (-)*), which indicates a pre-treatment trend in the outcome variable for real economic activity. Finally, variables indicating that a firm is currently using short-time work and whether it is exporting do not show any significant differences.[19]

[17] *Interest coverage* is set to zero if it takes on negative values and winsorised at the 95 percentile.

[18] Throughout this paper, only good and bad appraisals or assessments are reported for variables from the Ifo Business Survey. Neutral categories constitute the baseline category in all estimations.

[19] Short-time work is a labour market instrument widely used by German firms to adjust their capacities to business cycle or seasonal demand fluctuations. The practice was particularly widespread during the financial crisis. Export status is a crucial control variable because foreign demand in Germany was severely affected by the financial crisis, which could affect both access to credit and real economic activity of a firm.

Forward-looking firm characteristics in the data show further heterogeneity between restricted and unrestricted firms that might not be captured by backward-looking balance sheet variables. As shown in Table 3.4, restricted firms are significantly less likely to expect business to “improve” over the next six months (*State exp (+)*), but significantly more likely to expect business to “worsen” (*State exp (-)*).

Table 3.4: Firms' current business situation and future expectations in *t-1*

	$Restricted_{i,t} = 1$ (N=333)			$Restricted_{i,t} = 0$ (N=5,061)			
t-1	$\bar{X}_R$	X_R^{med}	σ_R	$\bar{X}_U$	X_U^{med}	σ_U	$p > t$
Current business situation							
State (+)	21.6%	0	41.2%	25.9%	0	43.8%	0.08*
State (-)	27.9%	0	44.9%	20.8%	0	40.6%	0.002***
Orders (+)	10.5%	0	30.7%	13.0%	0	33.6%	0.19
Orders (-)	39.3%	0	48.9%	32.9%	0	47.0%	0.02**
Production (+)	12.3%	0	32.9%	14.4%	0	35.1%	0.29
Production (-)	23.7%	0	42.6%	19.3%	0	39.5%	0.05*
Short-time	13.2%	0	33.9%	14.5%	0	35.2%	0.53
Export	87.4%	1	33.2%	88.6%	1	31.8%	0.50
Future expectations							
State exp (+)	15.3%	0	36.1%	19.0%	0	39.2%	0.09*
State exp (-)	25.5%	0	43.7%	18.8%	0	39.1%	0.002***
Empl exp (+)	5.7%	0	23.2%	6.6%	0	24.9%	0.51
Empl exp (-)	23.1%	0	42.2%	15.8%	0	36.5%	0.000***
Headcount (+)	5.7%	0	23.2%	6.3%	0	24.3%	0.67
Headcount (-)	27.0%	0	44.5%	20.1%	0	40.1%	0.002***
Short-time exp	19.2%	0	39.5%	19.7%	0	39.8%	0.82

Notes: The table shows descriptive statistics of pre-treatment variables $C_{i,t-1}$ and $F_{i,t-1}$ separately for treated and untreated firms; treatment status $Restricted_{i,t}$ is defined as described in Section 3.3.2; p-values are reported for a two-group mean comparison t-test with H_0: $\bar{X}_R = \bar{X}_U$; the samples contain only observations for which all pre-treatment control variables listed here and in Table 3.3 are available; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This picture is confirmed for the variable indicating that a firm expects its employment figures to decrease over the next three months (*Empl exp (-)*) and the variable indicating that a firm considers its workforce too large for the product demand over the next twelve months (*Headcount (-)*). There is no such difference in the expectation of short-time work (*Short-time exp*).

In sum, variables measuring firms' current business situation and future expectations are highly relevant in predicting constrained credit supply. As a firm's production and employment decisions also depend on its current business situation and future expectations, heterogeneity in these variables must be controlled for to ensure an unbiased estimation of the real effects of credit supply. As balance sheet variables are backward-looking only, additional control variables from survey data could provide a key contribution in this concern.

3.4 Methodology and Results

3.4.1 Methodology

For every firm i in the panel data set, we observe whether it receives a treatment of constrained credit supply in month t ($Restricted_{i,t} = 1$) and estimate the effect of this treatment on the likelihood of a decrease in production during the subsequent twelve months ($Slowdown_{i,t+12} = 1$).[20] In a first step, we estimate

$$E[Slowdown_{i,t+12}|B_{i,t-1}] = \beta_0^B + \beta_1^B Restricted_{i,t} + \beta_2^B B_{i,t-1} + \epsilon_i \tag{3.2}$$

where $B_{i,t-1}$ is a set of variables from a firm's most recent balance sheet in the pre-treatment month *t-1*.

[20] We illustrate the empirical strategy based on the notation of an OLS estimation. In Section 3.4.3, we also test the hypothesis using PSM estimation.

We then extend the estimation to

$$\begin{aligned} E[Slowdown_{i,t+12}|B_{i,t-1}, C_{i,t-1}, F_{i,t-1}] = \beta_0^{BCF} + \beta_1^{BCF} Restricted_{i,t} + \beta_2^{BCF} B_{i,t-1} \\ + \beta_3^{BCF} C_{i,t-1} + \beta_4^{BCF} F_{i,t-1} + \zeta_i \end{aligned} \quad (3.3)$$

by additionally controlling for a set of survey-based variables measuring firms' pre-treatment current business situation $C_{i,t-1}$ and expectations for the future business $F_{i,t-1}$. Finally, we calculate the impact of these additional control variables on the estimated treatment effect as

$$Diff^{CF} = \beta_1^{B} - \beta_1^{BCF} \quad (3.4)$$

which measures the degree to which the estimated real effects of credit supply are biased when controlling for balance sheet variables only, without accounting for heterogeneity in firms' current business situation and future expectations.

3.4.2 Ordinary Least Squares Estimations

Pooled OLS estimation provides a first test of whether controlling for firms' current business situation and future expectations affects the estimation of the real effects of credit supply. In line with Equation (1) in Section 3.4.1, the dummy variable $Slowdown_{i,t+12}$ is regressed on the treatment status $Restricted_{i,t}$, a set of balance sheet variables $B_{i,t-1}$, and quarter and industry dummy variables based on the two-digit WZ 2008 industry classification. The results in Estimation (1) in Table 3.5 suggest that constrained credit supply increases the probability of decreasing production between t and *t+12* by 8.65 percentage points.

When additionally controlling for firms' current business situation $C_{i,t-1}$ and future expectations $F_{i,t-1}$ in Estimation (2), this effect is reduced to 7.01 percentage points. Column (3) shows that the difference between the two estimated coefficients is highly statistically significant.[21] The adjusted R^2 statistics further show that the inclusion of the control variables $C_{i,t-1}$ and $F_{i,t-1}$ increases the explanatory power of the model substantially.

Therefore, OLS estimation provides first support for the hypothesis that not controlling for contemporaneous and forward-looking firm-side factors leads to a significant overestimation of the impact of credit supply-side factors on real economic activity at the firm-level. The inclusion of survey-based measures for firms' current business situation and future expectations reduces the estimated effect by almost 20 percent.

Table 3.5: OLS estimations using $Slowdown_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	0.0865***	0.0701***	**0.0164****
	(0.027)	(0.026)	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
Month	Yes	Yes	
Industry	Yes	Yes	
Adj. R^2	0.1666	0.2200	
N	5,193	5,193	

Notes: The table provides results for OLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Restricted_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; "Firm size", "Balance sheet data", "Current situation", and "Future expectations" are sets of control variables as listed in Table 3.1; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples contain only those observations for which all control variables of Estimation (2) are available; the third column provides the difference between the two estimated coefficients; its significance is tested using a t-test with H_0: $\beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[21] To test this significance, standard test procedures are not applicable because the two coefficients come from different estimations. Therefore, the significance level for the test procedure that Clogg, Petkova, and Haritou (1995) have developed for such a case is reported here.

3.4.3 Propensity Score Matching Estimations

OLS estimation has two potential disadvantages in the context of this analysis. First, it contains the risk of misspecifying the functional form of $E[Slowdown_{i,t+12}]$. Second, OLS estimation leads to a comparison of observations outside the common support if distributions of control variables vary between treated and untreated firms. This is most likely to be a problem for the balance sheet variables $B_{i,t-1}$, which show large standard errors that differ between treated and untreated firms according to Table 3.3. To address these issues, the effect of constrained credit supply on firm-level production can be estimated using a matching estimation approach, as done by Campello, Graham, and Harvey (2010) and Almeida, Campello, Laranjeira, and Weisbenner (2011).

The following analysis is based on PSM estimation following Rosenbaum and Rubin (1983) because the large number of control variables, including continuous balance sheet variables in $B_{i,t-1}$, inhibits the identification of matching firms that are identical with respect to all control variables. Comparing each restricted firm to unrestricted ones with a similar propensity score provides an estimated treatment effect that is close to the one derived from an experimental setting (Dehejia and Wahba, 1999) in which restrictive bank lending is randomly assigned. The identifying assumption still hinges on the choice of matching variables. In this concern, our study provides a major contribution to the existing literature by using a broad set of matching variables

Following the empirical strategy in Section 3.4.1, firms are first matched on balance sheet variables $B_{i,t-1}$ only. This requires the estimation of a logistic regression model

$$Pr(Restricted_{i,t} = 1 | B_{i,t-1}) = \Phi(\alpha_0 + \alpha_1 B_{i,t-1} + \alpha_2 Industry_i > u_i) \tag{3.5}$$

where $\Phi(\cdot)$ denotes the cumulative distribution function of the logistic distribution and $Industry_i$ is a set of industry dummy variables.[22] To allow for time-heterogeneous effects, estimations are run separately within every quarter.[23]

[22] To avoid the impact of extreme values on the estimation, the delta deviance influence statistic is extracted for every observation. Following Agresti and Finlay (2008), an observation is dropped if $\sqrt{|ddeviance|} > 3$ and the maximum likelihood estimation is re-run until no observations are dropped.

[23] Estimations within quarter-industry cells are not possible due to a lack of observations. We therefore estimate the model separately for every quarter and include industry dummy variables.

From the estimated models, the propensity score, which is the conditional probability of being treated, is predicted for every firm-month observation. Every restricted firm is then linked to unrestricted firms from the same quarter-industry cell that have a similar propensity score based on ten nearest neighbours matching. Using exact matching on quarter-industry cells rules out macroeconomic and industry-specific effects, and deals with both the time- and industry-dependence of the perception-based treatment variable. Finally, the treatment effect is estimated using a Weighted Least Squares (WLS) estimation of the outcome variable $Slowdown_{i,t+12}$ on the treatment status $Restricted_{i,t}$ with weights being drawn from the PSM procedure. In a second step, we re-run this matching procedure adding matching variables for firms' current business situations $C_{i,t-1}$ and future expectations $F_{i,t-1}$ in addition to $B_{i,t-1}$.

Table 3.6: PSM estimation using $Slowdown_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	5.10%**	2.59%	**2.51%**
	(0.0236)	(0.0297)	
Industry	Yes	Yes	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
p>t	0.03	0.38	
Upper bound	8.98%	7.47%	
Lower bound	1.22%	-2.30%	
Treated obs.	223	141	
Matching obs.	1,351	806	

Notes: The table provides results for WLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Restricted_{i,t}$; in Estimation (1), weights are derived from PSM based on pre-treatment firm size and balance sheet data; in Estimation (2), weights are derived from PSM based on pre-treatment firm size, balance sheet data, current business situation, and future expectations; industry dummy variables based on the two-digit WZ 2008 industry classification are also included in all PSM estimations; p-values are reported for a t-test of significance of the estimated treatment effect; the significance of the difference between the two estimated effects in the third column cannot be tested; upper and lower bounds are reported for the 95 percent confidence interval of each estimator; standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

When matching only on balance sheet variables, the results for Estimation (1) in Table 3.6 suggest that constrained credit supply increases the probability of a decreasing production in the post-treatment year significantly by 5.10 percentage points. Estimation (2), however, shows that this treatment effect is only 2.59 percentage points and statistically insignificant when also matching on firms' current business situation and future expectations.

This suggests that the sole reliance on balance sheet data leads to an overestimation of real effects of credit supply-side factors by almost 50 percent.[24] Unfortunately, the statistical significance of the difference between the two estimators cannot be tested because the test by Clogg, Petkova, and Haritou (1995) requires each observation to receive the same weight in both estimations, which is contrary to the core idea of a matching estimator.

It is particularly noteworthy that Estimation (2) shows no evidence for credit supply-side effects on real economic activity in Germany between 2003 and 2011 as suggested in Estimation (1). Therefore, PSM estimation strengthens the empirical support for the hypothesis that a lack of control variables to measure firms' current business situations and future expectations leads to an overestimation of real effects of credit supply.

3.5 Robustness Checks

3.5.1 Potential Caveats of the Survey Data

The survey questions raise three problems in the context of our analysis. First, respondents could be in a bad mood because of their firm's general situation and future expectations, and react by reporting restrictive bank lending (e.g., to blame banks for their situation). Therefore, the lower estimated effect of constrained credit supply could not be attributed to survey-based variables providing information that is not captured by balance sheets. Empirical evidence by Abberger, Birnbrich, and Seiler (2009) provides strong evidence against this critique. They use data from a meta-study among respon-

[24]Table C.1 in Appendix C shows that matching on balance sheet variables only does not eliminate bias in the survey-based variables entirely. Matching on all variables for firms' current business situation and future expectations improves the balancing properties.

dents of the Ifo Business Survey to show that general mood and economy-wide factors are not important determinants of firms' general appraisals in the survey.

Second, when providing appraisals of their current business situation, firms may just have their financial situation and therefore their balance sheet in mind. If that was the case, the smaller estimated effects when including survey-based control variables would only be mechanical due to the fact that more control variables are used. For firms' current business situation, their revenues and turnover are the most important determinants according to Abberger, Birnbrich, and Seiler (2009). There is no evidence that firms think about their financial situation when filling out the survey.

Finally, forward-looking appraisals could contain expectations of constrained credit supply. In this case, the explanatory power of these variables would mean that firms' anticipation of the treatment turn out correct, but it could not be taken as evidence for the importance of ruling out forward-looking firm-side factors. For firms' future expectations, Abberger, Birnbrich, and Seiler (2009) show that costs and liquidity, which could encompass the consequences of constrained credit supply, are only the fourth and fifth most important determinants.

We test the robustness of our results with respect to the anticipation of constrained credit supply by re-running the estimations controlling only for those survey-based variables that explicitly refer to factual aspects of business. These are assessments of the current stock of orders, as well as the current status concerning short-time work and export. We also keep the variables *Headcount (+)* and *Headcount (-)* as they refer explicitly to employment figures relative to future demand and are therefore unaffected by firms' expectations of access to credit supply. The general appraisals *State*, *Production*, *State exp*, *Short-time exp*, and *Empl exp* are excluded because they could be affected by expectations of constrained credit supply.

Table C.2 in Appendix C shows that controlling for such a reduced set of variables for firms' current business situation and future expectations in OLS estimations leads to a significant reduction of the estimated credit supply-side effects by 1.5 percentage points. The difference is only slightly smaller than in our baseline estimation in Section 3.4.2, and is still highly statistically significant.[25] The adjusted R^2 statistic increases substantially suggesting a better model fit in Estimation (2) even when using a reduced set of variables.

This is confirmed by PSM estimations with a reduced set of control variables. Matching on balance sheet variables only leads to an estimated treatment effect of 5.74 percentage points in Estimation (1) in Table C.3 in Appendix C. Controlling for the factual assessments of firms' current situation and future expectations without using general appraisals lowers the estimated effect to 3.16 percentage points in Estimation (2) and the estimator turns statistically insignificant.

Therefore, even if the power of general assessments of contemporaneous and forward-looking firm-side factors are not to be believed, the omission of more factual variables still induces an overestimation of real effects of credit supply. For both the OLS and PSM estimation the suggested overestimation is only slightly smaller than in estimations based on all survey-based variables in Section 3.4.2 and 3.4.3.

3.5.2 An Alternative Measure for Firm-Level Production

So far, results were shown for the estimated effects of constrained credit supply on the probability that a firm reports a decreasing production at least once in the twelve post-treatment months ($Slowdown_{i,t+12}$). As a robustness check, we re-run the previous estimations using the fraction of the twelve post-treatment months in which firms report decreasing production ($Slowdown\_avg_{i,t+12}$) as the dependent variable.

The results for OLS estimations in Table C.4 in Appendix C show that the omission of control variables for firms' current business situation and future expectations lowers the estimated treatment effect of $Restricted_{i,t}$ on $Slowdown\_avg_{i,t+12}$ by about 12 percent. The difference is statistically significant and the adjusted R^2 statistic indicates a much better model fit when including contemporaneous and forward-looking control variables.

[25] The effect when controlling only for balance sheets variables differs from the one shown in Table 3.5. Although control variables do not change compared to the estimation in Table 3.5, conditioning on the availability of the smaller set of control variables in Estimation (2) affects the sample composition, and therefore alters the estimated effect.

When applying PSM estimation with $Slowdown\_avg_{i,t+12}$ as the dependent variable, this is confirmed. Matching on balance sheet variables only, Estimation (1) in Table C.5 shows a highly significant credit supply-side effect on firm-level production of 3.78 percentage points. Adding matching variables for firms' current business situation and future expectations in Estimation (2) substantially lowers the effect by 1.01 percentage points. In contrast to estimations using $Slowdown_{i,t+12}$, however, the effect remains significant at the ten percent level. Even when using an alternative outcome variable for firm-level production, these estimation results underline the importance of controlling for firms' current business situation and future expectations in the estimation of real effects of credit supply.

3.5.3 Employment Effects

The previous estimated effects of constrained credit supply on firm-level production raise the question whether the omission of variables capturing contemporaneous and forward-looking firm-side factors also affects the estimation of employment effects of constrained credit supply. Tables C.6 and C.7 in Appendix C show that controlling for firms' current business situation and future expectations lowers the estimated effect of constrained credit supply on a firm's annual employment growth rate. The difference is statistically significant in the OLS estimation. For PSM estimation, the effect turns insignificant and the two effects lie outside each other's 95 percent confidence intervals. As mentioned in Section 3.3.3, the exact level of the estimated employment effects should be interpreted with caution due to the problems in linking annual employment data to the monthly treatment variable (see Appendix B for a discussion).

Table C.8 in Appendix C therefore provides OLS estimations of credit supply-side effects on the probability that a firm experiences a negative employment growth rate in the post-treatment year ($Empl\_decrease_{i,t+12}$). The inclusion of control variables for firms' current business situation and future expectations lowers the estimated effect significantly and the adjusted R^2 statistic increases substantially. PSM estimations in Table C.9 in Appendix C further show that the estimated treatment effect of constrained credit supply turns insignificant when including all control variables. The two estimators lie outside each others' 95 percent confidence intervals. In sum, these results show that heterogeneity in firms' current business situation and future expectations that is not captured by balance sheet data also affects the estimation of employment effects of constrained credit supply.

3.5.4 Sample Selection in PSM Estimations

The differences between PSM estimations with and without matching on firms' current business situation and future expectations could be caused by the mere fact that different samples of treated firms are used in the two estimations, which is obvious from the numbers of observations. The samples differ for three reasons. First, when matching on additional variables $C_{i,t-1}$ and $F_{i,t-1}$, the logistic regression may no longer converge in some quarters so that the propensity score is not estimated for the respective treated observations. Second, the inclusion of these binary variables may induce perfect predictability so that some variables and the respective treated observations are dropped. Finally, increasing the number of matching variables makes it more likely that observations are dropped because the common support condition is violated.

To test whether the previous results are actually driven by better matching quality, post-treatment outcome variables for the treated firms in both samples are compared in Table C.10 in Appendix C. For $Slowdown_{i,t+12}$, firms differ only slightly. In fact, firms in the second sample show an even larger probability of decreasing production, which strengthens the interpretation that solely matching on balance sheet data induces an overestimation of the real effects of credit supply. Two-group mean comparison tests indicate that the difference is statistically insignificant. The same holds for $Slowdown\_avg_{i,t+12}$. For $Empl\_decrease_{i,t+12}$ and $\Delta Empl_{i,t+12}$, parts of the reduction in the estimated treatment effect of constrained credit supply can be explained by differences between the treated firms, but the difference is statistically insignificant and there is still a substantial reduction remaining due to improved matching quality.

3.5.5 The Role of the Financial Crisis of 2007-09

The sample period from 2003 to 2011 covers the financial crisis, which might influence the extent to which omitting firms' current business situation and future expectations affect the estimation of the real effects of credit supply. Firms were operating under extreme conditions during the financial crisis. Therefore, firm balance sheet data from previous years turned less informative for banks. Under such circumstances, banks may base their lending decisions even more on contemporaneous and forward-looking firm characteristics.

As Tables C.11 and C.12 in Appendix C show, controlling for firms' current business situation and future expectations lowers the estimated treatment effect of constrained credit supply on firm-level production in the financial crisis subsample as of July 2007. For the pre-crisis sample, however, estimates are unreliable because only a few firms are treated in this period and the sample turns out too small for our estimation procedure. Therefore, the subsample analysis underlines the importance of contemporaneous and forward-looking information on firms in times of financial crisis when balance sheet data from previous years might turn less informative. Whether there is a difference compared to normal times, however, remains open for future research when more "non-crisis" data is available.

3.6 Discussion: No Real Effects of Credit Supply?

Most PSM estimations in this paper show no significant effect of credit supply-side factors on real economic activity. This contrasts what appears to be conventional wisdom;, for example about the financial crisis of 2007-09. The insignificant effects may be specific to this study because it is based on data from Germany where the economic crisis was less driven by bank-side factors.

According to the International Monetary Fund (2009), the economic crisis in the German economy, which depends strongly on exports, was induced by a sharp drop in foreign demand. Problems in the banking sector arose because firms were unable to repay their debt and increasing uncertainty prevented banks from lending. This, however, is in line with credit constraints being driven by firm-side, and not by credit supply-side factors. Supporting this view, Rottmann and Wollmershaeuser (2013) show that capital ratios of German banks rose from 4% early 2008 to 4.5% by the end of 2009. They also argue that the establishment of the Financial Markets Stabilization Fund in October 2008 helped avoid liquidity constraints in the banking sector. Therefore, it is not surprising that previous PSM estimations did not provide evidence for real effects of credit supply in Germany between 2003 and 2011.

Concerning the external validity of these results, we wish to emphasise that this picture may be different for other time periods or other countries. However, the finding that a lack of controlling for contemporaneous and forward-looking firm-side factors leads to overestimated credit supply-side effects, is generally applicable.

3.7 Conclusion

To design appropriate policy measures encountering a lack of access to credit for firms, it is crucial to understand whether this is driven by credit supply-side or firm-side factors. If economic growth is slowed down by limited lending due to credit supply-side factors (e.g., due to banks facing a liquidity crunch), government intervention in the banking sector may be justifiable. If, however, growth rates are hampered by firm-side factors (e.g., a drop in demand for the firms' products), and credit volumes decrease in response to higher default risk of firms, government intervention, if such is deemed necessary, should not necessarily be directed at banks.

To identify credit supply-side effects on real economic activity, firm-side effects must be ruled out. Existing firm-level analyses do so primarily using data from firms' balance sheets. Although these provide an accurate picture of a firm's financial condition, they are backward-looking and do not contain information on firms' current business situation and future expectations. Therefore, analyses are often complemented with market data, which limits empirical studies to publicly traded firms.

This study analyses the question of whether controlling for survey-based measures of firms' current business situation and future expectations makes a difference in the estimation of the real effects of credit supply. To that end, we estimate the effect of constrained credit supply on firm-level production in Germany between 2003 and 2011. When including survey-based appraisals of firms' current business situation and future expectations, in addition to balance sheet variables, OLS estimators remain significant, but turn out significantly lower than without these additional control variables. When applying a PSM approach, the estimated treatment effect of credit supply on firm-level production is also substantially reduced and turns insignificant. The importance of ruling out contemporaneous and forward-looking firm-side factors is further shown for estimations of the effects of constrained credit supply on firm-level employment growth.

Our results indicate that ignoring contemporaneous and forward-looking information on the credit demand-side may lead to an overestimation of credit supply-side effects on real economic activity. Studies analysing the real effects of credit supply should take this into account. For firm-level analyses, micro-data from surveys are an option for circumventing overestimation if other data (e.g., market data or precise information on order books) are not available. For macro-level analyses, indices such as the Purchasing Mangers Index (PMI) could be used. Further research using data for publicly traded firms could also shed light on the question whether the direct measures for firms' current situation and future expectations rule out bias that is not captured by market data.

Appendices

Appendix to Chapter 1

Appendix: Sample Representativeness

As the analysis in Chapter 1 is based on survey data, response behaviour could prompt sample selection issues. Firms with impaired credit finance could be overrepresented in the sample since the topic of the survey was the firms' financing situation. One could argue that firms that did not need credit finance and those that did not experience any impairments of it were less likely to participate in the survey because they lack interest in the topic or they did not consider themselves to have anything to contribute.

To test whether certain firms are overrepresented in the sample, we compare firms responding to the Ifo "Financing of the German Economy" survey ("response" firms) to those who did not respond ("non-response" firms) with respect to information that the Ifo Investment Survey provides about both groups. Since both surveys are based on the same address database, there is a sufficient overlap between the data sets. That firms with impaired credit finance are also overrepresented in the sample of the Ifo Investment Survey is unlikely because the main topic of this survey is the development and the structure of firms' investment activities. The motivation to respond should therefore not be affected by the firm's credit financing during the financial crisis.

The first two columns of Table A.1 show that response firms are slightly larger than non-response firms in terms of employment and turnover in 2010, but the differences are statistically insignificant. Even stronger arguments in favour of sample representativeness can be found in the lower part of Table A.1. Every year, firms participating in the Ifo Investment Survey are asked to assess how the financing situation affects their investment in the current year. The answers range from 1 ("strong animation") to 5 ("strong slowdown"). If firms that see credit finance impaired by the financial crisis were to be more likely to respond to the Ifo "Financing of the German Economy" survey, we would expect the according answers in the Ifo Investment Survey to differ significantly between response and non-response firms. Table A.1, however, shows no difference in the influence of finance on investment between the two groups.

Table A.1: Analysis of response behaviour based on data from the Ifo Investment Survey

	Non-response	Response	$p > t$
Turnover (2010, in m Euro)	265.68	398.94	0.2811
N	1,118	591	
Employment (2010)	808.49	1050.32	0.3881
N	1,118	591	
Influence Finance 2007	2.96	2.93	0.4737
N	809	444	
Influence Finance 2008	3.05	3.03	0.5120
N	859	468	
Influence Finance 2009	3.38	3.41	0.4632
N	922	500	

Note: The table shows the comparison of firms that participated in the Ifo "Financing of the German Economy" survey ("Response") and firms that did not return the questionnaire ("Non-response"); all variables used to compare the two groups are drawn from the Ifo Investment Survey; p-values are reported for a standard mean comparison t-test; employment is measured in heads; *Influence Finance* provides a firm's appraisal of the influence of the financing situation on investment in the current year with the following answer categories: 1 Strong animation, 2 Little animation, 3 No influence, 4 Little slowdown, 5 Strong slowdown.

Empirical Appendix

Table A.2: Features of main bank relationships

	N	Freq.	Perc.
Long duration	1,610	1,374	85.34%
Personal support	1,610	1,067	66.27%
Short distance	1,610	845	52.48%
Company knowledge	1,610	694	43.11%
Difficult times	1,610	591	36.71%
Important creditor	1,610	514	31.93%
Others	1,610	39	2.42%

Note: For the two most important bank relationships, firms reported whether or not the different features characterise the bank relationship and whether the bank is a main bank. Here, a data set of main bank relationships is constructed. The dummy variables equal one if the respective feature is reported for the respective main bank relationship.

Table A.3: Baseline estimations

	(1) *Information*	(2) *Reduction*	(3) *Availability*	(4) *Interest*	(5) *Maturities*	(6) *Collateral*
Main bank (1)	-0.1543***	0.0017	-0.0371	-0.0990**	-0.0547**	-0.0789**
	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)
Main bank (2)	-0.1044**	0.0125	-0.0298	-0.1122***	-0.0455**	-0.0472
	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)
log(Employees)	0.0066	0.0029	0.0203	0.0272*	-0.0187	0.0191
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
log(Assets)	0.0086	0.0104	-0.0011	-0.0100	0.0132*	-0.0036
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Equity	0.0025	-0.0341	-0.0462	-0.0422	-0.0004	-0.0375
	(0.05)	(0.04)	(0.05)	(0.05)	(0.03)	(0.04)
Long-term debt	0.0330	0.0299	0.0963	0.0645	-0.0204	0.0222
	(0.06)	(0.05)	(0.07)	(0.06)	(0.03)	(0.06)
Cash	-0.3082***	-0.0612	-0.1880**	-0.1475*	-0.0287	-0.2384***
	(0.10)	(0.07)	(0.09)	(0.09)	(0.05)	(0.08)
Return (3% to <7%)	-0.0759**	-0.0599***	-0.0394	-0.0782***	-0.0143	-0.0651**
	(0.03)	(0.02)	(0.03)	(0.03)	(0.01)	(0.03)
Return (7% to <10%)	-0.0416	-0.0846***	0.0085	-0.0615*	-0.0008	-0.0234
	(0.04)	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)
Return (10% +)	-0.0638	-0.0514*	0.0157	-0.0660	-0.0094	-0.0673*
	(0.05)	(0.03)	(0.05)	(0.04)	(0.02)	(0.04)
Earlypay (0% to <25%)	-0.0114	0.0048	0.0037	0.0642	-0.0959	-0.0201
	(0.11)	(0.07)	(0.09)	(0.10)	(0.08)	(0.10)
Earlypay (25% to <50%)	-0.0294	0.0353	0.0143	-0.0309	-0.0751	-0.0029
	(0.11)	(0.07)	(0.09)	(0.09)	(0.08)	(0.10)
Earlypay (50% to <75%)	0.0342	0.0331	0.0448	0.0542	-0.1312*	0.0067
	(0.11)	(0.07)	(0.09)	(0.10)	(0.08)	(0.10)
Earlypay (75% +)	-0.1058	-0.0463	-0.0763	-0.0657	-0.1124	-0.0770
	(0.10)	(0.06)	(0.08)	(0.09)	(0.08)	(0.09)
log(Age)	0.0037	-0.0070	-0.0234	0.0178	0.0016	-0.0123
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Incorporated	0.0317	0.0188	0.0008	0.0657**	0.0017	0.0401
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Ext. rating	-0.0114	-0.0127	0.0247	-0.0374	-0.0127	0.0204
	(0.04)	(0.02)	(0.03)	(0.03)	(0.01)	(0.03)
Group	-0.0149	0.0013	0.0473	0.0286	0.0203	-0.0159
	(0.04)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)
Family	0.0196	0.0490	0.0491	-0.0083	0.0272	0.0638
	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)
Export	0.0575	-0.0176	0.0267	0.0568*	-0.0015	0.0121
	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)
Concentration (10% to <30%)	0.0242	-0.0101	0.0282	0.0459	0.0277*	0.0387
	(0.04)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Concentration (30% to <50%)	0.0033	-0.0246	0.0307	-0.0118	0.0304	0.0534
	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)
Concentration (50% +)	0.0286	-0.0130	-0.0095	0.0110	0.0357	0.0427
	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)
Savings bank	0.0016	0.0191	0.0382	0.0184	0.0131	-0.0021
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)
Cooperative	-0.0147	-0.0023	0.0029	0.0208	-0.0149	0.0187
	(0.03)	(0.02)	(0.03)	(0.03)	(0.01)	(0.03)
Landesbank	-0.0072	-0.0346	-0.0371	0.0414	0.0073	-0.0634*
	(0.05)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)
Others	0.0097	-0.0194	-0.0216	0.0284	0.0017	0.0226
	(0.04)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)
Industry	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.0522	0.0164	0.0441	0.1059	0.0183	0.0275
N	652	652	652	652	652	652

Note: The table shows OLS estimation results for regressions of dummy variables for different kinds of impairments of credit finance due to the financial crisis on the number of main bank relationships, a set of control variables for firm characteristics, bank classification dummy variables, and industry dummy variables; the baseline category for the main bank relationship dummy variables is *Main bank (0/3+)*; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Estimations of binary probit models

	(1) *Information*	(2) *Reduction*	(3) *Availability*
Main bank (1)	-0.1176***	0.0089	-0.0191
	(0.03)	(0.02)	(0.02)
Main bank (2)	-0.0654**	0.0302	-0.0090
	(0.03)	(0.03)	(0.02)
Firm char.	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Bank class.	Yes	Yes	Yes
Pseudo R^2	0.1638	0.1901	0.2087
N	643	497	576

	(4) *Interest*	(5) *Maturities*	(6) *Collateral*
Main bank (1)	-0.0538**	-0.0295**	-0.0626**
	(0.02)	(0.01)	(0.03)
Main bank (2)	-0.0595***	-0.0139**	-0.0314
	(0.02)	(0.01)	(0.02)
Firm char.	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Bank class.	Yes	Yes	Yes
Pseudo R^2	0.2589	0.3100	0.1658
N	617	347	586

Note: The table shows the estimated binary probit marginal effects of the number of main bank relationships on the probability of facing different kinds of impairment of credit finance due to the financial crisis; estimations include a set of control variables for firm characteristics, bank classification dummy variables, and industry dummy variables; the baseline category for the main bank relationship dummy variables is *Main bank (0/3+)*; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Robustness check for reverse causality

	(1) *Information*	(2) *Reduction*	(3) *Availability*
Main bank (1)	-0.1541***	0.0046	-0.0059
	(0.05)	(0.03)	(0.04)
Main bank (2)	-0.1001**	0.0097	-0.0028
	(0.05)	(0.03)	(0.04)
Firm char.	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Bank class.	Yes	Yes	Yes
Adj. R^2	0.0480	0.0066	0.0450
N	564	564	564

	(4) *Interest*	(5) *Maturities*	(6) *Collateral*
Main bank (1)	-0.1143**	-0.0599**	-0.0755*
	(0.05)	(0.03)	(0.04)
Main bank (2)	-0.1270***	-0.0555**	-0.0392
	(0.04)	(0.03)	(0.04)
Firm char.	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Bank class.	Yes	Yes	Yes
Adj. R^2	0.0907	0.0232	0.0116
N	564	564	564

Note: Firms for which at least one of the two most important bank relationships has been established in 2007 or later are dropped from the sample to rule out reverse causality concerns; the table shows OLS estimations results for regressions of dummy variables for different kinds of impairments of credit finance due to the financial crisis on the number of main bank relationships, a set of control variables for firm characteristics, bank classification dummy variables, and industry dummy variables; the baseline category for the main bank relationship dummy variables is *Main bank (0/3+)*; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Robustness check using bank balance sheet variables

	(1) *Information*	(2) *Reduction*	(3) *Availability*	(4) *Interest*	(5) *Maturities*	(6) *Collateral*
Main bank (1)	-0.1438***	0.0205	-0.0432	-0.1135***	-0.0495**	-0.0516
	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)
Main bank (2)	-0.0918**	0.0279	-0.0249	-0.1138***	-0.0420*	-0.0324
	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)
$log(Bank\ assets)_{max}$	-0.0034	-0.0094	0.0074	-0.0132	-0.0078	-0.0047
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$log(Bank\ assets)_{min}$	-0.0034	0.0108	-0.0111	0.0017	0.0039	-0.0018
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
$Bank\ equity_{max}$	-0.0016	0.0013	0.0025	-0.0060**	-0.0029*	-0.0030
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$Bank\ equity_{min}$	-0.0105	0.0019	-0.0025	-0.0017	-0.0002	0.0001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
$Bank\ liquidity_{max}$	0.0088	0.3078**	-0.1915	0.0745	0.0408	-0.1186
	(0.16)	(0.13)	(0.13)	(0.14)	(0.06)	(0.13)
$Bank\ liquidity_{min}$	0.0226	-0.2984*	-0.0763	0.1077	0.0119	-0.0246
	(0.22)	(0.18)	(0.17)	(0.21)	(0.08)	(0.19)
$Bank\ deposits_{max}$	-0.1229	0.0913	-0.1840	0.0361	0.0625	0.0647
	(0.15)	(0.10)	(0.12)	(0.14)	(0.08)	(0.13)
$Bank\ deposits_{min}$	0.1042	-0.0855	0.1323	-0.0711	-0.0479	-0.1046
	(0.17)	(0.10)	(0.14)	(0.16)	(0.09)	(0.16)
Firm char.	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.0606	0.0450	0.0505	0.0959	0.0186	0.0319
N	649	649	649	649	649	649

Note: The table shows OLS estimation results for regressions of dummy variables for different kinds of impairments of credit finance due to the financial crisis on the number of main bank relationships, a set of control variables for firm characteristics, bank balance sheet variables, and industry dummy variables; the baseline category for the main bank relationship dummy variables is *Main bank (0/3+)*; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix to Chapter 2

Table B.1: Purpose of negotiated credit

Purpose (line) (N=531)		*Purpose (loan)* (N=535)	
Working capital	72.32%	Substitution	47.57%
Investments	57.06%	Enhancement	64.74%
Others	12.24%	Rationalisation	28.17%
		R&D	10.09%
		Acquisitions	8.79%
		Foreign investment	6.92%
		Others	15.14%

Notes: The table provides the purposes of negotiated lines of credit and loans as reported by firms in the Ifo “Financing of the German Economy” survey. For every negotiated credit contract, firms could report more than one purpose.

Table B.2: F&F finance and firm characteristics

	F&F Business			*F&F Private*		
	Yes	No	$p > t$	Yes	No	$p > t$
N	128	956		62	1,009	
Perc.	11.81%	88.19%		5.79%	94.21%	
log(Empl)	4.57	5.02	0.01**	4.62	4.97	0.14
log(Assets)	8.61	9.25	0.009***	9.12	9.16	0.90
log(Age)	3.96	4.05	0.32	3.82	4.06	0.06*
Incorporated	61.72%	62.41%	0.88	68.85%	61.74%	0.27
Ext. rating	25.00%	21.40%	0.36	25.00%	21.46%	0.52
Customer (< 10%)	20.00%	19.91%	0.98	11.48%	20.24%	0.10
Customer (10% to <30%)	41.60%	45.05%	0.47	44.26%	45.02%	0.91
Customer (30% to <50%)	26.40%	18.96%	0.05*	24.59%	19.44%	0.33
Customer (50% +)	12.00%	16.08%	0.24	19.67%	15.31%	0.36
Export	83.33%	88.22%	0.12	79.03%	88.04%	0.04**
Group	21.88%	39.79%	0.000***	32.26%	38.10%	0.36
Family	91.41%	74.18%	0.000***	62.90%	76.05%	0.02**
Control	70.22	73.69	0.21	70.26	73.85	0.34
Operating owner	72.66%	59.49%	0.004***	59.68%	61.14%	0.82
Rating	216.21	198.46	0.04**	224.79	197.87	0.02**
Equity	21.89%	36.84%	0.000***	16.47%	36.45%	0.000***
Debt	33.64%	23.14%	0.003***	41.43%	23.54%	0.000***
Cash	9.61%	11.19%	0.30	8.16%	11.39%	0.12
Return (<3%)	54.31%	44.10%	0.04**	66.10%	44.05%	0.001***
Return (3 to <7%)	31.90%	32.65%	0.87	20.34%	32.79%	0.05*
Return (7 to <10%)	7.76%	14.32%	0.05*	8.47%	14.29%	0.21
Return (10% +)	6.03%	8.93%	0.30	5.08%	8.87%	0.32
Earlypay (0%)	4.10%	3.02%	0.52	6.56%	3.17%	0.16
Earlypay (<25%)	16.39%	14.55%	0.59	29.51%	13.60%	0.001***
Earlypay (25 to <50%)	7.38%	9.27%	0.49	6.56%	9.00%	0.52
Earlypay (50 to <75%)	14.75%	9.70%	0.08*	14.75%	10.02%	0.24
Earlypay (75% +)	57.38%	63.47%	0.19	42.62%	64.21%	0.001***

Notes: The table shows descriptive statistics for firm characteristics separately for F&F firms and non-F&F firms; p-values are reported for t-tests of the significance of the difference between respective groups of firms with respect to the firm characteristics; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Probit estimations for IV approach

	(1) Rejected (line)	(2) Rejected (loan)
Discretion	-1.183***	-0.916***
	(0.34)	(0.30)
log(Empl)	-0.305	-0.148
	(0.22)	(0.14)
log(Assets)	0.147	0.141
	(0.17)	(0.11)
log(Age)	-0.183	-0.133
	(0.14)	(0.13)
Incorporated	1.048***	0.409
	(0.35)	(0.27)
Ext. rating	-0.521	-0.292
	(0.34)	(0.31)
Customer (10% to <30%)	0.514	-0.644*
	(0.54)	(0.35)
Customer (30% to <50%)	-0.288	0.236
	(0.62)	(0.40)
Customer (50% +)	0.373	-0.478
	(0.66)	(0.45)
Group	0.478	0.125
	(0.36)	(0.34)
Family	0.037	-0.317
	(0.35)	(0.40)
Control	0.007	0.002
	(0.00)	(0.00)
Export	-0.067	0.347
	(0.44)	(0.36)
Operating	1.086***	0.973**
	(0.37)	(0.40)
Rating	0.001	-0.000
	(0.00)	(0.00)
Equity	-1.157*	-1.493**
	(0.61)	(0.64)
Long-term debt	-1.355**	0.056
	(0.61)	(0.56)
Cash	5.849***	2.829**
	(1.82)	(1.22)
Return (3% to <7%)	-1.357***	-0.690**
	(0.37)	(0.28)
Return (7% to <10%)	0.279	0.187
	(0.42)	(0.39)
Return (10% +)	-1.090	.
	(1.12)	.
Earlypay (0% to <25%)	-0.014	-0.277
	(0.51)	(0.51)
Earlypay (25% to <50%)	0.343	-0.316
	(0.58)	(0.58)
Earlypay (50% to <75%)	-0.827	-0.856
	(0.63)	(0.57)
Earlypay (75% +)	-1.232**	-1.109**
	(0.56)	(0.49)
Industry	Yes	Yes
N	235	231

Notes: The table shows results for two separate probit estimations; firm characteristics comprise all variables listed in Table 2.3; industry dummy variables are included based on two-digit WZ 2008 industry classifications; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Sample selection in OLS estimations

	(1) *F&F*	(2) *F&F*	(3) *F&F Business*	(4) *F&F Business*	(5) *F&F Private*	(6) *F&F Private*
Rejected (line)	0.053		0.007		0.073	
	(0.09)		(0.07)		(0.08)	
Rejected (loan)		0.176*		0.093		0.161*
		(0.09)		(0.06)		(0.08)
λ_1	-0.059		0.219		-0.095	
	(0.19)		(0.14)		(0.17)	
λ_2		0.124		0.137		-0.044
		(0.34)		(0.22)		(0.28)
Firm char.	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.1472	0.1114	0.1439	0.1318	0.1511	0.1350
N	286	307	276	302	281	303

Notes: The table shows results for six separate OLS estimations; firm characteristics comprise all control variables listed in Table 2.3; industry dummy variables are included based on the two-digit WZ 2008 industry classification; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Sample selection in IV estimations

	(1) *F&F*	(2) *F&F*	(3) *F&F Business*	(4) *F&F Business*	(5) *F&F Private*	(6) *F&F Private*
Rejected (line)	0.236		0.097		0.077	
	(0.20)		(0.16)		(0.17)	
Rejected (loan)		0.682**		0.495**		0.472*
		(0.31)		(0.22)		(0.25)
λ_1	-0.149		0.187		-0.145	
	(0.22)		(0.15)		(0.18)	
λ_2		-0.292		-0.308		-0.282
		(0.39)		(0.27)		(0.33)
Firm char.	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
F	35.16***	15.99***	34.85***	13.07***	32.47***	15.82***
MES	36.08***	19.43***	35.13***	17.03***	35.54***	18.43***
N	234	225	225	220	230	222

Note: The table shows results for separate IV estimations; λ is the inverse Mills ratio from probit estimations for the firms' decision to enter bank credit negotiations; firm characteristics comprise all control variables listed in Table 2.3; industry dummy variables are included based on the two-digit WZ 2008 industry classification; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; F is the F-statistic for the first-stage estimation testing the significance of the instrumental variable; MES provides the minimum eigenvalue statistic for the Stock and Yogo (2005) test with *** indicating the 10 percent confidence level and ** indicating the 15 percent confidence level.

Table B.6: OLS estimations for discouraged borrowers

	(1) *F&F*	(2) *F&F*	(3) *F&F Business*	(4) *F&F Business*	(5) *F&F Private*	(6) *F&F Private*
Discouraged (line)	0.285		0.267		0.061	
	(0.22)		(0.19)		(0.12)	
Discouraged (loan)		0.126		0.104		0.049
		(0.12)		(0.11)		(0.10)
λ_1	-0.162		-0.032		-0.147	
	(0.10)		(0.04)		(0.10)	
λ_2		0.151		0.119		-0.049
		(0.22)		(0.15)		(0.18)
Firm char.	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.1707	0.1893	0.2441	0.3084	0.1391	0.1347
N	314	306	314	302	310	301

Notes: The table shows OLS estimation results for separate regressions; λ is the inverse Mills ratio from probit estimations for the firms' decision **not** to enter bank credit negotiations; firm characteristics comprise all control variables listed in Table 2.3; industry dummy variables are included based on the two-digit WZ 2008 industry classification; robust standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix to Chapter 3

Appendix A: The Timing of the Treatment

As of 2008, the bank lending supply question in the Ifo Business Survey is asked on a monthly basis. This allows the exact specification of the treatment month t as the month in which the firm first reports restrictive bank lending after reporting normal or accommodating bank lending in previous surveys. From 2003 to 2008, however, the question was asked only twice a year, in March and August. If, for example, a firm reports normal or accommodating bank lending in March and restrictive bank lending in August, it is unclear whether the shift has occurred in August or in a month between March and August.

For our analysis, we assume the treatment month t to be the month right after the firm reports "normal" or "accommodating" bank lending the last time (which would be April in this example). This ensures that our control variables, which are drawn from t-1, are measured in a month in which the firm is definitely untreated and not already affected by constrained credit supply.

Alternatively, we could assume that the treatment occurs in the month in which the firm reports "restrictive" bank lending the first time. We have re-run our whole analysis based on this alternative specification and found our results to be insensitive to a variation of the timing assumption.

Appendix B: Discussion of Annual Frequency in Employment Data

In the Ifo Business Survey firms report their employment figures in November of each year. Two extreme alternatives are available to link annual employment growth rates to monthly data, each leading to severe problems in the estimation of credit supply-side effects on firm-level employment growth.

Alternative 1: Put the number of employees in November into every **subsequent** month until a new figure becomes available in November of the following year and calculate year-on-year growth rates in every month.

Problem: If a firm receives the treatment of constrained credit supply (as defined in Section 3.3.2) in October 2008, for example, the employment growth rate in *t+12* (i.e. October 2009) contains the growth between November 2007 and November 2008, which obviously captures a large extent of what is in fact pre-treatment growth, but only one month of post-treatment growth. Estimated credit-supply side effects on firm-level employment growth would therefore be potentially biased.

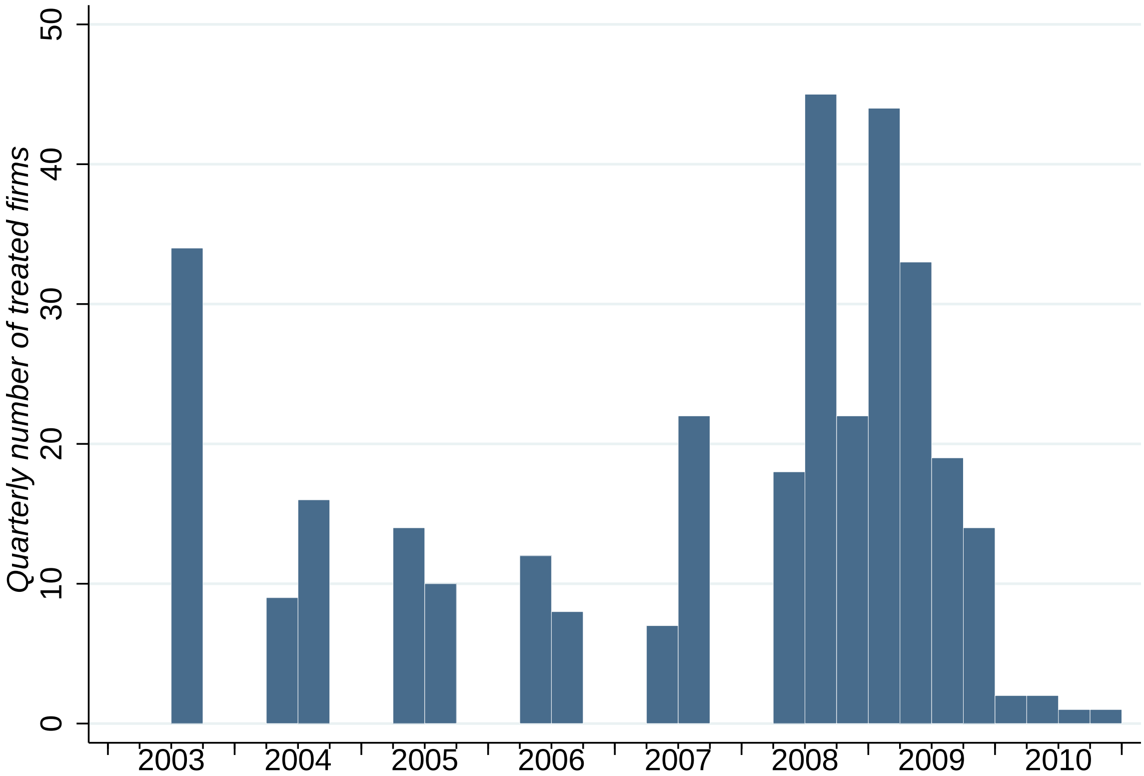
Alternative 2: Put the number of employees in November into every **previous** month until an old figure in November of the previous year appears and calculate year-on-year growth rates in every month.

Problem: If a firm receives the treatment of constrained credit supply (as defined in Section 3.3.2) in December 2008, for example, the employment growth rate in *t+12* (i.e. December 2009) contains the growth between November 2009 and November 2010, which captures only a very small part of the post-treatment year. Estimated credit-supply side effects on firm-level employment growth would again be potentially biased.

Our solution: We put the number of employees in November into the five previous and six subsequent months and calculate year-on-year growth rates in every month. For some observations, this may still not capture post-treatment employment growth precisely, but this intermediate solution is the most conservative way to deal with the problem.

Appendix C: Empirical Appendix

Figure 3.1: Number of treated firms over time



Notes: The graph shows the number of firms that are treated in every quarter; a firm is treated if it reports "restrictive" bank lending after having reported "normal" or "accommodating" bank lending in the previous 12 months; from 2003 to 2008, treatments can occur only in the second and third quarter because firms are surveyed on bank lending only in March and August and the treatment is assumed to occur in the month right after "normal" or "accommodating" bank lending was last reported.

Table C.1: Balancing properties for the two matching procedures

	(1) ($N_R = 223$; $N_U = 1,351$)			(2) ($N_R = 141$; $N_U = 806$)		
t-1	$\bar{X}_R - \bar{X}_U$	$p > t$	Bias	$\bar{X}_R - \bar{X}_U$	$p > t$	Bias
Firm size						
log(Empl)	-9.5	0.40	-7.82	-4.7	0.74	-3.94
log(Assets)	-9.4	0.54	-5.70	-5.6	0.77	-3.48
Balance sheet data						
Equity	-1.5%	0.47	-6.24	-0.7%	0.80	-2.88
Cash	-0.7%	0.54	-5.24	-1.0%	0.54	-7.74
Long-term debt	1.7%	0.36	9.07	1.4%	0.54	7.73
Short-term debt	1.1%	0.59	3.31	0.5%	0.85	1.48
Cash flow	-0.5%	0.71	-3.72	0.0%	1.00	0.07
ROA	0.4%	0.91	0.99	0.2%	0.97	0.42
Interest Coverage	-0.49	0.89	-1.11	-4.07	0.36	-9.53
Current business situation						
State (+)	-1.5%	0.71	-3.49	-0.2%	0.96	-0.58
State (-)	6.8%	0.10	15.87	2.3%	0.66	5.42
Orders (+)	-3.2%	0.29	-9.81	-1.9%	0.58	-5.96
Orders (-)	3.2%	0.48	6.77	2.8%	0.63	5.78
Production (+)	2.2%	0.45	6.53	-1.6%	0.60	-4.69
Production (-)	0.6%	0.88	1.48	0.2%	0.98	0.37
Short-time	3.1%	0.30	8.86	-1.8%	0.60	-5.13
Export	-1.8%	0.55	-5.52	0.2%	0.95	0.68
Future expectations						
Business expect (+)	-0.3%	0.94	-0.67	0.2%	0.96	0.56
Business expect (-)	2.8%	0.51	6.66	2.4%	0.66	5.60
Empl expect (+)	-1.3%	0.54	-5.53	-0.6%	0.76	-2.71
Empl expect (-)	3.4%	0.39	8.56	0.8%	0.86	2.03
Headcount (+)	-0.9%	0.70	-3.68	0.2%	0.95	0.72
Headcount (-)	6.4%	0.13	15.04	4.2%	0.42	9.87
Short-time expect	2.3%	0.52	5.91	-2.5%	0.59	-6.17

Notes: Sample (1) is derived from ten nearest neighbours matching on firm size and balance sheet variables only; Sample (2) is derived from ten nearest neighbours matching on firm size, balance sheet variables, current business situation, and future expectations; differences in means between restricted and unrestricted firms are reported for pre-treatment control variables $B_{i,t-1}$, $C_{i,t-1}$, and $F_{i,t-1}$; p-values are reported for t-tests with H_0: $\bar{X}_R = \bar{X}_U$; bias statistics are calculated as $(\bar{X}_R - \bar{X}_U)/\sqrt{\frac{\sigma_R^2+\sigma_U^2}{2}}$ following Rosenbaum and Rubin (1985).

Table C.2: Alternative OLS estimations using $Slowdown_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	0.0887***	0.0737***	**0.015*** **
	(0.026)	(0.026)	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Partly**	
Future expectations	No	**Partly**	
Month	Yes	Yes	
Industry	Yes	Yes	
Adj. R^2	0.1683	0.1895	
N	5,503	5,503	

Notes: The table provides results for OLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Restricted_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; "Firm size", "Balance sheet data", "Current situation", and "Future expectations" are reduced sets of control variables (excluding general appraisals) as explained in Section 3.5.1; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples contain only those observations for which all control variables of Estimation (2) are available; the third column provides the difference between the two estimated effects; its significance is tested using a t-test with H_0: $\beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Alternative PSM estimations using $Slowdown_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	5.74%**	3.16%	**2.58%**
	(0.0225)	(0.0241)	
Industry	Yes	Yes	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Partly**	
Future expectations	No	**Partly**	
p>t	0.01	0.19	
Upper bound	9.44%	7.12%	
Lower bound	2.03%	-0.80%	
Treated obs.	245	214	
Matching obs.	1,516	1,298	

Notes: The table provides results for WLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Restricted_{i,t}$; in Estimation (1), weights are derived from PSM based on firm size and balance sheet data in *t-1*; in Estimation (2), weights are derived from PSM based on firm size, balance sheet data, and reduced sets of control variables (excluding general appraisals) for current business situation and future expectations as explained in Section 3.5.1; industry dummy variables based on the two-digit WZ 2008 industry classification are also included in all PSM estimations; p-values are reported for a t-test of significance the estimated treatment effect; the significance of the difference between the two estimated effects in the third column cannot be tested; upper and lower bounds are reported for the 95 percent confidence interval; standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: OLS estimations using $Slowdown\_avg_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	0.0545***	0.0477***	**0.068****
	(0.010)	(0.010)	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
Month	Yes	Yes	
Industry	Yes	Yes	
Adj. R^2	0.2258	0.3041	
N	5,193	5,193	

Notes: The table provides results for OLS estimations of $Slowdown\_avg_{i,t+12}$ on the treatment status $Restricted_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; "Firm size", "Balance sheet data", "Current situation", and "Future expectations" are sets of control variables as listed in Table 3.1; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples contain only those observations for which all control variables of Estimation (2) are available; the third column provides the difference between the two estimated effects; its significance is tested using a t-test with H_0: $\beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: PSM estimations using $Slowdown\_avg_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	3.78%***	2.78%*	**1.01%**
	(0.0116)	(0.0149)	
Industry	Yes	Yes	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
p>t	0.001	0.06	
Upper bound	5.69%	5.23%	
Lower bound	1.88%	0.33%	
Treated obs.	223	141	
Matching obs.	1,351	806	

Notes: The table provides results for WLS estimations of $Slowdown\_avg_{i,t+12}$ on the treatment status $Restricted_{i,t}$; in Estimation (1), weights are derived from PSM based on firm size and balance sheet data in *t-1*; in Estimation (2), weights are derived from PSM based on firm size, balance sheet data, current business situation, and future expectations in *t-1*; industry dummy variables based on the two-digit WZ 2008 industry classification are also included in all PSM estimations; p-values are reported for a t-test of significance of the estimated treatment effect; the significance of the difference between the two estimated effects in the third column cannot be tested; upper and lower bounds are reported for the 95 percent confidence interval; standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: OLS estimations using $\Delta Empl_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	-0.0180***	-0.0152**	**0.0025****
	(0.006)	(0.006)	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
Month	Yes	Yes	
Industry	Yes	Yes	
Adj. R^2	0.0598	0.0849	
N	5,041	5,041	

Notes: The table provides results for OLS estimations of $\Delta Empl_{i,t+12}$ on the treatment status $Restricted_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; “Firm size”, “Balance sheet data”, “Current situation”, and “Future expectations” are sets of control variables as listed in Table 3.1; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples contain only those observations for which all control variables of Estimation (2) are available; the third column provides the difference between the two estimated effects; its significance is tested using a t-test with H_0: $\beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: PSM estimations using $\Delta Empl_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	-1.08%*	0.35%	**-1.43%**
	(0.0059)	(0.0078)	
Industry	Yes	Yes	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
p>t	0.07	0.65	
Upper bound	-0.11%	1.64%	
Lower bound	-2.04%	-0.94%	
Treated obs.	208	130	
Matching obs.	1,295	773	

Notes: The table provides results for WLS estimations of $\Delta Empl_{i,t+12}$ on the treatment status $Restricted_{i,t}$; in Estimation (1), weights are derived from PSM based on firm size and balance sheet data in *t-1*; in Estimation (2), weights are derived from PSM based on firm size, balance sheet data, current business situation, and future expectations in *t-1*; industry dummy variables based on the two-digit WZ 2008 industry classification are also included in all PSM estimations; p-values are reported for a t-test of significance of the estimated treatment effect; the significance of the difference between the two estimated effects in the third column cannot be tested; upper and lower bounds are reported for the 95 percent confidence interval; standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: OLS estimations using $Empl\_Decrease_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	0.0937***	0.0749***	**0.0188*** **
	(0.027)	(0.026)	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
Month	Yes	Yes	
Industry	Yes	Yes	
Adj. R^2	0.0881	0.1263	
N	5,141	5,141	

Notes: The table provides results for OLS estimations of $Empl\_Decrease_{i,t+12}$ on the treatment status $Restricted_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; "Firm size", "Balance sheet data", "Current situation", and "Future expectations" are sets of control variables as listed in Table 3.1; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples contain only those observations for which all control variables of Estimation (2) are available; the third column provides the difference between the two estimated effects; its significance is tested using a t-test with H_0: $\beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: PSM estimation using $Empl\_Decrease_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	6.23%**	1.24%	**4.99%**
	(0.0250)	(0.0324)	
Industry	Yes	Yes	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
p>t	0.01	0.70	
Upper bound	10.34%	6.56%	
Lower bound	2.13%	-4.08%	
Treated obs.	221	136	
Matching obs.	1,340	792	

Notes: The table provides results for WLS estimations of $Empl\_Decrease_{i,t+12}$ on the treatment status $Restricted_{i,t}$; in Estimation (1), weights are derived from PSM based on firm size and balance sheet data in *t-1*; in Estimation (2), weights are derived from PSM based on firm size, balance sheet data, current business situation, and future expectations in *t-1*; industry dummy variables based on the two-digit WZ 2008 industry classification are also included in all PSM estimations; p-values are reported for a t-test of the significance of the estimated treatment effect; the significance of the difference between the two estimated effects in the third column cannot be tested; upper and lower bounds are reported for the 95 percent confidence interval; standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Treated firms in different matched samples

	(1)	(2)	
	$\bar{X}^B$	$\bar{X}^{BCF}$	$p > t$
Production (t+12)			
Slowdown	70.0%	71.6%	0.73
Slowdown_avg	23.4%	24.1%	0.80
Employment (t+12)			
$\Delta Empl$	-1.0%	-0.3%	0.56
Empl_Decrease	45.2%	41.9%	0.54

Notes: The table provides the means of all outcome variables for treated firms in the matched sample when matching on firm size and balance sheet variables in *t-1* and the matched sample when matching on all variables in *t-1*; p-values are provided for a two-group mean comparison test with H_0: $\bar{X}_B = \bar{X}_{BCF}$.

Table C.11: Financial crisis OLS estimations using $Slowdown_{i,t+12}$

Restricted	0.0731**	0.0587*	**0.0144***
	(0.032)	(0.031)	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
Month	Yes	Yes	
Industry	Yes	Yes	
,			
Adj. R^2	0.2086	0.2552	
N	3,315	3,315	

Notes: The table provides results for OLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Restricted_{i,t}$ and different sets of pre-treatment control variables in the subsample as of July 2007; standard errors (reported in parentheses) are clustered at the firm-level; "Firm size", "Balance sheet data", "Current situation", and "Future expectations" are sets of control variables as listed in Table 3.1; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples contain only those observations for which all control variables of Estimation (2) are available; the third column provides the difference between the two estimated effects; its significance is tested using a t-test with H_0: $\beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.12: Financial crisis PSM estimation using $Slowdown_{i,t+12}$

	(1)	(2)	$\beta_1^B - \beta_1^{BCF}$
Restricted	4.97%*	3.84%	**1.14%**
	(0.0260)	(0.0306)	
Industry	Yes	Yes	
Firm size	Yes	Yes	
Balance sheet data	Yes	Yes	
Current situation	No	**Yes**	
Future expectations	No	**Yes**	
p>t	0.06	0.21	
Upper bound	9.25%	8.87%	
Lower bound	0.70%	-1.20%	
Treated obs.	160	116	
Matching obs.	928	690	

Notes: The table provides results for WLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Restricted_{i,t}$ in the subsample as of July 2007; in Estimation (1), weights are derived from PSM based on firm size and balance sheet data in *t-1*; in Estimation (2), weights are derived from PSM based on firm size, balance sheet data, current business situation, and future expectations in *t-1*; industry dummy variables based on the two-digit WZ 2008 industry classification are also included in all PSM estimations; p-values are reported for a t-test of significance of the estimated treatment effect; the significance of the difference between the two estimated effects in the third column cannot be tested; upper and lower bounds are reported for the 95 percent confidence interval; standard errors are reported in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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