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The Credit Channel and the Role of Monetary Policy Before, During and After the Global Financial Crisis

A Micro Data Approach to the Analysis of Bank-firm Relationships

Franz R. Hahn, Werner Hölzl (WIFO), Claudia Kwapil (OeNB)

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Abstract

At the center of this study is the identification of the empirically and, thus, policy relevant causes of financial contraction. Our focus is on identifying the "pure" credit-limiting supply effect in order to assess its importance as it is claimed in the theoretical literature.

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Contents

1 Introduction

The most striking element of the recent financial market crisis was the sudden and unexpected breakdown of the financial system in the most advanced economies. In 2008 driven by presumably endogenous forces a critical mass of banks stopped liquidity and credit creation to both the financial sector and the real sector aggravating recessions in the world's leading economies.

The aim of this study is to make a contribution towards understanding the underlying forces at work causing a large number of banks to malfunction in their capacity as lenders (not only to their fellow-banks but, most importantly, also to their private business clientele). We argue that the unambiguous identification of the market forces driving credit supply and credit demand is key to get a thorough understanding of the whys and wherefores of credit constraints and credit crunches particularly imposed upon businesses.

In contrast to the theoretical literature empirical contributions on credit constraints are limited. The latter is mainly due to lack of information. Our unique, interlinked micro-dataset is drawn from banks and business firms of the Austrian economy and covers the pre-crisis period from 2004 onwards, the crisis period 2008 and 2009 and the post-crisis period from 2010 onwards (until 2013). This allows for an in-depth analysis of the complex relationship between banks and their borrowing business customers at the individual level under changing environments unfolding over a time-span of a decade. The compilation of this high-quality micro-dataset is made possible by a joint venture of the Austrian Institute of Economic Research (WIFO) and the Austrian National Bank (OeNB) in close cooperation with the Kreditschutzverband von 1870 (KSV 1870), the country's largest creditors' protection association. This unique database provides the basis for resolving the so-called "missing data problem" that has as yet impeded the distinct determination of whether the contraction of credit is due to shrinking loan supply or shrinking loan demand.

Hence, at the center of this research project is the identification of the empirically and, thus, policy relevant causes of financial contraction. Our focus is to identify the "pure" credit supply effect in order to assess its importance as it is claimed in the theoretical literature.

This study enriches the scientific discussion in applied monetary economics by making contributions to the following areas:

- Resolving the identification problem in applied credit market analysis by applying advanced methods of the matching approach. To the best of our knowledge our research project is the first that uses matching techniques to resolve the identification problem in conjunction with credit supply and credit demand. Most importantly, advanced matching techniques applied to such a rich bank-firm dataset allow us to identify the causal effect of credit constraints on businesses. By comparing the effects of credit constraints on businesses that were exposed to credit limits to those that were not exposed to credit limits but were otherwise identical to the credit-constrained firms prior to the treatment, the selection problem can be solved. The average treatment on the treated is computed as the difference in outcomes between these two groups. The motivation for the use of matching techniques in this research project is extensively laid out in Section 5.

- The in-depth analysis of the relationship between banks and private businesses at the individual level before, during and after the recent global financial crisis provides valuable new information for monetary authorities. They need this information to effectively steer the credit cycle. The credit channel is at the center of modern monetary policy making. It hardly needs mentioning that a new regulatory and policy view on the working of this important monetary transmission channel is overdue.
- Austria is a downright bank-based economy. Thus, an in-depth analysis of limitations in credit supply based on firm-level data stemming from a country like Austria carries weight with corresponding developments in continental Europe, since the great majority of these economies is likewise heavily funded by banks. Beyond that, the great majority of Austrian banks are relationship banks. The overwhelmingly small to medium-sized banks serve primarily their local markets allowing them to develop a close relationship with their clientele, particularly with their credit customers (see for a detailed discussion, among others, Hahn, 2007A, 2007B, 2008, and 2014). This "institutional feature" of the Austrian banking system makes Austria an ideal test field for studying the credit channel of monetary policy.

The organization of the study is structured as follows: Section 2 gives an overview of the study, Section 3 reviews the literature directly and indirectly related to our research work, Section 4 presents the dataset, Section 5 introduces the methodological framework of the empirical analysis, Section 6 discusses the estimation procedure applied, Section 7 presents the major empirical findings gained, and Section 8 concludes.

2 Study Overview

Theoretical work in economics overwhelmingly suggests that credit supply is critical in explaining the dynamics of the business cycle (e. g., *Bernanke and Gertler*, 1989; *Holmström and Tirole*, 1997; *Kiyotaki and Moore*, 1997; *Diamond and Rajan*, 2005). The so-called "financial accelerator" literature argues that when an economy is hit by an adverse shock the concurrent deterioration of financial conditions leads to credit constraints which in turn amplify the worsening of the economic situation.

In contrast to the theoretical literature empirical contributions on credit constraints appear to be limited in scope. *Degryse et al.* (2009) argue that "[…] with a few notable exceptions (e. g. *Berger and Udell*, 1994; *Chakravarty and Scott*, 1999), most of the empirical work […] focuses on the availability of credit measured by loan amount […] for firms with particular and observable characteristics, not on the likelihood otherwise similar firms will be denied credit" (*Degryse et al.*, 2009, p.123). This criticism is, by now, a widely shared perception within the research community.

The reason for limited scope in existing empirical work is a lack of information. Applied credit-limitation analysis is almost always impaired by the existence of the so-called "missing-data problem". It severely obstructs the determination of whether the contraction of credit is due to shrinking loan supply or shrinking loan demand[1]. The understanding of the pro-cyclicality of bank-credit growth remains limited as long as these two causes cannot be separated in empirical analysis. The identification of the causes of financial contraction becomes particularly important when it comes to the assessment of implications of credit shortening for social welfare and monetary policy making, respectively.

Hence, the aim of our investigation is to study empirically the effect of credit constraints in due consideration of the causation identification problem. Put differently, our focus is to identify the "pure" credit supply effect in order to assess its importance as it is claimed in the theoretical literature.

The empirical analysis is based on a unique, interlinked micro-dataset drawn from banks and business firms of the Austrian economy. This data-set allows for the distinction between demand and supply effects in times of credit contraction. The sample covers the period from 2004 to 2013.

Beyond, value added comes from the fact that Austria is a downright bank-based economy. Thus, an in-depth analysis of limitations in credit supply based on firm-level data stemming from a country like Austria carries weight with corresponding developments in Continental Europe, since the great majority of these economies is likewise heavily funded by banks. Beyond that, the great majority of Austrian banks are downright relationship banks. The

[1] According to *Udell* (2009) "economists generally define a credit crunch as a significant contraction in the supply of credit reflected in a tightening of credit conditions".

overwhelmingly small to medium-sized banks serve primarily their local markets allowing them to develop a close relationship with their clientele (see for a detailed discussion, among others, *Hahn*, 2007A, 2007B, 2008, and 2014). This particular applies to their credit customers. This "institutional feature" of the Austrian banking system makes Austria an ideal test field for studying the credit channel mechanism of monetary policy on condition that the banks involved are relationship banks.

The center of this study is the methodological framework capable of coping with causation identification in applied credit contraction analysis. Contrary to the most widely used approach in economics to identify causal effects – the instrumental variable technique – we base our causation analysis approach on information gained from a "quasi-natural experiment"[2]. It provides sufficient (causal) information on key credit-related variables that can be assumed to adhere to the requirement of treatment (causation) randomness.

Historical economic events bearing qualifications of a natural experiment are rare. We regard the global financial crisis in the aftermath of the Lehman collapse in 2008 to come close to such a rare "natural incidence" revealing underlying causation identification of interest. Under the conditions of the "2008 crisis", by generating classifiable variation in credit-related variables, we hold that causal determination of the individual effects of these variables is, in principle, feasible, in the presence of heterogeneity.

Hence, our analysis focuses on the following questions: (1) Causal identification of the impact of credit supply contraction on key business performance parameters (i. e., employment, output) during and after the crisis years 2008/09. This task will be accomplished by setting up a treatment group (firms reporting on credit limitations) and a control group (firms sharing key characteristics with firms of the treatment group prior to and during the treatment phase, but reporting no credit limitations) and by comparing respective outcomes during and after the treatment phase[3]; (2) Identification of characteristics causing banks to impose credit limits on business clients (particularly, in times of global financial stress). This task will be accomplished by conducting an in-depth analysis of the differences between the characteristics of banks, reported to have imposed credit limits, and banks, not having restricted credit.

[2] Basically, there are four avenues to resolve causation identification in social sciences: experiments (researcher generates variation), natural experiments (nature generates variation), instrumental variables (variables known provide variation), and econometric identification (identification of variation cause due to testable assumptions).

[3] Comparing outcomes of the treatment group with those of the control group prior to and after treatment, by applying "difference in differences", captures the supply effect only, since firms are identified to be otherwise identical.

3 Current Status of Applied Limited Credit Analysis – An Overview

3.1 Contributions not related to the Financial Crisis

Early contributions in this field that try to identify loan supply shifts focus on separating banks by their differential balance sheet characteristics that are tied to a bank's ability to supply loans, but are independent of loan demand shocks. *Kashyap and Stein* (1995) separate banks according to asset size. They find that banks' loan growth in the smallest asset category is most responsive to shocks. Although this finding is compatible with a bank lending channel, the authors admit that this test may not be stringent enough to separate loan supply from demand effects. Small banks may have a larger proportion of loans to small businesses whose demand tends to be pro-cyclical. A summary of additional evidence from various loan markets and various countries is found in *Kashyap and Stein* (1997).

Peek and Rosengren (1995, 2000) as well as *Kishan and Opiela* (2000, 2006) extend the above literature by specifying an additional differentiating characteristic, namely the bank capital ratio along with asset size. They find that low-capitalized banks respond more strongly to shocks than well-capitalized banks. However, the fundamental problem that differences in bank capital are also likely to be associated with differences in borrowers quality is not solved. Consequently, differences in credit growth may again just reflect differences in firms' conditions rather than in banks' conditions.

All these studies on cross-sectional differences in the responsiveness of bank credit to shocks refer to the U.S. *Altunbaş et al.* (2002) and *Ehrmann et al.* (2003) find that lending of low-capitalized banks also reacts more strongly to shocks in the Euro area. However, their results are not significant at conventional levels for the main European countries. *Gambacorta and Mistrulli* (2004) as well as *Gambacorta* (2005), using a comprehensive sample of Italian banks, show – in line with the U.S. literature – that the decrease in lending is lower for well-capitalized banks that are perceived as less risky by the market. Liquid banks can better protect their loan portfolio against shocks by drawing down cash and securities. Moreover, *Gambacorta and Marquez-Ibanez* (2011) document that not only a bank's capital position (or more generally, the bank risk as perceived by financial markets), but also other bank characteristics (e. g. short-term funding, securitization activity, fee-based revenues) have an impact on the credit supply.

However, all these contributions use cross-sectional bank-level data and their analysis suffers from the missing link between borrowers and banks. *Jiménez et al*. (2012A) put it like that, "[…] any analysis based only macro data or bank-level data may suffer from an omitted-variables problem" (see *Jiménez et al*., 2012A, p. 2302).

A completely different avenue in research is followed by *Degryse and Ongena* (2004, 2008). They widen the view of credit limitations by exploring to what extent location may be affecting the availability of bank loans (geographical rationing). They find that retail banking remains to a large extent local, meaning that pricing and availability of credit mainly depends on local market conditions. This is especially true for Europe, where different legal systems and languages remain important barriers for cross-border retail banking.

Khwaja and Mian (2008) as well as *Schnabl* (2012) push the research frontier forward, because they manage to link bank-level data with firm-level data. However, they do so for developing countries, namely Pakistan and Peru, while our focus will be on an industrial country. Since the origins and main repercussions of the financial crisis have been in industrial countries, focusing on advanced economies as we do is, in our mind, the proper choice. *Khwaja and Mian* (2008) apply a new empirical methodology for identifying the bank lending channel by focusing on firms borrowing from multiple banks, where the banks differ in their exposure to a liquidity shock (e. g. the nuclear tests of Pakistan in 1998). They analyze how the same firm's loan growth from one bank changes relative to another more affected bank. This within-firm comparison should fully absorb firm-specific changes in credit demand. *Schnabl* (2012) applies the same empirical strategy and uses data from Peruvian banks and firms for analyzing the effect of a liquidity shock stemming from the 1998 Russian default via banks to Peruvian firms. Both papers show that firms borrowing from banks that are less affected by the liquidity shock are less likely to suffer from credit constraints. Moreover, *Schnabl* (2012) finds that firms which are less likely to face credit restrictions are also less likely to show real effects.

3.2 Contributions on the Recent Financial Crisis

Based on information on loan rejection rates over the current financial crisis, *Puri et al.* (2011) show that some of German savings banks (Sparkassen), although acting only locally and following a version of "narrow banking", had to decrease their credit supply, because their capital position suffered during the crisis. Other papers try to exploit firm-level and sectoral-level data. However, also these contributions cannot distinguish "pure" supply factors from other factors: *Dell'Ariccia et al.* (2008) identify loan supply factors by exploiting sectoral differences in dependence on the banking sector; *Borensztein and Lee* (2002) use information at the firm-level and proxy credit demand with some observable balance sheet items (e. g., net investment and cash flow).

Rottmann and Wollmershäuser (2013) try to circumvent the identification problem by applying a micro-data approach that uses information on the credit supply behavior of banks obtained from a regular survey among firms. In this survey firms are asked to give their perception of the current willingness of banks to extend credit to businesses. These survey answers are used to create a credit crunch indicator that represents (perceived) shortages in the credit supply which can neither be explained by changes in the quality of potential borrowers, nor by variations in the refinancing costs of banks. While being a promising approach, it suffers from the weakness of the survey questions on which it is based. Firms are only asked to give their perception of the willingness of banks to extend credit to businesses in general. They are not asked whether they experience financing problems themselves (which, in contrast, is done in our WIFO survey). Consequently, the credit crunch indicator represents only perceived shifts in the supply of loans and not true shifts in the supply of loans.

This shortcoming is not present in the micro-data approach followed by *Popov and Udell* (2012), *Brown et al.* (2011) as well as *Ongena and Popov* (2011) who use firm-level information on loan rejection rates and even on firms that are discouraged from applying for a loan. This information is taken from a firm-level survey conducted in Central and Eastern European countries. They show that only 1/3 of credit-constrained firms are firms that apply for a loan and are rejected, while 2/3 of constrained firms are firms that need a loan but are discouraged from applying. With this micro-data at hand they investigate e. g. how the financial distress during the financial crisis was transmitted to local economic conditions in Central and Eastern Europe. However, their analysis also suffers from the missing link between borrowers and lenders. This missing link is proxied e. g. by the location of the firm and the banks.

Another strand of literature proposes to analyze individual loan data together with both, firm and bank characteristics. E. g. *Albertazzi and Marchetti* (2010) as well as *De Mitri* (2010) use Italian data on outstanding loans merged with data on corresponding balance sheet indicators of the firms' quality. Since the compilation of a micro-data set with bank-firm relationships is a challenging task, *Albertazzi and Marchetti* (2010) are not able to analyze the evolution of loan supply over time and only provide a cross-sectional analysis for a specific point in time after the collapse of Lehman. *Bofondi et al.* (2013) overcome this problem. Also using the Italian Credit Register (CR), they analyze the effect of the sovereign debt crisis in 2011 on credit creation in Italy. Therefore, they compare the pre-crisis (first half of 2011) and the crisis (second half of 2011) patterns of credit supplied and find that Italian banks tightened credit supply more than foreign banks.

Perhaps most convincingly, *Jiménez et al.* (2012A) go one step further by analyzing individual bank-firm relationships in Spain until December 2008. They find that a worse economic environment and tighter monetary conditions reduce loan granting, especially to firms and from banks with lower capital and lower liquidity ratios. Moreover, *Jiménez et al.* (2012B) focus on the financial crisis, as their sample covers years from 2002 up to 2010. They find that while heterogeneity in bank balance-sheet strength does not determine loan granting in good times, it does so in crisis times. In contrast, firm heterogeneity in balance-sheet strength determines the probability in loan granting in good as well as in crisis times. The Credit Register of the Banco de Espana (CIR) was the first data source – we are aware of – that allowed for analyzing credit relations on a bank-firm level over time. Thus, it marks a big step forward in the empirical literature that tries to separate demand from supply effects. However, this database suffers from the fact that it only covers loan applications. *Jiménez et al.* (2012A, 2012B, 2014) only observe those potential borrowers that "seriously approach the bank to obtain credit". However, as argued by *Brown et al.* (2011) many firms are credit constrained not only because their applications for loans are rejected, but also because firms are discouraged to approach a bank, e. g. because they assess the probability of receiving a loan as extremely small. All these firms are missing in the analysis of *Jiménez et al.* (2012A, 2012B).

While *Albertazzi and Marchetti* (2010) as well as *Jiménez et al.* (2012A, 2012B) link banks' health to loans received by firms (on a bank-firm level), they are missing the last step in the bank lending channel, the effect credit restrictions have on firms' output. This gap is filled by *Amiti and Weinstein* (2011) and *Ongena et al.* (2012), who trace the effect from banks cutting back on credit supply to firms' real performance. *Amiti and Weinstein* (2011) find that the health of financial institutions is an important determinant of firm-level exports in Japan. *Ongena et al.* (2012) show that internationally-borrowing banks and foreign owned banks cut back lending to firms in Eastern and Southeastern Europe by more than domestic banks. However, the broad picture the paper gives is that firms' facing credit restrictions are not performing significantly worse than other firms.

Our approach is closely related to *Amiti and Weinstein* (2011) as well as to *Ongena et al.* (2012) in the sense that we also aim for tracing the shock from the financial crisis 2008/09 to banks' credit supply and further to the real performance of firms. Moreover, we will investigate the firm-bank relationship at individual levels prior to the crisis (2004 to 2007), during the crisis (2008/09) and also after the crisis (2010 to 2013). Finally, our research project is different in its empirical strategy. As most of the papers cited above, we use the financial crisis as a "quasi-natural experiment" to cope with causation identification. We augment, however, the difference-in-difference analysis with advanced matching techniques (see Section 6).

4 Data

To qualify as a proper setting for a natural credit-related experiment, the data field generated by the financial crisis in 2008 had yet to be supplemented by an ancillary, but critical piece of information that happened not to be unveiled (at least, directly) by nature. This "missing prerequisite" has been amended by the Austrian Institute of Economic Research (WIFO) by surveying non-financial firms[4]. This survey collects firms' records of whether (local) banks impose credit limits on them or keep funding going unconstrained. More than 11,200 non-financial companies had been approached, more than 1,500 of which completed and returned the questionnaire. A third of the respondents reported respective exposures to credit constraints such as 'bank financing not granted', 'amount of bank credit lowered', 'bank financing not extended', 'credit line reduced', or 'collateral requirement increased' during the course of the global financial stress and afterwards. Accordingly, two thirds of the respondents reported that no financial restrictions were imposed on them by their local banks (that is, full funding provided) during and after the financial crisis. The survey was pre-tested 2014 and conducted during 2015. These responses are used to set up the treatment indicator (that is, experiencing credit limits 'yes' or 'no') in the analysis. In addition, information on company age, sales and employment for 2008, 2010, and 2013 at the firm level have been gathered.

Since, for this research project, WIFO has a cooperation agreement on unconditional business data-sharing with the "Kreditschutzverband von 1870" (KSV 1870), we are in the favorable position to supplement the WIFO credit survey data with business data[5]. Most importantly, we got unlimited access to the KSV 1870 credit ratings of the companies covered in the WIFO-survey since 2004 until 2013. This provides us with firm-level data on borrowers' credit risk for the more than 1,500 non-financial Austrian companies under study.

The KSV 1870 database not only contains firm-level information on the creditworthiness of non-financial businesses (and on company age, export- and import-orientation etc.), but also includes bank connections of the companies covered in the WIFO survey. Moreover, the KSV 1870 reports the name and the number of local banks a company is related with and whether a bank connection is classified as 'minor' or 'major'. In order to single out the local bank of a firm that is most likely the main provider of external funding for its investment projects and/or working capital, we exclusively focus on non-financial companies with a single major bank connection. Moreover, we only include non-financial companies that are small to medium-sized and, hence, unfit for the capital markets. Our set-up requires that the local banks under study have not been merged with or have not been taken over by other banks since 2000, and that both the company's and the bank's headquarters are located either in the same district or in neighboring districts. Due to these data design requirements, a

[4] For details concerning organization, structure and findings of the survey, see Appendix B.

[5] The cooperation agreement with KSV 1870, worked out by legal experts, has been confirmed to be in full compliance with standing data protection regulations as stipulated by Austria's data protection acts.

detailed analysis of firm-bank relationships on an individual level can be conducted without running too high a risk of violating key principles of the potential output approach such as the so-called stable unit-treatment value assumption (*SUTVA*), the backbone of the methodological framework of observational studies introduced in the following chapter.

Bank data stem from the Austrian Nationalbank. The data set covers the entire Austrian banking sector at the level of individual banks. The bank data are primarily extracted from non-consolidated income statements and balance sheet data.

The imposition of all these requirements results in a sample consisting of 948 non-financial companies that are serviced by 235 local banks classified as their 'major banks'. Almost a third (to be exact, 294) of the non-financial companies report that they have experienced credit-limitations by their house banks (Hausbank) during the financial crises and thereafter. We assume credit limitations, if at least one of the restrictions specified in the WIFO questionnaire applies to the firm. More than two thirds (to be exact, 654) of the companies in our sample report that they have felt sufficiently funded by their house banks.

The subsequent Tables 4.1 and 4.2 give an overview, by means of standard descriptive statistics, of our interlinked data-set focusing on the relationship between non-financial companies covered in the WIFO-survey and their 'Hausbanken' prior to, during and after the financial crisis 2008/09.

Table 4.1: Descriptive Statistics of Inter-linked Dataset

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Non-financial enterprises						
Rating 2004 to 2007	948	306	59	296	177	550
Rating 2008 to 2009	948	296	64	280	167	567
Rating 2010 to 2013	948	301	59	285	205	539
Sales 2008, in €	850	3,325,706	13,200,000	988,350	0	275,000,000
Sales 2010, in €	855	3,283,176	13,200,000	995,000	0	286,000,000
Sales 2013, in €	859	3,655,978	15,200,000	1,072,000	0	313,000,000
Employment 2008, in persons	918	19	60	8	0	1,306
Employment 2010, in persons	925	19	58	8	0	1,192
Employment 2013, in persons	931	19	55	8	0	1,021
Local growth environment 2004 to 2007[1]	948	3.2	0.9	3.0	0.3	5.8
Local growth environment 2008 to 2009[1]	948	-1.4	1.2	-1.5	-4.5	2.0
Local growth environment 2010 to 2013[1]	948	1.6	0.7	1.5	-0.8	3.8
Company age	948	37	32	27	11	564
Banks						
Total assets 2004 to 2007, in mn €	235	410	1,363	116	22	14,859
Total assets 2008 to 2009, in mn €	235	493	1,938	136	26	25,927
Total assets 2010 to 2013, in mn €	235	503	1,992	135	26	25,611
Employment 2004 to 2007, in persons	235	56	113	25	4	1,082
Employment 2008 to 2009, in persons	235	58	117	26	5	1,285
Employment 2010 to 2013, in persons	235	57	117	25	5	1,207
Bank capital 2004 to 2007[2]	235	0.09	0.03	0.08	0.04	0.19
Bank capital 2008 to 2009[2]	235	0.09	0.03	0.09	0.03	0.20
Bank capital 2010 to 2013[2]	235	0.09	0.03	0.09	0.03	0.20
Cost-income ratio 2004 to 2007	235	0.69	0.09	0.70	0.40	1.03
Cost-income ratio 2008 to 2009	235	0.74	0.09	0.74	0.48	1.08
Cost-income ratio 2010 to 2013	235	0.69	0.09	0.70	0.36	0.99
Credit to non-banks 2004 to 2007[2]	235	0.59	0.13	0.59	0.10	0.89
Credit to non-banks 2008 to 2009[2]	235	0.60	0.13	0.61	0.17	0.89
Credit to non-banks 2010 to 2013[2]	235	0.57	0.14	0.58	0.14	0.89

S: WIFO calculations. - [1] Regional domestic product, arithmetic mean. - [2] In percent of total assets. - According to the definition of the World Bank, bank capital to assets is the ratio of bank capital and reserves to total assets. Capital and reserves include funds contributed by owners, retained earnings, general and special reserves, provisions, and valuation adjustments. Capital includes tier 1 capital (paid-up shares and common stock), which is a common feature in all countries' banking systems, and total regulatory capital, which includes several specified types of subordinated debt instruments that need not be repaid if the funds are required to maintain minimum capital levels (these comprise tier 2 and tier 3 capital). Total assets include all non-financial and financial assets.

Table 4.2: Number of Non-financial Firms under Study according to Business Sector Affiliation and Treatment Assignment

ÖNACE 2008		t_wifo=0 Untreated	t_wifo=1 Treated
		Number	
A	Agriculture, forestry, fishing	10	9
B	Mining and quarrying	4	2
C	Manufacturing	118	48
D,E	Electricity, gas, steam, water supply	7	0
F	Construction	110	33
G	Wholesale and retail trade	207	111
H	Transportation and storage	32	20
I	Accommodation and food service activities	54	23
J	Information and communication	11	4
K	Financial and insurance activities	12	8
L	real estate activities	27	8
M	Professional, scientific and tech. activities	33	17
N	Administrative and support service activities	16	9
P	Education	2	0
Q	Human health and social work activities	3	0
R	Arts, Entertainment and recreation	2	1
S	Other service activities	6	1

S: WIFO calculations.

5 Methodological Framework of the Potential Outcome Approach[6]

In their seminal monograph on causal inference, *Imbens and Rubin* (2015) start with stressing that there are different perspectives in theoretical and applied statistics to approach causality. The predominant view in econometrics, particularly in time series analysis is closely related to the notion of Granger-Sims-causality. This principle views causality as a prediction property. A variable A causes another variable B in the Granger-Sims sense, if, conditional on the past values of the variable B, and possibly conditional on other variables, past values of variable A predict future values of variable B. This approach fully builds on observable outcomes and the linkages between them, respectively[7]. This non-randomized framework, known as observational outcome approach, has been developed along the lines of the so-called Cowles Commission approach. According to this view, statistical models (mostly in the form of linear simultaneous equations or structural models) are specified for observed outcomes in terms of observed explanatory variables, estimated (exclusively by way of regression techniques), and then analyzed and tested in various ways (*Fair*, 1990)[8]. Since these models are designed so as to fully capture the interdependence of observed outcomes true causal effects should (or, have been assumed to) become clearly distinguishable from spurious causal effects. Unfortunately, as a vast body of econometric literature shows this expectation has proven to be a bitter disappointment much too often. As to assigning true causality, one major drawback of this approach is that it does a poor job in discerning 'true causality' when it comes to the point when nailing down whether changes of observed outcomes are driven by supply-side factors and/or demand-side factors is at issue. This shortcoming of structural econometric modeling has become known as 'identification problem of structural econometric modeling'.

Unlike standard econometrics, both experimental sciences and statistics have assumed (that is, taken for granted) causal inference to be fundamentally based on both potential outcomes and, hence, randomized experiments. This school of causal inference dates back to *Neyman* (1923) and *Fisher* (1926), and has been substantially advanced and retooled by *Rubin* (1974) and his numerous followers. In potential outcome modeling causal inference is drawn from randomized experiments in which a well-defined treatment assignment (that is, intervention mechanism designed by the investigator), based on a truly random device (i. e.,

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[6] This section builds on *Egger and Hahn* (2010), *Guo and Fraser* (2015), *Imbens and Rubin* (2015), and *Rosenbaum* (2010).

[7] According to *Imbens and Rubin* (2015, p. 27), this approach builds on statistical models relating the observed value of the outcome variable to covariates and indicator variables for treatment levels, with the causal effects defined in terms of the parameters of these models, a tradition that appears to originate with *Yule* (1897). The authors add that this approach is primarily aimed at estimating associations, for example, correlations between observed variables, and then attempts, using various external arguments about temporal ordering of the variables, to infer causation, that is, to assess which of these associations might be reflecting a causal mechanism.

[8] The Cowles Commission approach is closely associated with the work of, among others, J. Tinbergen and T. Haavelmo, the two Nobel-prize winning founding fathers of modern econometrics.

coin toss, random numbers generator) is applied so that a clear distinction between covariates measured prior to the treatment, and outcomes measured after treatment can be made (*Imbens and Rubin*, 2015). Since the seminal paper by *Rubin* (1974) the potential outcome approach has been extended beyond pure randomized experiments. Potential outcome modeling now allows for assessing causation even when treatment is beyond control of the researcher such as in observational studies where treatment assignment is not random but merely haphazard. That is to say, when the setting of a study comes close to that of a natural experiment the outcomes subjects would exhibit under treatment or under control may be regarded as to be unrelated to the treatment assignment (*Guo and Fraser*, 2015). Observational studies strongly rely on the comparability of the treated and the untreated under study on the condition that they share enough features prior to the treatment. Broadly speaking, treated and untreated subjects in the observational study framework are assumed to be identical twins from a statistical (that is, measurable) point of view prior to the treatment. The only feature the treated and their controls do not share is the treatment itself. Since in observational designs treatment is assigned 'by nature' and, hence, as good as random differences in outcomes after treatment between the treated and their controls are assumed to be, most likely, due to a cause-effect relationship between treatment and outcome.

The most developed strategy for causal analysis in observational studies is the method of propensity score (*Rosenbaum and Rubin*, 1983), or propensity score matching (PSM), that is applied in this study. The very basics of this framework are introduced below.

A natural approach to cope with causation identification in a "quasi-natural experiment" is a difference-in-difference analysis augmented by advanced matching techniques. This statistical framework is extensively used in statistics, economics, psychology, sociology and program evaluation in order to estimate treatment effects (i. e., *Stuart*, 2010). Matching, in our understanding, refers to any method that aims to equate (balance) the distribution of covariates in the treated and control groups in order to identify causal treatment effects.

In our application we argue that credit limits can be analyzed as treatment effects. Like every observational study (in contrast to randomized experiments), our application faces the problem that each individual or firm has only one observable outcome, either an outcome with treatment (limited credit) or without treatment (no credit limitation). Thus, the analysis of credit limits faces the fundamental problem of assessing how a firm receiving treatment (exposed to a credit limit) would have performed without treatment. The latter outcome is, of course, unobservable. This is the "fundamental problem of causal identification" (*Holland*, 1986). For efficient causal identification a counterfactual outcome needs to be constructed. Thus, the problem faced in our analysis is one of missing data. Matching provides one avenue to construct counterfactual outcomes.

Alternatives to matching methods (*Rosenbaum and Rubin*, 1983, 1984; *Imbens*, 2004; *Stuart*, 2010) include adjusting for background variables in regression analysis, instrumental variable techniques (e. g. *Wooldridge*, 2002), structural equation modeling (e. g. *Pearl*, 2009) and selection models (e. g. *Heckman*, 1978)[9].

For our application we prefer matching techniques, since they fit our research tasks quite naturally. Regarding instrumental variable estimators, we consider the identification of appropriate instruments using enterprise data even more challenging than in applications involving individuals or regions. *Cochran and Rubin* (1973) and *Heckman et al*. (1998) show that linear regression adjustment can actually bias the treatment effect, if the relationship between covariates and outcome is moderately linear. An additional argument in favor of matching is that it allows for identify both the "average treatment effect on the treated" (*ATT*) and the "average treatment effect" (*ATE*). The instrumental-variable techniques allow for the computation of the "average treatment effect" (*ATE*) only[10]. Moreover, because matching can be considered a nonparametric pre-processing technique to adjust data prior to parametric analysis, matching methods are not in conflict with regression adjustment. In fact, matching methods and regression analysis can be considered to be complements (see *Ho et al.*, 2007; *Stuart*, 2010).

However, there are important assumptions that need to be met in order to be able to estimate causal effects using matching methods. As most non-experimental methods of causal identification, matching relies on ignorability. This requires that there are no unobserved differences between treatment and control groups, conditional on the observed covariates used in the matching procedure (see *Imbens*, 2004). While at first sight this assumption sounds very strong, this is not necessarily the case, as matching methods control also for unobserved variables, if they are correlated with the observed covariates. Thus, only time-varying unobserved covariates are problematic.

Another important assumption is the stable unit treatment value assumption (*SUTVA*), which states that the outcome of one firm is not affected by the treatment assignment of other units. In our context this refers to the fact that limiting credit for one firm should not affect a different firm. This is also not necessarily the case. However, if credit limits are predominantly imposed on small businesses this should cause less concern. Moreover, the plausibility of *SUTVA* can be improved by the specific design of the matching routing in order to reduce the possible interactions between treated and untreated groups of firms. To round up, since our dataset is exclusively composed of small to medium-sized non-financial firms related to small to medium-sized 'Hausbanken' we consider the occurrence of general equilibrium feedback effects in the given setting as rather unlikely.

Although in practice researchers use a large number of matching methods, the theoretical ideal is exact matching on covariates. However, when the covariates are high dimensional, exact matching does not work well, as many observations are not matched, which may lead to a higher bias than inexact matching. The most widely used technique to circumvent this

[9] For an introduction see *Cobb-Clark and Crossley* (2003).

[10] An insightful discussion of the relationship among these methods is given in *Angrist and Pischke* (2009).

pitfall is propensity score matching (*PSM*) introduced by *Rosenbaum and Rubin* (1983). *PSM* is easy to implement and allows to cope with the high dimensionality of covariates X_{it-1} (pre-treatment firm-specific characteristics) and, most importantly, provides an operational measure of similarity for continuous data.

The idea behind *PSM* is that functions $b(X_{it-1})$ are such that the conditional distribution of X_{it-1} given $b(X_{it-1})$ is independent of assignment into treatment of firm i in year *t* on average. The probability of receiving treatment in year *t* given observed characteristics X_{it-1} is called propensity score $P(X_{it-1})$ and, hence, the respective matching technique is termed propensity score matching[11]. Since limited credit imposition is captured by a binary variable, the propensity scores $P(X_{it-1})$ can be estimated on the basis of a probit model or a logit model.

Since propensity scores are independent of assignment into treatment, establishing equality of the average propensity scores of treated firms (exposed to limited credit) and untreated firms (not exposed to limited credit, but otherwise being equal) provides a metric of similarity.

Based on the different methodologies the simplest estimator of the treatment effect is a t-test (or an equivalent non-parametric test, e. g. a Fligner-Policello test) on the difference in outcome between treated and untreated firms denoted by y_t^1 and y_t^0, in difference-in-difference form:

(5.1) $$\Delta_{ATT,t} = [E(y_{it}^1 \mid W_{it} = 1, X_{it-1}) - E(y_{it}^0 \mid W_{it} = 0, X_{it-1})],$$

where W_{it} is the binary treatment indicator, X_{it-1}, is the vector of covariates measured at time *t-1*.

Let us focus on the consequences of credit limits in year t and denote the performance outcome vector of firms with (without) credit restriction in that year by y_t^1 (y_t^0). In our application, y_t^1 (y_t^0) denotes a vector of changes in the outcome level variable employment and output from year *t-1* to year *t*. Furthermore, let the variable w_t denote a binary treatment indicator, where an entry of one denotes a credit restriction of the respective firm in year *t* (treatment) but not before, and an entry of zero indicates that a firm did not experience a credit limitation in year *t* or before. What we are primarily interested in is the difference in outcomes with and without credit limitation for company *i*. More formally, with a possible credit limit in year *t*, we are interested in

(5.2) $$\Delta_{it} = y_{it}^1 - y_{it}^0 .$$

[11] An excellent introduction to the most common matching procedures based on propensity scores give, among others, *Caliendo and Kopeinig* (2005) and, more recently, *Guo and Fraser* (2015).

Since y_t^1 and y_t^0 are based on differences in employment and output from *t-1* to *t* already, Δ_{it} represents a difference of differences. As mentioned above, we only can observe either y_{it}^1 or y_{it}^0 , but not both for the same company *i* as given by

(5.3) $$y_{it} = (1 - w_{it}) y_{it}^0 + w_{it} y_{it}^1 = y_{it}^0 + w_{it}(y_{it}^1 - y_{it}^0).$$

Thus, we are not capable of estimating (1) directly. Though individual treatment effects Δ_{it} cannot be estimated directly, indirect inference on the basis of population averages can be drawn provided the so-called stable unit-treatment value assumption (*SUTVA*) applies. The *SUTVA* states that the treatment of each company *i* is independent of treatment participation of other firms $j \neq i$ in year *t*. This implies that there are no (or only negligible) feedback effects such as peer effects or general equilibrium effects that may bias the estimands, the population average treatment effects.

As to population averages, there are several treatment effect concepts used in the evaluation literature, the most prominent of which are the 'average treatment effect on the treated' (*ATT*)

(5.4) $$ATT_t \equiv E(y_{it}^1 - y_{it}^0 \mid X_{it-1}, w_{it} = 1) = E(y_{it}^1 \mid X_{it-1}, w_{it} = 1) - E(y_{it}^0 \mid X_{it-1}, w_{it} = 1),$$

and the 'average treatment effect' (ATE)

(5.5) $$ATE_t \equiv E(y_{it}^1 - y_{it}^0 \mid X_{it-1}) = E(y_{it}^1 \mid X_{it-1}) - E(y_{it}^0 \mid X_{it-1}),$$

where X_{it-1} denotes a vector of pre-treatment firm-specific characteristics or covariates.

As to *ATT*, the second term on the right hand side of equation (5.4) is unobservable as it represents the counterfactual. Thus, in order to compute *ATT* we need to state an identifying assumption that allows for assessing $E(y_{it}^0 \mid X_{it-1}, w_{it} = 1)$ [12]. A reasonable presumption is that $E(y_{it}^0 \mid X_{it-1}, w_{it} = 1)$ equals $E(y_{it}^0 \mid X_{it-1}, w_{it} = 0)$. If this condition holds, sufficiently funded companies can serve as an adequate control group. While this requirement is most likely met by randomized experiments it may not be met by non-experimental data like our's.

There are several solutions to the selection problem, one of which is the matching approach[13]. Overcoming the self-selection bias through matching techniques calls for some further identifying assumptions. In the evaluation literature based on the matching approach,

[12] Estimating *ATE* requires additional identifying assumptions since both counterfactual outcomes $E(y_{it}^1 \mid X_{it-1}, w_{it} = 0)$ and $E(y_{it}^0 \mid X_{it-1}, w_{it} = 1)$ have to be constructed.

[13] As to the causal treatment analysis, there basically are three strands of research ongoing: (i) matching techniques based on the propensity score (see *Rosenbaum and Rubin*, 1983, 1984; *Abadie*, 2005; *Imbens*, 2004), (ii) estimating the selection equation and the average treatment effect equation jointly by maximum likelihood (see Heckman, 1978), and (iii) adopting an instrumental variable approach (see *Wooldridge*, 2002). An introduction to these methods is given, among others, in *Cobb-Clark and Crossley* (2003).

a common (and reasonable) identification strategy is one that is guided by the so-called 'conditional independence' assumption (*CIA*) and the 'common support or overlap' condition (*CSC*), respectively. The former holds that the assignment to treatment be unconfounded, the latter states that the probability of assignment be bounded away from zero and one. In other words, the *CIA* allows for the construction of the missing counterfactual means since, conditional on X_{it-1}, the potential outcomes and the assignment to treatment are taken to be independent. The *CSC* makes sure that the construction is well-defined since, according to this assumption, there always exists, conditional on (the same) X_{it-1}, a positive probability of belonging to both groups, the treated population and the control (that is, untreated) population. In the evaluation literature, these assumptions together are referred to as 'strong ignorability' (*Rosenbaum and Rubin*, 1983). As stressed in *Caliendo and Kopeinig* (2005), these assumptions are very strong indeed since they propose 'that selection is solely based on observable characteristics and that all variables that influence treatment assignment and potential outcomes simultaneously are observed by the researcher'[14].

It clearly depends on the data quality, whether the imposition of such strong assumptions is justified. Given our dataset, however, we strongly believe that the assumptions apply. Emphasizing the importance of good data, we consider the available dataset rich enough to justify all three basic assumptions that are elemental to the matching approach. Since we primarily deal with credit limitations imposed by small to medium-sized banks on their small to medium-sized clients, which happen to operate overwhelmingly locally, we are confident that there are no feedback effects violating the *SUTVA*. Furthermore, given the size and the structure of our sample. Further, we certainly expect that the group of sufficiently funded non-financial firms allows for the construction of viable counterfactuals for the estimation of both, *ATT* and *ATE*, respectively. Since the non-financial firms under study (fully funded as well as limited funded) are small to medium-sized, the data available ought to allow for the composition of a large group of fully funded non-financial firms similar to credit-restricted firms in terms of the relevant pre-treatment characteristics or covariates X.

As a result, the data situation at hand is most likely to support fully both core matching assumptions, (*CIA*) and (*CSC*), so that the differences in performance outcomes between the group of credit-constrained firms and the adequate control group can be rightly attributed to credit limitations as covered in the WIFO survey.

[14] According to *Heckman et al.* (1998) for the estimation of both the ATT and ATE it often suffices to assume mean-independence only.

6 Implementing Propensity Score Matching[15]

In order to cope with the identification problem as described in the introduction (and to a lesser extent with the dimensionality of the covariate vector X_{it-1}) we apply the propensity score matching (*PSM*) introduced by *Rosenbaum and Rubin* (1983). As indicated above, this matching technique is based on the idea that the functions $b(X_{it-1})$ are such that the conditional distribution of X_{it-1} given $b(X_{it-1})$ is independent of assignment into treatment of firm *i* in year *t* on average. The probability of receiving treatment in year *t* given observed characteristics X_{it-1} is called propensity score $P(X_{it-1})$. The respective matching technique is termed propensity score matching[16].

The *PSM* estimator for *ATT*, for example, has then the following general form

(6.1) $$\Delta^{PSM}_{ATT,t} = E_{P(X_{it-1}|w_{it}=1)}[E(y^1_{it} \mid w_{it}=1, P(X_{it-1})) - E(y^0_{it} \mid w_{it}=0, P(X_{it-1}))].$$

Since credit limitations are captured by a binary variable, the propensity scores $P(X_{it-1})$ can be estimated on the basis of a probit or a logit model. Matching on the same estimated propensity is conducted by applying the nearest neighbor procedure, which is supported by various robustness checks[17].

Possible time composition effects can be avoided by a difference-in-difference (*DID*) analysis that provides treatment effect estimates which are free of the sample-composition effects, and can address issues of the time pattern of treatment effects on outcome indicators. In other words, the *DID* estimator allows for the analysis of immediate versus sluggish adjustment, respectively.

In accordance with the respective literature, we view the *DID* estimator combined with a cross-section matching estimator to be more robust than the matching approach applied single-handedly since it allows both, selection on observables and selection on time-invariant unobservables (*Caliendo and Hujer*, 2005, p. 12).

For the sake of clarity, let us briefly summarize the procedure of estimating the average treatment effect of the treated ($\Delta^{PSM}_{ATT,t}$) and the average treatment effect ($\Delta^{PSM}_{ATE,t}$) by means of propensity score matching step by step:

[15] This section builds on *Egger and Hahn* (2010). *Guo and Fraser* (2015), *Imbens and Rubin* (2015), and *Rosenbaum* (2010).

[16] An excellent account of the most common state-of-the-art matching procedures applied in propensity score analysis give, among others, *Caliendo and Kopeinig* (2005), and *Guo and Fraser* (2015).

[17] The nearest neighbor matching estimator relies on the comparison of some outcome variables between the treated and their 'closest twins' among the untreated in terms of the estimated propensity score. For a detailed discussion of the different matching algorithms, see *Caliendo and Kopeinig* (2005).

- Estimate the propensity score $P(X_{it-1})$ of being credit-constrained for treatment year t given characteristics X_{it-1}.
- For each credit-constrained non-financial firm in *t*, find the closest two twins of sufficiently funded firms in that year[18]. With nearest-neighbor matching, the closest financially unconstrained firm is the firm with the closest propensity score. It may be the case that there are more than just two closest "twins". In this case, the average is taken. If financially unrestricted firms are matched onto more than one credit-constrained firm in the sample, this has to be respected when computing the standard error of the treatment effect. All credit firms for which no closest match can be found within the support region of the propensity scores are dropped. Similarly, find closest twins to all fully funded firms in the subsample of credit-constrained firms in year *t*.
- Now compute the average difference between y_{it}^1 and $\tilde{y}_{it}^0$, where the latter denotes the change in outcome between years *t-1* and *t* for the matched control units across all *i* and *t*. This is the average treatment effect of the treated ATT_t which bears subscript *t* since it considers contemporaneous effects of credit limitations on outcome. Similarly, compute the average difference between $\tilde{y}_{it}^1$ and y_{it}^0, where the former denotes the change in outcome between years *t-1* and *t* for the matched treated units (financially constrained firms which are matched onto financially unconstrained ones) across all *i* and *t*. This is the average treatment effect of the untreated ATU_t. The average treatment effect ATE_t corresponds to a weighted average of ATT_t and ATU_t where the respective frequencies of merged and unmerged banks within the support region of the propensity score serve as weights.

According to *Guo and Fraser* (2015, p. 134) the advantage of the propensity score in matching, stratification, and weighting is its reduction of dimensions. The vector X may include many covariates, which represent many dimensions, and the propensity approach reduces all this dimensionality to a one-dimensional score.

Guo and Fraser (2015) also re-emphasize that the propensity score $P(X_{i,t-1})$ is a balancing measure (so called the coarsest score) that summarizes the information of vector $x_{i,t-1}$ in which each x covariate is a finest score. *Rosenbaum and Rubin* (1983) derived and proved a series of theorems and corollaries showing the properties of propensity scores. The most important property is that a coarsest score can sufficiently balance differences observed in the finest scores between treated and control participants. The properties of propensity scores include, among others, that propensity scores balance observed (and, if correct specified, unobserved) differences between treated and control participants in the sample. *Rosenbaum* (2002) showed that a treated and control participant with the same value of the

[18] *Austin* (2010) recommends that a treated subject be matched to 1 or 2 untreated subjects in order to minimize bias.

propensity score have the same distribution of the observed covariate X. The latter represents the balancing property of the propensity score. This means that in a stratum or matched set that is homogenous on the propensity score, treated and controls may have differing values on X but the differences will be chance (random) differences rather than systematic differences (*Guo and Fraser*, 2015 p. 134).

6.1 Selection on Observables into Constraining Credit

We formulate the following binary response model to assess the determinants of the occurrence/probability of a credit restriction imposed on a non-financial firm:

(6.2)
$$P(w_{it} = 1) \approx P\left(\alpha_0 + \sum_{k=1}^{K} \alpha_k X_{k,it-1} + \varepsilon_{it} > 0\right),$$

where α_0 is a constant, K denotes the number of explanatory variables $X_{k,it-1}$ in the selection equation, and ε_{it} is an identically and independently distributed error term. In our applications, ε_{it} is assumed to be distributed logistically (logit model).

(6.3)
$$P(W_{i,t} \mid X_{i,t-1} = x_{i,t-1}) = E(W_{i,t}) = \frac{e^{x_{i,t-1}\beta_i}}{1 + e^{x_{i,t-1}\beta_i}} = \frac{1}{1 + e^{-x_{i,t-1}\beta_i}}.$$

Obviously, equation (6.3) is non-linear but can easily be linearized by the logit function (that is, the natural logarithm of odds)

(6.4)
$$\ln\left(\frac{P(W_{i,t})}{1 - P(W_{i,t})}\right) = X_{i,t-1}\beta_i.$$

The left-hand-side variable $W_{i,t}$ is set to one for the treatment period from 2008 to 2009 when the non-financial firm *i* was credit-constrained (due to the unfolding of the global financial crisis) by its 'Hausbank' at least through one of the credit-limits covered in the WIFO-survey (that is, 'bank financing not granted', 'amount of bank credit lowered', 'bank financing not extended', 'credit line reduced', or 'collateral requirement increased') and set to zero when it was not (that is, 'full funding provided' by its 'Hausbank'). In addition to this key treatment variable, each single credit-limiting intervention imposed by the 'Hausbanken' on their clients as surveyed by WIFO is also captured by a binary response variable. These additional treatment indicators are used for checking which of the limitations played out strongly due to the financial crisis and which one only moderately.

We consider the treatment assignment as designed to be well motivated, because prior to the recent financial crisis the relationship between small and medium-sized non-financial firms (as represented in our sample) and their local banks, called 'Hausbanken' (as represented in our sample) was primarily conditioned by features strongly linked to the notion of vicinity. That is, banks in close vicinity had been (and still are) particularly strongly favored by small and medium-sized firms as their 'prime banks' since these banks are assumed to be most likely best capable of assessing the creditworthiness of their business model. Conversely, local customers and their business activities are particularly well known to local banks not only due to hard data but also, among others, due to soft facts such as reputation and standing in the local business community. Easier access to the latter type of 'insider information' gives local banks a competitive edge over banks that lack first-hand knowledge and understanding of the local color of the business environment the local firms are operating in. As a result, local banks are very likely to prefer local customers and local customers are very likely to prefer local banks. This constitutes the very essence of relationship banking. Hence, prior to the financial crisis supply-side determinants such as regulatory banking standards (i. e., liquidity, capitalization etc.) had been less perceptible as credit-limiting factors in the relationship between local banks and their local clients. Self-selection of non-financial firms on observables such as bank capital requirements into connecting to a particular bank (i. e., to a particularly strong-capitalized bank) can thus be neglected as a potential source of errors in estimating causal effects concerning the bank-client-relationship.

Table 6.1 summarizes our findings for different model specifications that explain the probability of a credit-limitation occurrence. As mentioned above, the coefficient estimates reported are from the logit model. In order to shed light on the relative importance of the exogenous and endogenous factors driving particular forms of credit-constraints (most notably those covered by WIFO), we also run regressions where we employ each treatment variable separately.

From Table 6.1 it may appear that our specification of the propensity score equation is likely to suffer from an omitted-variable bias, as only five determinants are accounted for (when we leave those variables out of consideration, due to statistical insignificance, that are aimed at controlling for sector affiliation of the non-financial firms under study)[19]. However, we are in the fortunate position to have high-quality information on the creditworthiness of all non-financial firms covered in our sample for the period prior to (starting 2004), during and after the financial crisis (ending 2013). Without dispute, creditworthiness of a borrower is the most relevant demand-side determinant in the loan market.

As already indicated, the country's largest and highly reputed credit bureau, the KSV 1870, has made its credit ratings of the non-financial companies investigated in this study available to us[20]. Firstly, the KSV 1870 runs a highly sophisticated state-of-the-art credit-scoring model aimed at covering all available information on a client's creditworthiness. Secondly, it looks

[19] In following *Wooldridge* (2002 p. 465) we hold that goodness of fit is, particularly in our modeling context with the focus on decomposition of supply- and demand-side effects, not as important as statistical and economic significance. Thus, as to model specification we focus exclusively on explanatory variables that meet the latter requirements with goodness-of-fit guidance levels as to Pseudo R^2 coming second. Besides, we are dealing with cross-sectional data where due to missing time trends the typical R^2s are usually much lower than in time series data settings.

[20] The authors in turn ensure the full compliance with both the country's and the much stricter KSV 1870 internal data protection rules.

Table 6.1: Logit Selection Equations [1]

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6			Model 7		
	Coef.	Std.dev.		Coef.	Std.dev.		Coef.	Std.dev.		Coef.	Std.dev.		Coef.	Std.dev.		Coef.	Std.dev.		Coef.	Std.dev.	
ekq_b_before	-7.431	3.372	**	-7.635	3.298	**	-14.655	7.194	**	-5.738	5.236		-7.057	10.259		-7.028	6.005		-7.375	3.494	**
rating_before	0.009	0.001	***	0.008	0.001	***	0.013	0.002	***	0.008	0.002	***	0.010	0.004	***	0.009	0.002	***	0.007	0.001	***
gruendksv	-0.005	0.003	*	-0.005	0.003	*	-0.016	0.009	*	-0.009	0.006		0.001	0.005		-0.002	0.004		-0.006	0.003	*
wachs_before	-0.244	0.092	***	-0.261	0.090	***	-0.343	0.171	**	-0.197	0.145		-0.730	0.258	***	-0.505	0.161	***	-0.316	0.096	***
opentt	0.358	0.193	*	0.469	0.165	***	0.253	0.343		0.452	0.266	*	0.603	0.482		0.825	0.284	***	0.425	0.175	**
onace_A	1.572	1.249																			
onace_B	1.281	1.474																			
onace_C	0.537	1.161																			
onace_F	0.154	1.167																			
onace_G	0.844	1.153																			
onace_H	0.483	1.195																			
onace_I	0.634	1.180																			
onace_J	0.894	1.316																			
onace_K	1.276	1.271																			
onace_L	0.522	1.225																			
onace_M	0.833	1.196																			
onace_N	1.065	1.236																			
onace_R	0.434	1.766																			
Constant	-2.619	1.288	**	-1.650	0.554	***	-3.989	1.089	***	-3.161	0.898	***	-3.852	1.510	**	-2.975	0.962	***	-1.420	0.580	**
Observations	812			822			606			631			577			618			780		
Log-likelihood	-474.814			-484.198			-144.467			-215.888			-78.835			-182.604			-439.587		
Pseudo-R²	0.074			0.063			0.163			0.054			0.093			0.083			0.058		
LR chi	76.040			65.110			56.310			24.390			16.110			33.070			54.390		
Number of treated		265			265			50			74			20			61			223	
Number of controls		547			557			556			557			557			557			557	

S: WIFO calculations. - [1] List of variables see Appendix A: Table A1.- Std.dev .... standard deviation; Model 1 and model 2: Treatment intervention (1,0): (1) at least one exposed to the following intervention (a) bank financing not granted, (b) amount of bank credit lowered, (c) bank financing not extended, (d) credit line reduced, (e) collateral requirement increased; (0): full funding provided; model 3: bank financing not granted; model 4: amount of bank credit lowered; model 5: bank financing not extended; model 6: credit line reduced; model 7: collateral requirement increased. *** significant at 1 percent; ** significant at 5 percent; * significant at 10 percent.

back on many years of active life in assessing the creditworthiness of debtors (individuals and firms alike) in Austria. The KSV 1870 rating scores are not only highly appreciated by suppliers (for checking if payment after delivery can be expected as contracted), but also by banks for double-checking the quality of their own credit scores drawn from bank-internal rating models. It is worth noting that KSV 1870 rating scores are broadly enriched by a huge body of soft factors aimed at reflecting social and general business standing of debtors as closely and accurately as possible. In so doing, the KSV 1870 provides a very broad picture of a debtor's creditworthiness furnishing its credit risk measurement with a flavor of 'multiple precision'. We are not able to give a detailed technical account of the rating model of the KSV 1870, since it is a complete in-house development and as such strictly proprietary. However, we have been reassured by KSV 1870 rating experts that the KSV 1870 credit ratings of non-financial firms are drawn from credit scoring models that are in full compliance with the standards stipulated in the most recent Basel Accords concerning regulatory bank capital adequacy requirements.

From the viewpoint of banks, credit-risk ratings of credit applicants are by far the most critical demand-side factor in the process of appraising loan applications. Loan application appraisals are usually conducted by experts of banks' credit departments with the help of internal credit rating models approved by the banking supervisory authorities. The decision whether a credit application is approved or rejected by banks strongly hinges on the probability to what extent the applicant will default on the loan when granted. Rating credit risk means no less than putting a single figure on such an occurrence by using and properly weighing all information available that may affect the financial standing of an applicant for today and in the future. Per design, credit ratings are similar in spirit and meaning to propensity scores drawn from binary response models like our logit model. That is to say, loan applicants who share the same or similar credit rating scores are regarded by banks as identical or similar in terms of credit risk exposure regardless of observable differences in characteristics such as company size, legal form, location, sector affiliation etc. The upshot is that from the viewpoint of the loan-granting bank companies of same credit rating, per construction of the credit rating models, differ not systematically, they only differ by chance, and hence on a (statistically) negligible scale.

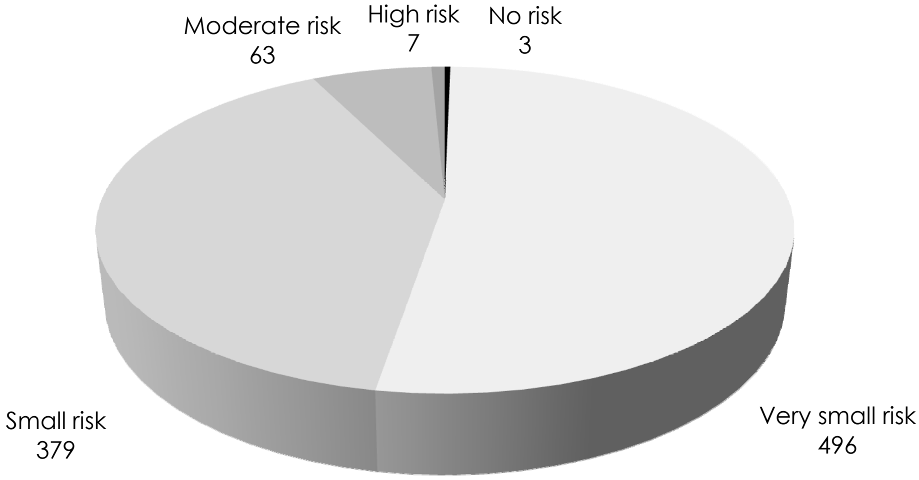
Table 6.2 reports the probability to default and probability to insolvency, respectively across the spectrum of scores gained from the KSV 1870 rating models. In the run-up to the financial crisis more than half of the businesses under investigation were considered to be of 'very low credit risk', more than one third viewed as slightly riskier but still assessed as 'low credit risk'. Figure 6.1 gives a graphical exposition of the average credit risk scores assigned to the businesses in our sample for the period 2004 to 2007.

Table 6.2: KSV 1870 Ratings of the Non-financial Firms under study

Rating category	Risk	Probability to bankruptcy	Probability to default (PD)
100 to 199	No risk	0.01% to 0.03%	No PD
200 to 299	Very small risk	0.03% to 0.22%	Very low PD
300 to 399	Small risk	0.22% to 1.4%	Average PD
400 to 499	Moderate risk	1.4% to 8.55%	Above average PD
500 to 599	High risk	8.55% to 38.1%	Hig PD
600 to 699	Very high risk	Over 38.1%	Very high PD
700 and more	Insolvency		

S: KSV 1870.

Figure 6.1: Average credit risk scores assigned to the businesses under study, average 2004 to 2007



S: KSV 1870.

While credit scores are a prime measure of a client's creditworthiness on the demand side of the loan market, bank capitalization is the prime determinant on the supply side. This has been confirmed by numerous specification tests applied to a broad spectrum of bank-specific covariates affecting bank's supply behavior, such as bank profitability and bank liquidity. Not surprisingly the simple leverage ratio (defined as bank capital over total assets) turns out to be the only bank-specific supply-side determinant that significantly affects the probability whether a client gets credit-constrained or not[21].

[21] Bank capital to total assets is the ratio of bank capital and reserves to total assets as defined in Table 4.1. It is worth noting that all results reported in this study hold when core capital over total assets is applied instead.

The specification of our binary response model is completed by adding three further covariates that are to cover (a) the strength of the relationship between bank and client, (b) the local economic environment of borrowers and lenders, and (c) the borrower's range of business activities.

The relationship between bank and borrower needs time to build. The longer a relationship lasts the stronger it gets. Over time borrowers with close ties to their Hausbanken tend to become more and more inclined to share insider information with their lenders thereby building up credibility as a good debtor. Likewise, as time goes by banks learn to understand and assess the business model of their clients better. As a consequence, the endurance of a relationship tends to affect the strength of the relationship between bank and borrower positively, with financial ties getting stronger as time proceeds. Since we are not able to gather data on relationship length, instead we use the companies' age as a proxy. This builds on the expectation that a company's age is positively correlated with the length of its relationship with the Hausbank.

The relationship between a local bank and its clients may also be affected by the economic environment. The lending-relations between a bank and its borrowers may be in a more upbeat mood when both operate in an economic environment that shows strong growth. The opposite is more likely to occur when overall economic growth is moderate or even weak. We capture this macro-based impact by explicitly controlling for economic growth of the very region/district both Hausbank and borrower are headquartered in.

Finally, the lender-borrower relationship may also be influenced by the range of the business activities of the borrower. The credit risk borne by a local lender is usually considered to be lower when the borrower does business only at the local or domestic level. Businesses that venture into foreign markets either as exporters or importers may be more vulnerable to financial imbalance due to its widened exposure to external shocks. Hence, we control for credit risk due to borrowers' international connections by introducing a binary variable into our logit model. This variable takes on the value 1, if the firm has a foreign orientation, and 0, if it has a domestic orientation.

The estimation results in Table 6.1 present a picture suggesting that banks are more likely to impose credit limits (of various kinds) on their borrowers when

- the borrowers' creditworthiness (as measured by credit risk scores) is low,
- the lender-borrower relationship (proxied by companies' age) is weak,
- the borrowers' economic growth environment (as measured by the growth rate of real regional domestic product) is weak,
- the borrowers' business activities stretch beyond national borders, and, most importantly,
- the banks' own capital buffer (as measured by the simple leverage ratio) is low.

These findings strongly corroborate the presumption – held by many not only in the general public but also in academia (see the discussion of related literature in Section 3) – that bank credit limits are not only imposed due to unfavorable demand-side factors (i. e., bad debtors, poor information, weak growth environment, high exposure to external shocks) but also due to supply-side weaknesses such as poor bank capitalization.

Interestingly, roughly the same picture arises for each single type of interventions covered in the WIFO survey though not always at the same high level of significance. However, as the results for model (3) and model (7) in Table 6.1 illustrate quite clearly bank credit restrictions in the form of credit denials (model 3) and higher collateral requirements (model 7) have been significantly more unlikely when banks are rich in capital. The overall results for both models resemble to a large extent those of model (1) and (2) which are aimed at measuring the likelihood of occurrence of at least one of the credit limits defined in the WIFO survey.

On the basis of the estimates summarized in Table 6.1, we proceed with evaluating the impact of credit limits on employment and output of non-financial companies.

7 The Treatment Effect of Credit Limits on Employment and Output – Preliminary Results[22]

Following the argumentation in *Egger and Hahn* (2010) we stress that consistent estimation of treatment effects by selection on observables using matching techniques requires the construction of a suitable control group based on some measures of similarity. The probability of getting exposed to credit limits in the years 2008 and 2009 (and, most probably, in the following years at least up to 2013) as predicted by the propensity scores drawn from our preferred logit model (that is, model (2) in Table 6.1) is only a valid compound measure of similarity, if the treatment group and the control group are similar in each and every respect, besides the treatment assignment which is, per design, assumed to be haphazard. If these preconditions are not met, we cannot infer that the difference in the change of the outcome variables of credit-restricted firms and sufficiently funded firms is in fact due to the difference in other determinants rather than credit-limitations. The relevant condition for the propensity score to be a valid measure of similarity is referred to as the balancing property. The corresponding results of a comparison of the explanatory variables for the treatment and control group subdivided into four strata, respectively, are summarized in Table 7.1[23]. In testing the balancing property we follow *Egger and Hahn* (2010) where a median-based t-test procedure as suggested by *Rosenbaum and Rubin* (1985) is applied.

According to Table 7.1, the median absolute t-statistic is 0.08. This is a very clear indication that the balancing property is most likely not violated for the average covariate in the logit model (2). Hence, the median absolute bias after matching is zero (according to *Rosenbaum and Rubin*, 1985), it should be smaller than 20 percent as a rule-of-thumb).

For inference, it is important that all explanatory variables in the selection model are not significantly different for the matched treated and control units. As can be immediately seen, this condition holds for all included explanatory variables. Hence, in this regard there is no concern about matching based on propensity scores derived from the logit models as specified in Table 6.1.

A closer inspection of the results for the four strata reported in Table 7.1 shows that the probability of getting exposed to credit limitations during the financial crisis lies above zero for non-financial companies encompassed by stratum 1. These non-financial companies in the stratum show a very low credit risk according to the KSV1870 rating (on average equaling 250), have a long-lasting relationship with their comparatively well capitalized Hausbanken (the former proxied by average company age equaling 54, the latter by average bank capital ratio equaling 0.10), face a favoring growth environment and

[22] The line of argumentation in this section follows closely section 5.3 in *Egger and Hahn* (2010).

[23] A graphical exposition of the respective balancing properties is given in Appendix A, Figure A1 to Figure A7.

Table 7.1: Balancing Property

	Treated t_wifo=1	Control t_wifo=0	t	p>|t|
Stratum 1				
Propensity	0.16	0.16	-0.09	0.93
ekq_b_before	0.10	0.10	0.44	0.66
rating_before	252.40	251.95	-0.08	0.94
gruendksv	54.35	52.22	-0.19	0.85
wachs_before	3.76	3.77	0.08	0.93
opentt	0.09	0.09	0.03	0.98
Observations	23	158		
Stratum 2				
Propensity	0.30	0.29	-1.60	0.11
ekq_b_before	0.09	0.09	0.22	0.83
rating_before	299.79	296.98	-0.76	0.45
gruendksv	33.86	36.36	1.04	0.30
wachs_before	3.20	3.20	0.11	0.91
opentt	0.29	0.29	-0.03	0.97
Observations	171	402		
Stratum 3				
Propensity	0.49	0.47	-1.42	0.16
ekq_b_before	0.08	0.08	0.25	0.80
rating_before	369.53	368.04	-0.20	0.84
gruendksv	28.94	27.92	-0.29	0.77
wachs_before	2.82	2.81	-0.04	0.96
opentt	0.55	0.46	-1.08	0.28
Observations	84	84		
Stratum 4				
Propensity	0.69	0.66	-1.36	0.19
ekq_b_before	0.08	0.07	-1.20	0.24
rating_before	465.57	434.30	-1.43	0.17
gruendksv	28.87	18.30	-1.35	0.19
wachs_before	2.57	2.73	0.38	0.71
opentt	0.67	0.70	0.17	0.87
Observations	15	10		

S: WIFO calculations.

have a strong domestic business orientation. The balancing property features for stratum 3 and stratum 4, however, are more appropriate for an overall-assessment whether the average Austrian non-financial company was running a high or low credit-limitation risk during the financial crisis and thereafter. Since at least half of the Austrian non-financial firms and, importantly, the great majority of Austrian commercial banks share the features that drive stratum 3 and stratum 4, we have to conclude that the likelihood of getting credit-constrained during the financial crisis must have been much higher than many, experts and authorities alike, initially assumed.

In the following analysis, we estimate average treatment effects of the treated (*ATT*, conditional on having experienced at least one of the five credit restriction as covered in the WIFO survey in the years 2008 and 2009) and average treatment effects (*ATE*, unconditional on the actual exposure to at least one of these credit constraints) on employment and output (proxied by sales), respectively.

Table 7.2 and Table 7.3 summarize our estimates for *ATT* and *ATE*, respectively not only for 2010 (that is, immediately after the imposition of credit constraints) but also for 2013. The analysis is carried out on the basis of difference in absolute differences and of difference in change of change rates, respectively between 2008 and 2010, and between 2008 and 2013, respectively.

Most importantly, the causal inference indicators under study signal clearly that there were most likely significant credit-limitation losses in terms of employment and output cutbacks as measured in the year following the credit restriction (2010) and, even stronger, four years later (2013).

The analysis indicates that credit-constrained non-financial companies score badly compared with their non-constrained 'twins' in terms of relative changes in employment immediately after the financial crisis (4.5 percentage points less; 2010) and relative changes in output (6.9 percentage points less; 2010) and, even more distinctively, a while later (employment: 16.6 percentage points less; output: 17.3 percentage points less; both 2013).

So far, we have analyzed the effects of credit-limitations on employment and output of those non-financial firms that actually were exposed to credit-constraints, i. e., *ATT*, within the considered periods of time (Table 7.2). From a more general welfare analysis point of view, it is of interest to know the potential effect of credit-limits on a non-financial firm that is randomly drawn from the sample, irrespective of whether it was exposed to the treatment (i. e., it was exposed to a credit limitation) or not, i. e., *ATE*. We summarize the results in Table 7.3. The 'average treatment effects' (*ATE)* are almost as large though not always as significant as the 'average treatment effects on the treated' (*ATT*). This underlines strongly that credit-limitations, if due to supply-side constraints, such as bank capital shortage ought to be viewed as a potential source of substantial welfare losses. This holds particularly true for economies with a bank-based financial system such as the Austrian economy.

However, a word of caution is needed on this point: observational studies like the presented analysis are usually confronted with the critique that the results only hold when the model applied be precisely true. Hidden (unobserved) biases not accounted for in the model may cast strong doubts on an observational study's conclusions. In order to check the sensitivity of our findings to impacts caused by unobserved covariates we apply the sensitivity analysis approach suggested by *Rosenbaum* (2002). Accordingly, the sensitivity check aims at assessing to what extent the estimated treatment effects vary when the estimated odds of receiving a particular treatment are artificially altered. Using Wilcoxon's signed rank test Rosenbaum's procedure can only be applied to matched pair studies, with the following

Table 7.2 : Treatment Effect for the Treated (ATT)

Difference in absolute differences

Period and statistic	Employment	Output[1]
t		
ATT	-1.18 *	-255286.1
Standard deviation	0.70	161065.7
95 percent confidence interval	(-2.56; 0.20)	(-570,968.90; 60,396.82)
t+1		
ATT	-2.25 ***	-690825.90 ***
Standard deviation	0.78	215899.00
95 percent confidence interval	(-3.78; -0.72)	(-1,113,980.00; -267,671.60)

Difference in change of change rates

Period and statistic	Employment	Output[1]
t		
ATT	-4.52 *	-6.91 **
Standard deviation	2.65	2.82
95 percent confidence interval	(-9.72; 0.68)	(-12.43; -1.39)
t+1		
ATT	-16.62 **	-17.35 ***
Standard deviation	6.70	6.01
95 percent confidence interval	(-29.75; -3.50)	(-29.13; -5.56)

S: WIFO calculations. - [1] Approximated by sales. - *** significant at 1 percent; ** significant at 5 percent; * significant at 10 percent; t= 2010; t+1=2013; t-1=2008; nearest neighbor: 2 matches requested.

odds relation at its center

$$\frac{1}{\Gamma} \leq \frac{\pi_j(1-\pi_k)}{\pi_k(1-\pi_j)} \leq \Gamma \quad for\, all\; j,k \;\; with \;\; x_j = x_k \, . \tag{7.1}$$

Relation (7.1) states that given two units, j and k, sharing both the same observed covariates x and the same odds of receiving treatment π, then Γ equal 1. The latter means that the study is free of hidden biases. If $\Gamma = 2$, then two units that appear similar, that have the same x, could differ in their odds of receiving the treatment by as much as a factor of 2, so one unit might be twice as likely as the other to receive treatment due to unobservable heterogeneity (*Guo and Fraser*, 2015, pp. 358).

Table 7.3: Average Treatment Effect (ATE)

Difference in absolute differences

Period and statistic		Employment	Output[1]
t	ATE	-1.61	-111068.70
	Standard deviation	1.35	177550.40
	95 percent confidence interval	(-4.25; 1.03)	(-459,061.10; 236,923.70)
t+1			
	ATE	-2.19	-507043.00 **
	Standard deviation	1.36	216230.30
	95 percent confidence interval	(-4.86; 0.48)	(-930,846.50; -83,239.47)

Difference in change of change rates

Period and statistic		Employment	Output[1]
t			
	ATE	-4.55 *	-6.76 ***
	Standard deviation	2.45	2.58
	95 percent confidence interval	(-9.35; 0.25)	(-11.81; -1.70)
t+1			
	ATE	-9.43 *	-14.41 ***
	Standard deviation	5.01	4.56
	95 percent confidence interval	(-19.25; 0.39)	(-23.35; -5.48)

S: WIFO calculations. - [1] Approximated by sales. - *** significant at 1 percent; ** significant at 5 percent; * significant at 10 percent; t= 2010; t+1=2013; t-1=2008; nearest neighbor: 2 matches requested.

We report the results of Rosenbaum's sensitivity analysis applied to our selection model in the Appendix (see, Appendix A, Table A2). We leave it to our readers to judge for themselves whether the estimated treatment effects reported in this study can be considered to be sufficiently robust to a plausible range of selection biases[24]. Yet, we dare to hope that our results as preliminary as they may be – which is readily conceded in the section heading – will eventually prove to be good enough for giving, researchers and practitioners alike, at the very least some food for thought.

[24] A thorough introduction to statistical sensitivity analysis in observational studies is given in *Guo and Fraser* (2015).

8 Concluding Remarks

At the center of this study has been the identification of the empirically and, thus, policy relevant causes of financial contraction. To be specific, our focus has been on identifying the "pure" credit-limiting supply effect in order to assess its importance as it is claimed in the theoretical literature.

In order to resolve the underlying identification problem in empirical credit market analysis we apply advanced methods of the matching approach. To the best of our knowledge our work is the first that uses matching techniques to resolve the identification problem in conjunction with credit supply and credit demand. Most importantly, advanced matching techniques allow us to identify the causal effect of credit constraints on non-financial businesses. By comparing the effects of credit constraints on businesses that were exposed to credit limits to those that were not exposed to credit limits but were otherwise identical to the credit-constrained firms prior to the treatment, the selection problem can be solved and the average treatment on the treated is computed as the difference in outcome between those two groups.

The empirical analysis is based on a unique, interlinked micro-dataset drawn from banks and business firms of the Austrian economy that covers the pre-crisis period from 2004 onwards, the crisis period 2008 and 2009 and the post-crisis period from 2010 onwards (until 2013). This allows for an in-depth analysis of the complex relationship between banks and their borrowing business customers at the individual level under changing environments unfolding over a time-span of a decade. The compilation of this high-quality micro-dataset has been made possible by a joint venture of the Austrian Institute of Economic Research (WIFO) and the Austrian National Bank (OeNB) in close cooperation with the Kreditschutzverband von 1870 (KSV 1870), the country's largest creditors' protection association. This unique database provides the basis for resolving the so-called "missing data problem" that has as yet impeded the distinct determination of whether the contraction of credit is due to shrinking loan supply or shrinking loan demand.

By applying propensity score analysis we are able to show that bank credit limits are not only imposed due to unfavorable demand-side factors (i. e., bad debtors, poor information, weak growth environment, high exposure to external shocks) but also due to supply-side weaknesses such as poor bank capitalization. The overall-picture of our findings even fuels speculation that the average Austrian non-financial company might have been running a much higher credit-limitation risk during the financial crisis and thereafter as many, experts and authorities alike, initially assumed and, what's more, still refuse to believe.

The analysis also indicates that actually credit-constrained non-financial companies score badly compared against their non-constrained 'twins' in terms of employment and output growth immediately after the financial crisis (2010) and, even more distinctively, a while later (2013).

To sum up, it can be stated that our findings corroborate strongly the view that credit-limitations if due to supply-side constraints such as bank capital shortages ought to be viewed as a (potential) key source of substantial welfare losses. This holds particularly true for economies burdened with a rather inefficient and weakly capitalized banking system such as the Austrian economy.

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Appendix A

Table A1: Variable list

ekq_b_before	Bank capital over total assets, average 2004 to 2007
rating_before	KSV1870-ratings, average 2004 to 2007
gruendksv	Company age according KSV1870 databank
wachs_before	Growth rate of regional domestic product, arithmetic mean 2004 to 2007
opentt	Binary indicator for international business orientation, 1 = foreign; 0=domestic
onace_A	Agriculture, forestry, fishing
onace_B	Mining and quarrying
onace_C	Manufacturing
onace_DE	Electricity, gas, steam, water supply
onace_F	Construction
onace_G	Wholesale and retail trade
onace_H	Transportation and storage
onace_I	Accommodation and food service activities
onace_J	Information and communication
onace_K	Financial and insurance activities
onace_L	Real estate activities
onace_M	Professional, scientific and tech. activities
onace_N	Administrative and support service activities
onace_O	Public administration and defense
onace_P	Education
onace_Q	Human health and social work activities
onace_R	Arts, Entertainment and recreation
onace_S	Other service activities
onace_T	Activities of households
t_wifo	Exposed to at least one of the following credit limits (1): (a) bank financing not granted, (b) amount of bank credit lowered, (c) bank financing not extended, (d) credit line reduced, (e) collateral requirement increased; (0) full funding provided
t1_wifo	Bank financing not granted (1=exposed; 0=full funding provided)
t2_wifo	Amount of bank credit lowered (1=exposed; 0=full funding provided)
t3_wifo	Bank financing not extended (1=exposed; 0=full funding provided)
t4_wifo	Credit line reduced (1=exposed; 0=full funding provided)
t5_wifo	Collateral requirement increased (1=exposed; 0=full funding provided)
ums_w_diff	Sales, difference in change of change rates between 2008 and 2013
besch_w_diff	Employment, difference in change of change rates between 2008 and 2013
ums_w_adiff	Sales, difference in absolute differences between 2008 and 2013
besch_w_adiff	Employment, difference in absolute differences between 2008 and 2013
ums_w_diffb	Sales, changes of change rates between 2008 and 2010
besch_w_diffb	Employment, changes of change rates between 2008 and 2010
ums_w_adiffb	Sales, changes of absolute differences between 2008 and 2010
besch_w_adiffb	Employment, changes of absolute differences between 2008 and 2010

S: WIFO calculations, KSV 1870, OeNB.

Table A2: Sensitivity analysis due to Rosenbaum (2002)

ums_w_diff

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.00	0.00	-16.12	-16.12	-21.76	-10.47
1.1	0.00	0.00	-18.21	-13.97	-24.17	-8.00
1.2	0.00	0.00	-20.26	-11.95	-26.51	-5.68
1.3	0.00	0.00	-22.28	-9.95	-28.46	-3.68
1.4	0.00	0.02	-24.08	-8.13	-30.20	-1.82
1.5	0.00	0.05	-25.85	-6.37	-31.83	-0.18
1.6	0.00	0.10	-27.45	-4.70	-33.43	1.42
1.7	0.00	0.19	-28.81	-3.18	-34.92	2.97
1.8	0.00	0.30	-30.19	-1.83	-36.36	4.45
1.9	0.00	0.43	-31.44	-0.61	-37.83	5.92
2	0.00	0.56	-32.63	0.63	-39.26	7.47

besch_w_diff

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.00	0.00	-13.04	-13.04	-19.19	-6.93
1.1	0.00	0.00	-15.53	-10.63	-21.73	-4.29
1.2	0.00	0.02	-17.56	-8.35	-23.89	-1.96
1.3	0.00	0.05	-19.76	-6.45	-25.83	0.00
1.4	0.00	0.13	-21.67	-4.40	-27.94	2.38
1.5	0.00	0.25	-23.33	-2.52	-29.55	4.17
1.6	0.00	0.40	-25.00	-0.79	-31.19	5.97
1.7	0.00	0.56	-26.39	0.56	-33.00	7.69
1.8	0.00	0.70	-27.92	2.38	-34.17	9.31
1.9	0.00	0.81	-29.17	3.71	-35.54	11.11
2	0.00	0.89	-30.32	5.00	-36.69	12.50

ums_w_adiff

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.00	0.00	-278,000	-278,000	-389,269	-171,605
1.1	0.00	0.00	-321,246	-235,500	-445,000	-130,589
1.2	0.00	0.00	-361,500	-200,000	-498,500	-97,606
1.3	0.00	0.00	-397,000	-161,500	-555,000	-62,252
1.4	0.00	0.02	-442,554	-132,500	-612,000	-32,186
1.5	0.00	0.05	-482,249	-106,625	-667,768	-2,425
1.6	0.00	0.10	-527,298	-79,720	-720,500	24,500
1.7	0.00	0.19	-565,500	-55,255	-781,700	50,000
1.8	0.00	0.31	-611,530	-32,283	-835,930	75,731
1.9	0.00	0.44	-655,054	-10,000	-896,000	107,248
2	0.00	0.57	-691,000	10,500	-952,815	134,000

S: WIFO calculations. - Γ: log odds of differential assignment due to unobserv ed factors; sig+: upper bound significance lev el; sig-: lower bound significance lev el; t-hat+: upper bound Hodges-Lehmann point estimate; t-hat-: lower bound Hodges-Lehmann point estimate; CI+ upper bound confidence interv al (a=.90); CI-: lower bound confidence interv al (a=.90).

Table A2/continuoued: Sensitivity analysis due to Rosenbaum (2002)

besch_w_adiff

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.00	0.00	-1.50	-1.50	-2.00	-0.50
1.1	0.00	0.00	-1.50	-1.00	-2.50	-0.50
1.2	0.00	0.01	-2.00	-1.00	-2.50	0.00
1.3	0.00	0.05	-2.00	-0.50	-3.00	0.00
1.4	0.00	0.12	-2.50	-0.50	-3.50	0.00
1.5	0.00	0.24	-2.50	-0.50	-3.50	0.50
1.6	0.00	0.38	-3.00	0.00	-4.00	0.50
1.7	0.00	0.54	-3.00	0.00	-4.00	0.50
1.8	0.00	0.68	-3.50	0.00	-4.50	1.00
1.9	0.00	0.80	-3.50	0.50	-4.50	1.00
2	0.00	0.88	-3.50	0.50	-5.00	1.00

ums_w_diffb

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.01	0.01	-4.96	-4.96	-8.13	-1.76
1.1	0.00	0.03	-6.21	-3.64	-9.57	-0.41
1.2	0.00	0.10	-7.29	-2.55	-10.72	0.74
1.3	0.00	0.23	-8.44	-1.47	-11.88	1.80
1.4	0.00	0.41	-9.51	-0.46	-13.03	2.85
1.5	0.00	0.59	-10.41	0.42	-14.06	3.81
1.6	0.00	0.75	-11.28	1.27	-15.04	4.62
1.7	0.00	0.86	-12.15	2.07	-15.93	5.49
1.8	0.00	0.93	-13.02	2.84	-16.83	6.36
1.9	0.00	0.97	-13.79	3.56	-17.61	7.09
2	0.00	0.98	-14.55	4.22	-18.41	7.80

besch_w_diffb

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.02	0.02	-4.17	-4.17	-8.27	-0.25
1.1	0.00	0.08	-5.60	-2.95	-9.52	0.00
1.2	0.00	0.20	-7.06	-1.50	-10.71	1.57
1.3	0.00	0.38	-8.33	0.00	-11.90	2.86
1.4	0.00	0.57	-9.52	0.00	-12.72	4.14
1.5	0.00	0.74	-10.24	1.12	-14.06	5.00
1.6	0.00	0.86	-11.29	2.20	-15.34	6.08
1.7	0.00	0.93	-12.32	3.23	-16.27	6.94
1.8	0.00	0.97	-12.71	4.09	-16.67	7.71
1.9	0.00	0.99	-13.89	4.81	-17.27	8.33
2	0.00	0.99	-14.59	5.56	-18.33	9.52

S: WIFO calculations. - Γ: log odds of differential assignment due to unobserv ed factors; sig+: upper bound significance lev el; sig-: lower bound significance lev el; t-hat+: upper bound Hodges-Lehmann point estimate; t-hat-: lower bound Hodges-Lehmann point estimate; CI+ upper bound confidence interv al (a=.90); CI-: lower bound confidence interv al (a=.90).

Table A2/continuoued: Sensitivity analysis due to Rosenbaum (2002)

ums_w_adiffb

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.00	0.00	-117,738	-117,738	-185,000	-61,505
1.1	0.00	0.00	-143,000	-95,000	-215,765	-41,500
1.2	0.00	0.01	-167,233	-75,000	-245,097	-21,500
1.3	0.00	0.04	-191,502	-57,734	-274,613	-3,946
1.4	0.00	0.10	-214,823	-42,108	-301,376	12,723
1.5	0.00	0.21	-236,790	-26,442	-329,500	28,500
1.6	0.00	0.35	-259,164	-13,021	-359,000	43,665
1.7	0.00	0.50	-280,657	224	-387,000	57,500
1.8	0.00	0.65	-301,000	12,546	-417,618	72,500
1.9	0.00	0.77	-322,000	24,500	-447,476	86,600
2	0.00	0.86	-343,456	35,500	-475,613	100,177

besch_w_adiffb

Γ	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	0.06	0.06	-0.50	-0.50	-0.50	0.00
1.1	0.01	0.17	-0.50	0.00	-1.00	0.00
1.2	0.00	0.35	-0.50	0.00	-1.00	0.50
1.3	0.00	0.55	-0.50	0.00	-1.00	0.50
1.4	0.00	0.73	-1.00	0.00	-1.00	0.50
1.5	0.00	0.86	-1.00	0.00	-1.50	0.50
1.6	0.00	0.93	-1.00	0.50	-1.50	0.50
1.7	0.00	0.97	-1.00	0.50	-1.50	1.00
1.8	0.00	0.99	-1.00	0.50	-1.50	1.00
1.9	0.00	1.00	-1.50	0.50	-2.00	1.00
2	0.00	1.00	-1.50	0.50	-2.00	1.00

S: WIFO calculations. - Γ: log odds of differential assignment due to unobserv ed factors; sig+: upper bound significance lev el; sig-: lower bound significance lev el; t-hat+: upper bound Hodges-Lehmann point estimate; t-hat-: lower bound Hodges-Lehmann point estimate; CI+ upper bound confidence interv al (a=.90); CI-: lower bound confidence interv al (a=.90).

Table A3: Descriptive Statistics of Inter-linked Dataset for untreated non-financial Enterprises

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Rating 2004 to 2007	654	297	54	288	177	509
Rating 2008 to 2009	654	286	56	273	167	514
Rating 2010 to 2013	654	291	52	277	205	490
Sales 2008, in €	578	3,796,215	15,700,000	1,000,000	0	275,000,000
Sales 2010, in €	580	3,808,013	15,700,000	1,000,000	0	286,000,000
Sales 2013, in €	585	4,326,506	18,100,000	1,136,583	0	313,000,000
Employment 2008, in persons	630	20	69	8	0	1,306
Employment 2010, in persons	635	21	68	8	0	1,192
Employment 2013, in persons	641	21	63	9	0	1,021
Local growth environment 2004 to 2007[1]	654	3.3	0.9	3.3	0.3	5.8
Local growth environment 2008 to 2009[1]	654	-1.4	1.3	-1.5	-4.5	2.0
Local growth environment 2010 to 2013[1]	654	1.7	0.7	1.5	-0.8	3.8
Company age	654	39	35	29	10	564

S: WIFO calculations. - [1] Regional domestic product, arithmetic mean.

Table A4: Descriptive Statistics of Inter-linked Dataset for treated non-financial Enterprises

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Rating 2004 to 2007	294	325	67	315	199	550
Rating 2008 to 2009	294	320	73	303	197	567
Rating 2010 to 2013	294	324	66	314	215	539
Sales 2008, in €	272	2,325,874	4,223,774	900,000	38	37,000,000
Sales 2010, in €	275	2,176,246	4,348,331	824,000	0	46,800,000
Sales 2013, in €	274	2,224,376	4,481,918	870,024	0	43,000,000
Employment 2008, in persons	288	16	28	8	0	285
Employment 2010, in persons	290	15	27	8	0	324
Employment 2013, in persons	290	15	27	8	0	309
Local growth environment 2004 to 2007[1]	294	3.1	0.8	3.0	0.3	5.5
Local growth environment 2008 to 2009[1]	294	-1.5	1.2	-1.5	-4.5	2.0
Local growth environment 2010 to 2013[1]	294	1.6	0.7	1.5	-0.5	3.8
Company age	294	34	26	25	10	156

S: WIFO calculations. - [1] Regional domestic product, arithmetic mean.

Table A5: Descriptive Statistics of Inter-linked Dataset for treated non-financial Enterprises: Bank Financing not granted

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Non financial enterprises						
Rating 2004 to 2007	56	351	83	332	220	544
Rating 2008 to 2009	56	340	85	322	203	563
Rating 2010 to 2013	56	348	81	331	244	539
Sales 2008, in €	51	2,817,171	4,607,343	1,500,000	65,000	25,700,000
Sales 2010, in €	52	2,296,316	4,413,588	858,500	51,478	29,700,000
Sales 2013, in €	52	2,352,024	4,888,437	1,250,000	65,000	34,200,000
Employment 2008, in persons	54	17	25	10	0	160
Employment 2010, in persons	55	14	15	8	0	71
Employment 2013, in persons	55	15	17	7	0	77
Local growth environment 2004 to 2007[1]	56	3.0	1.0	3.0	0.3	4.8
Local growth environment 2008 to 2009[1]	56	-1.2	1.2	-1.3	-4.0	2.0
Local growth environment 2010 to 2013[1]	56	1.6	0.8	1.5	-0.5	3.8
Company age	56	28	15	24	10	72

S: WIFO calculations. - [1] Regional domestic product, arithmetic mean.

Table A6: Descriptive Statistics of Inter-linked Dataset for treated non-financial Enterprises: Amount of Bank Credit lowered

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Non financial enterprises						
Rating 2004 to 2007	83	330	66	315	199	550
Rating 2008 to 2009	83	321	79	298	197	567
Rating 2010 to 2013	83	326	66	309	227	506
Sales 2008, in €	76	2,492,000	3,919,360	1,275,878	38	25,700,000
Sales 2010, in €	77	2,450,582	4,124,243	1,000,000	0	29,700,000
Sales 2013, in €	77	2,596,714	4,817,042	1,100,000	0	34,200,000
Employment 2008, in persons	81	16	19	10	0	140
Employment 2010, in persons	82	15	17	10	0	110
Employment 2013, in persons	81	16	18	10	0	130
Local growth environment 2004 to 2007[1]	83	3.2	0.9	3.0	1.0	5.5
Local growth environment 2008 to 2009[1]	83	-1.5	1.3	-1.5	-4.5	2.0
Local growth environment 2010 to 2013[1]	83	1.5	0.6	1.5	0.0	3.3
Company age	83	32	24	24	10	125

S: WIFO calculations. - [1] Regional domestic product, arithmetic mean.

Table A7: Descriptive Statistics of Inter-linked Dataset for treated non-financial Enterprises: Bank Financing not extended

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Non financial enterprises						
Rating 2004 to 2007	23	325	73	293	231	474
Rating 2008 to 2009	23	329	84	311	224	489
Rating 2010 to 2013	23	322	58	310	242	441
Sales 2008, in €	21	4,596,773	8,002,522	800,000	28,000	25,200,000
Sales 2010, in €	21	4,918,353	10,600,000	600,000	29,000	46,800,000
Sales 2013, in €	20	4,630,807	10,500,000	557,500	49,000	43,000,000
Employment 2008, in persons	22	33	59	7	0	242
Employment 2010, in persons	22	33	71	6	0	324
Employment 2013, in persons	22	31	69	5	0	309
Local growth environment 2004 to 2007[1]	23	2.8	0.7	2.8	1.5	4.5
Local growth environment 2008 to 2009[1]	23	-1.6	1.2	-1.5	-4.0	1.5
Local growth environment 2010 to 2013[1]	23	1.6	0.5	1.5	0.5	2.3
Company age	23	42	33	26	12	133

S: WIFO calculations. - [1] Regional domestic product, arithmetic mean.

Table A8: Descriptive Statistics of Inter-linked Dataset for treated non-financial Enterprises: Credit Line reduced

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Non financial enterprises						
Rating 2004 to 2007	66	321	58	320	209	523
Rating 2008 to 2009	66	320	65	310	203	538
Rating 2010 to 2013	66	329	61	319	235	539
Sales 2008, in €	63	2,804,968	4,845,711	1,057,000	38	25,700,000
Sales 2010, in €	63	2,506,478	4,490,940	1,000,000	36	29,700,000
Sales 2013, in €	62	2,286,356	4,721,923	1,066,500	28	34,200,000
Employment 2008, in persons	65	17	21	10	0	110
Employment 2010, in persons	65	16	20	9	0	102
Employment 2013, in persons	65	14	18	8	0	97
Local growth environment 2004 to 2007[1]	66	2.9	0.7	2.8	1.5	4.5
Local growth environment 2008 to 2009[1]	66	-1.5	1.2	-1.0	-4.0	1.5
Local growth environment 2010 to 2013[1]	66	1.7	0.7	1.5	-0.5	3.3
Company age	66	36	31	26	11	133

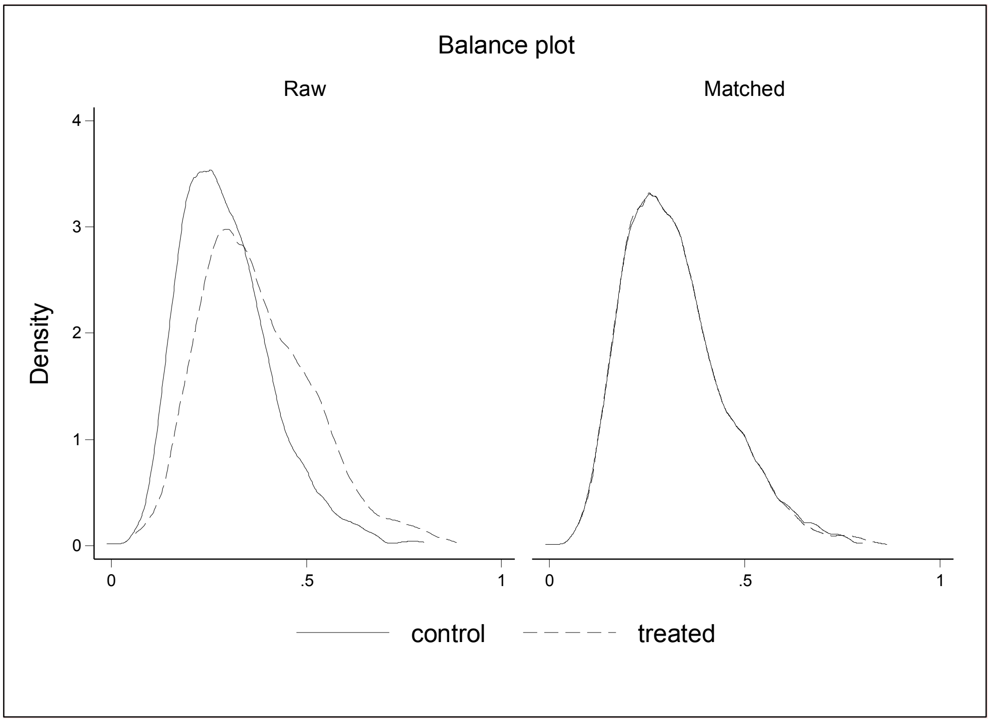
S: WIFO calculations. - [1] Regional domestic product, arithmetic mean.

Table A9: Descriptive Statistics of Inter-linked Dataset for treated non-financial Enterprises: Collateral Requirement increased

	Observations	Mean	Std.dev.	Median	Minimum	Maximum
Non financial enterprises						
Rating 2004 to 2007	246	322	64	313	199	523
Rating 2008 to 2009	246	318	70	304	197	567
Rating 2010 to 2013	246	321	64	310	215	539
Sales 2008, in €	229	2,287,655	4,352,496	900,000	38	37,000,000
Sales 2010, in €	231	2,224,673	4,628,398	900,000	0	46,800,000
Sales 2013, in €	230	2,286,253	4,778,294	870,024	0	43,000,000
Employment 2008, in persons	241	15	29	8	0	285
Employment 2010, in persons	242	15	29	8	0	324
Employment 2013, in persons	242	15	28	8	0	309
Local growth environment 2004 to 2007[1]	246	3.1	0.8	3.0	0.3	4.8
Local growth environment 2008 to 2009[1]	246	-1.4	1.1	-1.5	-4.0	2.0
Local growth environment 2010 to 2013[1]	246	1.6	0.7	1.5	-0.5	3.8
Company age	246	33	25	24	10	156

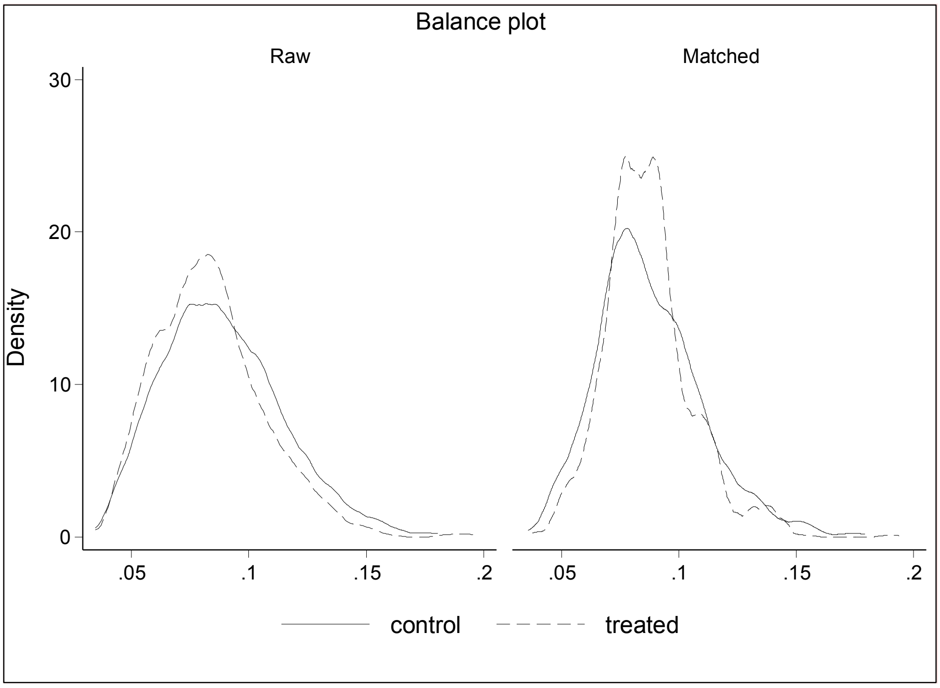
S: WIFO calculations. - [1] Regional domestic product, arithmetic mean.

Figure A1: Balancing Property – Propensity Score



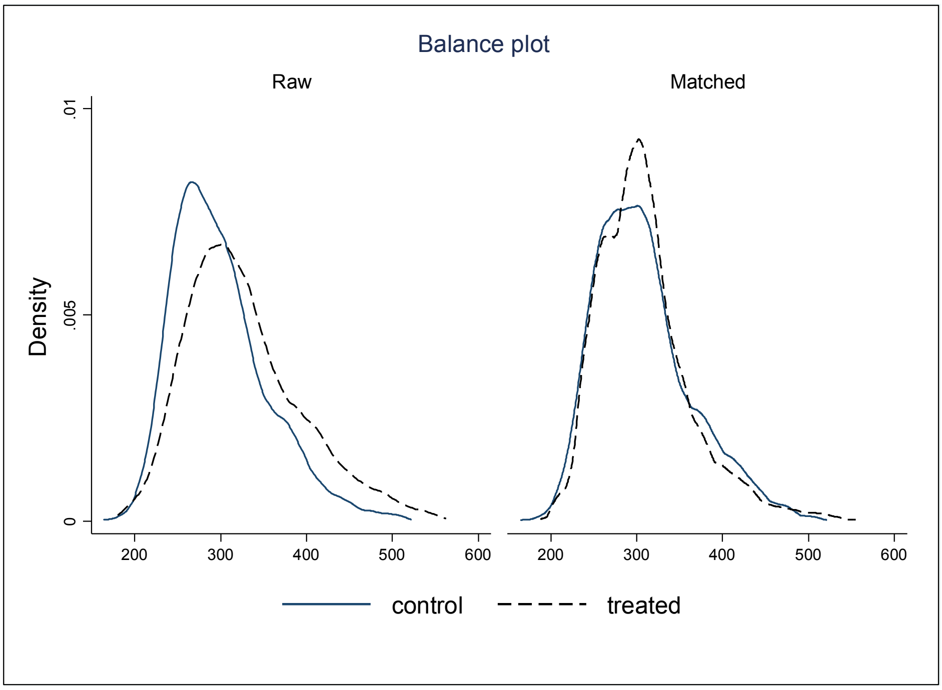
S: WIFO calculations.

Figure A2: Balancing Property – Bank Capital Ratio, average 2004 to 2007



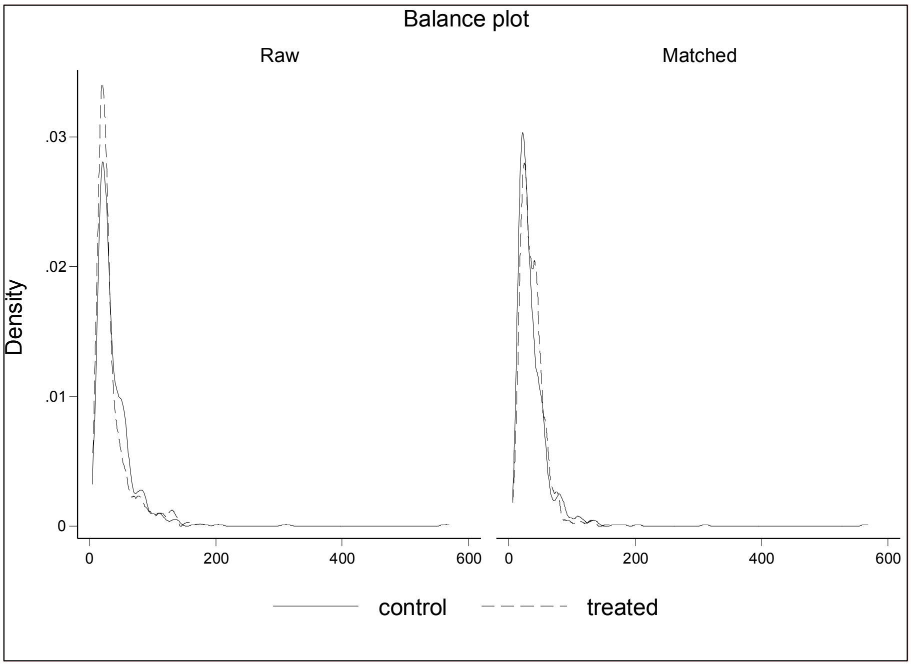
S: WIFO calculations.

Figure A3: Balancing Property – Rating Scores due to KSV 1870, average 2004 to 2007



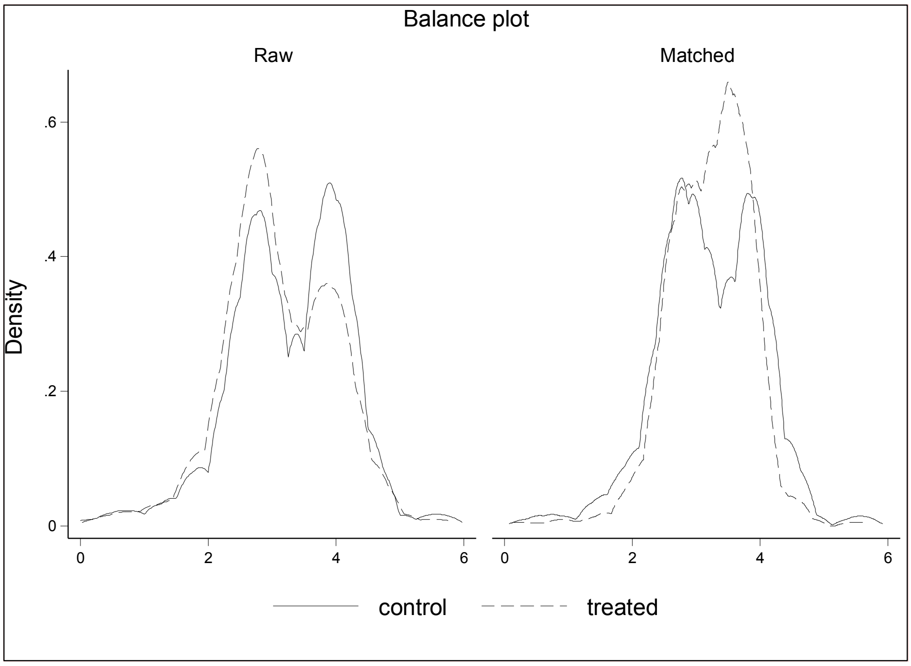
S: WIFO calculations.

Figure A4: Balancing Property – Company age



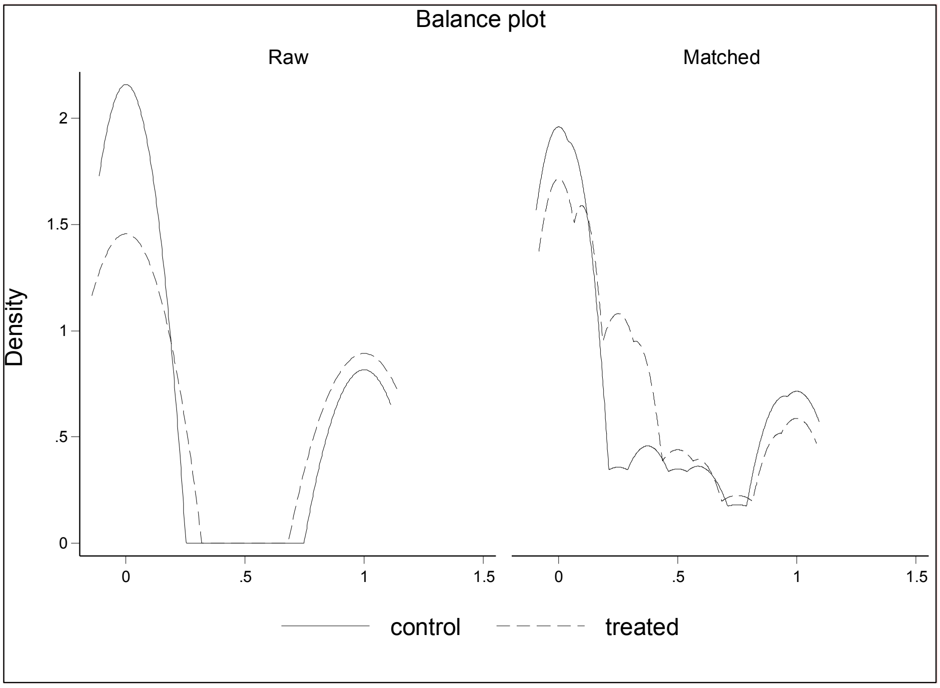
S: WIFO calculations.

Figure A5: Balancing Property – Local Growth environment of non-financial firms, average 2004 to 2007



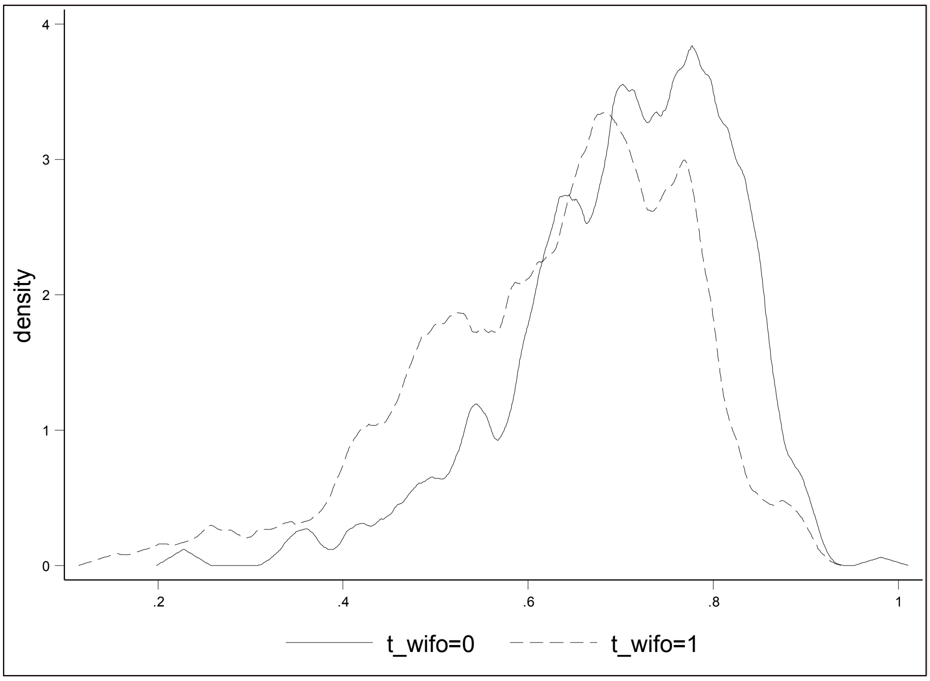
S: WIFO calculations.

Figure A6: Balancing Property – Range of business orientation (0 domestic; 1 international), average 2004 to 2007



S: WIFO calculations.

Figure A7: Overlap of propensity score; control versus treated



S: WIFO calculations.

Appendix B: The firm survey

The survey „Unternehmenskredite in der Finanzkrise / enterprise loans during the times of the financial crisis" was primarily conducted as mail survey. The selected enterprises received the paper questionnaire by mail and were asked to return the questionnaire using the enclosed pre-paid envelope. However, the enterprises also had the possibility to participate in the survey online. For this purpose the respective URL and individualized passwords were printed on the cover letters or the questionnaire. A few enterprises returned the questionnaire by fax or returned a scanned copy via email.

Figure B1 presents the questionnaire.

The gross sample was selected on the basis of the database provided by the KSV. Only firms with no more than one (main) bank connection were selected. Furthermore, the (main) bank's headquarters had to be located in the same district or in a neighboring district of the selected firm, in order to minimize problems of causal interference. The gross sample consisted of 11,216 enterprises.

The survey was conducted in two waves. Prior to the first wave the questionnaire was sent to a small randomly selected sub sample of firms in order to test the suitability of the survey (pre-test) at the end of July 2015. The pre-test was conducted successfully, no changes had to be applied to the questionnaire. As the pre-test questionnaire was identical to the final questionnaire, project-management decided to include the data collected during the pre-test into the data collected during regular fieldwork. The first wave of the survey was fielded in the fall of 2015. Beginning of October the questionnaire was sent to the enterprises of the gross sample. The second wave was posted at the end of November 2015: a reminder including a replacement questionnaire was sent to all firms (including those from the pre-test group) that had not yet reported back at this time.

Of the 11,216 enterprises of the gross sample 589 enterprises were selected for the pre-test sample. 323 (2.9 percent) enterprises were eliminated from the gross sample because they were no longer active or not traceable. The adjusted gross sample consisted of 10,893 enterprises, of which 675 (6.2 percent) rejected participation in the survey. 1,627 (14.9 percent) participated in the survey. 8,564 (78.6 percent) enterprises did not give any feedback. Most of the questionnaires were returned by mail (80.1 percent). 15.3 percent answered online, 4.5 percent by fax and 0.1 percent by Email. Of the 1,627 returned questionnaired 64 (3.9 percent of the answers) had to be excluded from the study, because the identifier on the cover page was removed by the answering enterprise thus making it impossible to link the questionnaire to its corresponding firm in the sample.

For reference Table B1 to Table B4 provide the descriptive statistics for questions 1 to 6 of the raw net sample (including anonymous answers).

Figure B1: The survey

1 In welchem Jahr wurde Ihr Unternehmen gegründet?

_________ Jahr

2 Wie viele Beschäftigte hatte ihr Unternehmen 2008, 2010 und 2014 jeweils zum Jahresende?

Wurde Ihr Unternehmen nach 2008 gegründet, geben Sie bitte die Beschäftigten zum Ende des Gründungsjahres an.

Beschäftigte

2008: _______________ Personen

2010: _______________ Personen

2014: _______________ Personen

3 Bitte geben Sie den Umsatz ihres Unternehmens in den Jahren 2008, 2010 und 2014 an.

Wurde Ihr Unternehmen nach 2008 gegründet, geben Sie bitte den Umsatz im Gründungsjahr an.

Umsatz

2008: _______________ €

2010: _______________ €

2014: _______________ €

4 Hatten Sie in den Jahren 2008-2010 Schwierigkeiten bei der Finanzierung Ihres Unternehmens durch Ihre Bank (Hausbank)?

NEIN, weil …
- ☐ kein Finanzierungsbedarf
- ☐ die Bank die gewünschte Finanzierung gewährte
- ☐ Finanzierungsbedarf anderweitig gedeckt
- ☐ sonstiger Grund: ____________________________

JA, weil …
- ☐ gewünschte Finanzierung gar nicht erhalten
- ☐ gewünschte Finanzierung nur zum Teil erhalten
- ☐ Laufzeit bestehender Finanzierung nicht verlängert wurde
- ☐ Kreditlinie gekürzt wurde
- ☐ mehr Sicherheiten verlangt wurden
- ☐ sonstiger Grund: ____________________________

5 Wenn Sie in den Jahren 2008-2010 Schwierigkeiten bei der Finanzierung hatten, welche Auswirkungen hatte das auf Ihr Unternehmen?

- ☐ Einschränkungen in der „normalen" Unternehmenstätigkeit
- ☐ Ersatzinvestitionen konnten nicht durchgeführt werden
- ☐ Erweiterungsinvestitionen konnten nicht getätigt werden
- ☐ sonstige Auswirkungen: ____
- ☐ keine Auswirkungen

6 Wie hat sich die Finanzierungssituation Ihres Unternehmens durch Ihre Bank (Hausbank) nach 2010 entwickelt?

- ☐ verbessert
- ☐ weitgehend gleich geblieben
- ☐ verschlechtert

7 Im Frühjahr 2016 werden wir einen ausführlicheren Fragebogen zu Geschäftsbeziehungen der österreichischen Unternehmen mit Ihren Banken versenden. Dürfen wir Ihnen diesen Fragebogen zusenden?

- ☐ Ja
- ☐ Nein

Herzlichen Dank für Ihre Bemühungen!

Table B1: Descriptive Statistics for Questions concerning Company age, Employment and Sales

Variable	Observations	Mean	Std. dev.
Founding year	1,625	1,973	44
Employees 2008	1,544	22.0	105
Employees 2010	1,551	22.4	110
Employees 2014	1,576	23.2	112
Sales 2008	1,415	6,955,725	106,000,000
Sales 2010	1,426	6,558,710	94,700,000
Sales 2014	1,443	6,691,792	84,400,000

S: WIFO-survey.

Table B2: Descriptive Statistics for Question 4: Did you face Bank Financing Problems in 2008 to 2010 (Hausbank)?

	Number of firms	Percentage shares
Bank financing problems during 2008 to 2010: no	1,288	80.0
No need for financing	589	36.6
Bank provided financing	680	42.3
Financing needs were covered using other sources	54	3.4
Other	21	1.3
Bank financing problems during 2008 to 2010 : yes	321	20.0
Bank financing not granted	61	3.8
Amount of bank credit lowered	85	5.3
Bank financing not extended	25	1.6
Credit line was reduced	71	4.4
Collateral requirement increased	261	16.2
Other	32	2.0
Total	1,609	100.0

S: WIFO-survey.

Table B3: Descriptive Statistics for Question 5: Credit Limits led to Constraints in

	Number of firms	Percentage shares
Normal business	154	49.7
Replacement investment	88	28.4
Expansion investment	149	48.1
Other	63	20.3
No constraints	34	11.0
Total	310	

S: WIFO-survey.

Table B4: Descriptive Statistics for Question 6: Development of Bank Financing Situation after 2010

	Number of firms	Percentage shares
Improved	152	10.4
Remained much the same	1,050	71.8
Deteriorated	261	17.8
Total	1,463	

S: WIFO-survey.