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Does restrictive bank lending cause lower employment growth at the firm-level or does it reflect firm characteristics that drive the deterioration of employment figures? Applying propensity score matching, we estimate the treatment effect of restrictive bank lending on employment growth. Combining balance sheet information and survey data on a firm's current and expected future business situation, we rule out the impact of firm heterogeneity. We find that credit constraints have a significant negative effect on employment growth. Restricted firms also apply for short-time work more often, but this effect is small and not significant in all estimations.

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1 Introduction

The recent financial crisis has highlighted the effects of shocks to the banking system on real economic activity. According to this narrative, banks came under distress and had to reduce lending activities (for example because of liquidity constraints). Firms that were cut off from bank credit could not finance investment and working capital, which, in the worst case, forced them to a decrease their business activity. Thereby, bank lending ultimately affected employment at the firm-level. Objectors of this view argue that restrictive bank lending has not by itself caused a decrease in firms' employment, but only reflected the fundamentals that were the actual cause of lower employment. When firms faced a lack of demand for their products due to the worldwide slowdown of economic activity accompanying the financial crisis, they may have been forced to reduce business activity and employment.[1] Thereby, firms' creditworthiness deteriorated, which induced banks to restrict lending to them. Thus, restrictive bank lending during the financial crisis would have been only a reflection of firms' characteristics.

In general, descriptive statistics might indicate a positive relationship between bank lending and employment at the firm-level even in non-crisis periods. When policy makers and regulators are called into action against a "credit crunch", it is crucial to understand whether measures have to be taken at the bank-level (e.g. lower capital requirements for banks or bank bail-outs) or the firm-level (e.g. public guarantees for firms). Therefore, identifying to what extent employment effects are caused by restrictive bank lending is a key obstacle to overcome in the empirical analysis of credit constraints at the firm-level.

We identify a causal effect of restrictive bank lending on employment holding firm characteristics constant by applying propensity score matching. We use data for German manufacturing firms from the "EBDC Business Expectations Panel" between 2003 and 2011. We generate a survey-based measure of credit constraints and measure employment effects using year-on-year growth in the number of employees, a qualitative assessment of a firm's current workforce as well as a variable indicating that a firm is working short-

[1] This was particularly widespread among exporting firms when economic activity slowed down worldwide after the break-out of the financial crisis.

time. The propensity score estimation is based on a wide range of firm characteristics determining both a firm's creditworthiness and its employment development. Here, we are able to combine precise but backward-looking balance sheet information with a number of assessments of a firm's current and expected future business situation to fulfill the identifying assumptions of our empirical approach.

Comparing credit constrained firms to their unconstrained matches, we find that credit constrained firms show a lower year-on-year growth in employment measured twelve months after the treatment. The effect is statistically significant and robust to changes in the matching algorithm. Controlling for the demand for a firm's products during the twelve months after the treatment does not affect this result. We also find that six months after the treatment the likelihood that a firm appraises its current workforce as too large is higher for constrained than for unconstrained firms. The likelihood of short-time work is only weakly affected by restrictive bank lending, but rather driven by the demand for a firm's products.

Our analysis contributes to the research on financial intermediation and its effects on real economic activity. In the early macroeconomic literature financial intermediaries were not modeled explicitly. Instead, it was assumed that the central bank has complete control about the money supply within an economy. From this view, financial constraints can only arise from a firm's fundamentals, but not through bank lending channels. Bernanke (1983), however, presented evidence for the impact of the collapse of the financial system on borrowers, and therefore the economy as a whole, during the Great Depression 1930 to 1933. From this perspective, financial market imperfections and financial intermediation play an important role in determining the allocation of credit, and bank lending may therefore affect real economic activity. Holmstrom and Tirole (1997) provide a first theoretical model in which both bank-side and firm-side factors determine the allocation of credit.

More recent empirical studies analyzed whether real effects of financial constraints can be found at the firm-level. Beck, Demirgüc-Kunt, and Maksimovic (2005) use firm-level data from 54 different countries to show that financial obstacles are a slowdown to firm growth in terms of sales. The negative impact of financing obstacles is found to be strongest for small firms. Based on the approach of Rajan and Zingales (1998), Kroszner, Laeven, and Klingebiel (2007) argue that a “sudden negative shock” to the banking system should have severe negative effects on sectors that depend on funding from banks. Using data from 38 countries, they show that external finance dependence drives sector growth in normal times. If the country is hit by a banking crisis, however, sectors that rely heavily on bank funding experience a severe slowdown in growth relative to less external finance dependent sectors. DellAriccia, Detragiache, and Rajan (2008) confirm this finding using data for 28 countries at the 3-digit ISIC sector-level. They show that in response to a banking crisis value added, capital formation and the number of establishments grow at a slower rate in industries with a high level of dependence on external finance. Instead of looking at sector-level growth, Duchin, Ozbas, and Sensoy (2010) analyze balance sheet data for firms between July 2007 and March 2009 to identify how the recent financial crisis affected firms’ investment behaviour. They provide evidence for a supply-side shock in credit markets that hit firms in external finance dependent sectors more severely than firms that are less dependent on external finance.

If firms have to halt investment or reduce current operations because of credit constraints, this may have immediate effects on employment decisions of the firm. Duygan-Bump, Levkov, and Montoriol-Garriga (2010) use data on individuals and their main industry of occupation from the US Current Population Survey (CPS) to test this hypothesis. They match the individual information to a measure of external finance dependence for every industry and find that workers who are employed by small firms in an industry that depends strongly on external finance face a higher risk of unemployment during the recent recession following the financial crisis. They interpret this result as evidence for an impact of reduced bank lending on firms’ employment decisions during the crisis. Campello, Graham, and Harvey (2010) use data from surveys among CFOs in the US to analyze how financial constraints change a firm’s behaviour at the micro-level. Using a matching approach, they find that firms reporting financial constraints are significantly more likely to plan employment cuts than unconstrained firms. This holds during the pre-crisis period (2007Q3 ton 2008Q3) and even more so during the financial crisis in 2008Q4.

We extend the existing empirical literature in different ways. First, we use a firm's assessment of banks' lending behaviour to measure credit constraints. Therefore, our treatment variable measures bank lending more explicitly than studies based on indirect measures, such as the external finance dependence suggested by Rajan and Zingales (1998). Since a firm's perception of banks' lending behaviour might still be affected by the firm's characteristics, we use propensity score matching to rule out firm heterogeneity. Our matching approach is based on a broader set of variables combining both balance sheet information and survey-based assessment of the firm's current and expected future business. This is a significant improvement compared to the closely related study by Campello, Graham, and Harvey (2010), who only match on a small number of very general firm characteristics. Furthermore, the panel structure of our data set allows us to quantify the actual differences in employment growth while Campello, Graham, and Harvey (2010) measure employment effects using firms' plans for future employment cuts only. Since many of our variables are also available on a monthly or quarterly basis, we can perform a more detailed analysis of the time structure of the employment effects of restrictive bank lending compared to previous studies focusing on annual data. Finally, we are to the best of our knowledge the first who analyze whether firms are more likely to work short-time in response to credit constraints.

2 Empirical strategy

We aim to identify whether restrictive bank lending causes lower employment growth at the firm-level or if it only reflects firms' characteristics that are the actual cause of lower employment growth. Estimating the causal effect of restrictive bank lending on a firm's employment growth is complicated by issues arising from selection bias.

If credit constraints were randomly assigned to firms, then comparing the average employment growth rates of constrained firms $(C_i{=}1)$ to the average growth rates of unconstrained firms $(C_i{=}0)$ would provide an reliable estimate of the causal effect of restrictive bank lending. In reality, however, banks' lending behaviour to firms is not randomly assigned, but strongly driven by firm heterogeneity (in Section 4 we show that constrained and unconstrained firms differ significantly in the month before credit constraints are assigned).

At least two sorts of selection bias are inherent in the relationship between credit constraints and employment growth. On the one hand, a firm could face a slowdown in demand for its products (e.g. due to a failing business model). This induces lower production and investment activities and could also cause a slowdown in employment growth. Since the slowdown in demand also lowers the firm's creditworthiness, its likelihood of experiencing restrictive bank lending increases. The employment effects, however, would still be caused by the lower demand for the firm's products and not by restrictive bank lending. The correlation between credit constraints and employment growth would therefore overstate the employment effect of restrictive bank lending. On the other hand, young and small firms often show high rates of employment growth. Since these firms are typically more opaque than older and larger firms, they are more likely to experience restrictive bank lending. In this case the correlation between credit constraints and employment growth would understate the employment effect of restrictive bank lending.

The observed difference in average employment growth rates Y_i can be written formally as

$$\begin{aligned} &E[Y_{1i}|C_i = 1] - E[Y_{0i}|C_i = 0] \\ &= \underbrace{E[Y_{1i}|C_i = 1] - E[Y_{0i}|C_i = 1]}_{\text{Effect of restrictive bank lending}} + \underbrace{E[Y_{0i}|C_i = 1] - E[Y_{0i}|C_i = 0]}_{\text{Effect of firm heterogeneity}} \end{aligned} \tag{1}$$

where Y_{1i} and Y_{0i} denote the potential employment growth rates of constrained and unconstrained firms respectively. The identification of the causal effect of restrictive bank lending, however, relies on the average employment growth that a constrained firm would have experienced if it had not been constrained $E[Y_{0i}|C_i = 1]$, which is hypothetical and not observable.

This issue can be tackled by an approach generally referred to as selection on observables (see for example Angrist (1998)). Assuming that all firm characteristics that determine the assignment of restrictive bank lending are observed, for every credit constrained firm one could find a firm that is unconstrained but otherwise identical in all relevant characteristics. Credit constraints would then be randomly assigned given these firm characteristics. On average, the difference between constrained firms and their unconstrained matching firms could be interpreted as a causal effect of restrictive bank lending ruling out firm heterogeneity. In order to analyze the effect of credit constraints on firms' decision making, Campello, Graham, and Harvey (2010) follow this approach by matching on categories in size, ownership and credit rating as well as industry.

Compared with the standard ordinary least squares approach matching estimates rely on fewer identifying assumptions. As a non- or semi-parametric approach matching avoids potential misspecification of the functional form of $E[Y_{0i}|X_i]$. The potential bias arising from misspecification also builds up if the distribution of the covariates differs between the constraint and unconstraint firms. This is especially the case if the support differs conditional on the credit constraint, and thus, ordinary least squares estimates rely on a counter factual comparison based on extrapolation outside the common support. In contrast, matching effectively compares only comparable firms by balancing the distributions of X and restricting the samples to the common support.

We rely on a semi-parametric matching approach. For our analysis, a non-parametric matching or the identification of identical matching firms for every credit constrained firm is not achievable because we match on a larger number of firm characteristics than e.g. Campello, Graham, and Harvey (2010) and some of them are continuous, which inhibits two firms having exactly the same values in their covariates. That is why we follow Rosenbaum and Rubin (1983) by matching on a propensity score defined as

$$p(X_i) = Pr(C_i = 1 \mid X_i) \tag{2}$$

where X_i is a vector of variables covering a broad range of firm characteristics. The propensity score is the probability of being credit constrained given the set of characteristics and can be estimated using binary choice models (see Section 4). The identifying assumption in our empirical approach is referred to as the conditional independence assumption

$$(Y_{0i}, Y_{1i}) \perp C_i \mid p(X_i) \tag{3}$$

Credit constraints are randomly assigned if all relevant variables are included in X_i. Comparing each constrained firm to an unconstrained firm with a most similar propensity score leads to an estimated treatment effect that is close to the one derived from an experimental setting (Dehejia and Wahba 1999). Campello, Graham, and Harvey (2010) show results for propensity score matching on firms' size, ownership, credit rating and industry. They find that these are very similar to the results they found when applying exact matching on the categories in each of these variables.

We use different matching algorithms to calculate the treatment effect of restrictive bank lending on employment growth rates using the following formula

$$\hat{\tau} = \frac{1}{N_C} \sum_{i=1}^{N_C} (Y_i - \frac{1}{N_U} \sum_{j=1}^{N_U} Y_j) \tag{4}$$

where N_C is the number of constrained firms for which unconstrained matching firms are available and N_U is the number of unconstrained matching firms assigned to each constrained firm. Every matching firm is assigned a weight taking into account the total number of matching firms that are available for the corresponding constrained firm and considering an adjustment because we draw with replacement from the matching firms (Dehejia and Wahba (1999), Stuart (2010)).

In our baseline estimation, we identify for every constrained firm the ten unconstrained firms with the most similar propensity score. In order to ensure the quality of the matching, we determine a maximum distance of the matching firms in terms of the propensity score. We follow Austin (2011) who suggests that a caliper of 0.2σ (where σ is the standard deviation of the propensity score in the full sample) is optimal if the set of explanatory variables includes binary as well as continuous variables and the variance of the propensity score in the treatment group is twice as large as that in the control group.[2] Both conditions apply to our sample.[3] To test the robustness of our results with respect to the matching algorithm, we also apply five nearest neighbour matching, two radius matchings (with 0.2σ and 0.25σ as calipers) and a kernel-based matching.

3 Data

3.1 Databases

To combine the advantages of balance sheet information and survey-based assessments, we use data from the "EBDC Business Expectations Panel". This data set links the Bureau van Dyk Amadeus database and the Hoppenstedt database with survey data from the Ifo Business Survey.

The Amadeus data set contains final statements, balance sheet data and other firm specific information for European firms. Amadeus' covers about one million mainly not listed German firms, while its primary source for Germany is the Creditreform database. Hoppenstedt is a leading provider for German balance sheet data, covering almost 3 million financial statements for about one million firms. The public press and commercial registries are among its main data sources. It has almost full coverage of publicly available final statements in Germany.

Balance sheet data provides the most accurate information on a firm's financial condition, but it also has two disadvantages in the context of our analysis. First, balance sheet information are backward-looking. Banks, however, also assess a firm's current and expected future business to discover its creditworthiness. Such information can be drawn from a firm's order book, its interim financial statements or business plans, but not from the balance sheet. A second caveat of balance sheet data is its limitation to hard information. In banks' assessment of a firm's creditworthiness, however, soft information about the firm might also be accounted for.

[2] Rosenbaum and Rubin (1985) suggest a caliper of 0.25σ as a rule of thumb. We use this caliper in an alternative estimation.

[3] Austin (2011) further shows that this caliper removes 98% of the bias in a normally distributed covariate.

We tackle these caveats of balance sheet information by complementing them with survey data. The Ifo Business Survey is a monthly business tendency survey questioning about 5000 firms from the German manufacturing sector on their appraisal of their current and expected future business situation.[4] The Ifo Institute continuously ensures that the panel is representative for the German manufacturing sector by attracting and incorporating new firms whenever particular industry subclasses are at risk to thin out.

In the Ifo Business Survey every firm is asked explicitly for the assessment of its current business situation and its expected business over the next six months. This is an important amendment to backward-looking balance sheet information. The open character of the questions also allows a firm's appraisal to capture a lot of information beyond hard information. From the survey, we can also draw variables indicating the demand for a firm's products and a firm's employment that are not captured by its balance sheet.

We make use of the combination of survey data in three different ways. First, we derive a treatment variable indicating that a firm is credit constrained from the Ifo Business Survey (see Section 3.2). Second, the survey data provides the precise number of a firm's employees on an annual basis so that we can estimate the effect of restrictive bank lending on year-on-year employment growth at the firm-level (see Section 3.3 and 5.1). Third, the quality of our propensity score matching is increased by the inclusion of firm characteristics from survey-based assessments in addition to balance sheet information (see Section 4).

Linking the balance sheet information and the survey data is done based on the name and physical address of the firms.[5] The final dataset has a panel structure and contains 75,000 monthly observations for 2,260 firms between 2003 and 2011. A description of all variables relevant for our empirical analysis is provided in Table 1.

[4]The survey was launched in 1949 to provide a timely measure of current economic activity. For this purpose the survey data are aggregated to the Ifo Business Climate Index, which is perceived as being the most accurate and up-to-date coincident business cycle indicator in Europe. Ehrmann and Fratzscher (2005) show that the Ifo Business Climate Index is the only real economic variable of the Euro area/Germany that significantly determines the US dollar – Euro/DEM exchange rate over the period 1993-2003.

[5]When connecting annual balance sheet data to the monthly survey data we take into account the alternative fiscal years across firms. We assume that the balance sheets are available to the banks at the end of the firms' fiscal year to ensure that we closely map the balance sheet data that firms can provide to their bank.

Table 1: Variable descriptions

Variable	Description	Frequency
Treatment variable (Survey)		
Constrained	Change in perception of bank lending conditions from "accommodating" or "normal" to "restrictive" (see Section 2.1)	Varying
Outcome variables (Survey)		
Empl	Number of employees in production	Annual
Δ*Empl*	Year-on-year growth rate in the number of employees	Annual
Hcount (too large)	Too many employees for demand over the the next 12 months	Quarterly
Hcount (enough)	Enough employees for for demand over the the next 12 months	Quarterly
Hcount (too small)	Too few employees for for demand over the the next 12 months	Quarterly
Short-time	Working short-time	Quarterly
Expected employment (Survey)		
Empl expect (+)	Expecting increasing employment over next 3 months	Monthly
Empl expect (=)	Expecting no change in employment over next 3 months	Monthly
Empl expect (-)	Expecting decreasing employment over next 3 months	Monthly
Short-time expect	Expecting to work short-time in next 3 months	Quarterly
Balance sheet information		
Equity / Assets	Equity to total assets	Annual
Gross profit / Assets	Gross profit to total assets	Annual
Cash flow / Assets	Cash flow to total assets	Annual
Fixed assets / Assets	Fixed assets to total assets	Annual
Business Indicators (Survey)		
State (good)	Appraisal: Current business situation is good	Monthly
State (satisfactory)	Appraisal: Current business situation is satisfactory	Monthly
State (bad)	Appraisal: Current business situation is unsatisfactory	Monthly
Business expect (+)	Expecting improvement of business situation over the next 6 months	Monthly
Business expect (=)	Expecting no change of business situation over the next 6 months	Monthly
Business expect (-)	Expecting worsening of business situation over the next 6 months	Monthly
Demand for a firm's products (Survey)		
Demand (+)	Demand increased this month compared to the last one	Monthly
Demand (=)	Demand was unchanged this month compared to the last one	Monthly
Demand (-)	Demand decreased this month compared to the last one	Monthly
Orders (high)	Appraisal: Stock of orders relatively high	Monthly
Orders (enough)	Appraisal: Stock of orders satisfactory or enough	Monthly
Orders (too small)	Appraisal: Stock of orders too small	Monthly

3.2 Treatment definition

For the identification of a causal effect of restrictive bank lending on employment growth, we define a firm as "constrained" based on its assessment of banks' willingness to lend, which the firm reports in the Ifo Business Survey. Possible assessment categories are "restrictive", "normal" and "accommodating". For the remainder of this study, we refer to a firm as treated or constrained when it completes a transition from experiencing "normal" or "accommodating" bank lending in one survey to experiencing "restrictive" bank lending in the next one. Furthermore, we refer to t as the month in which a firm is treated. Furthermore, we refer to a firm as unconstrained when it reports "normal" or "accommodating" bank lending in one survey and does not switch to reporting "restrictive" bank lending in the next one. Because the panel is unbalanced, we do not consider cases in which answers to this question are not available for two consecutive surveys.

Due to a change in the frequency of the bank lending question in the Ifo Business Survey, we have to make an assumption on the timing of the treatment. As of 2008 the question is asked every month, which allows us to analyze the time structure of effects of credit constraints better than studies based on annual data. From 2003 to 2008, however, the question is only asked twice a year, in March and August. To exactly specify the timing of the treatment for this period, we assume that the treatment has occurred in the month right after the firm reports "normal" or "accommodating" bank lending the last time. Since we will match on the firm characteristics measured in the month preceding the treatment we make this assumption to ensure that the characteristics are not already affected by the treatment. We could alternatively assume that the treatment occurs in the month in which the firm reports "restrictive" bank lending the first time, since our empirical results are not sensitive to this assumption.

3.3 Outcome variables

In Table 2 we compare post-treatment year-on-year employment growth of credit constrained firms to employment growth of firms that remain unconstrained in the treatment month t. We compare only firms within the same month to rule out that the firms' perceptions of bank lending behaviour differ due to time-varying conditions (e.g. macroeconomic factors or reports about credit crunches in the news). Twelve months after the treatment *(t+12)* year-on-year employment growth is slightly lower for constrained firms. The difference is not statistically significant, which could be driven by the high variance of Δ*Empl*.[6]

We complement our analysis with a qualitative appraisal of the current workforce. The dummy variable *Hcount (too large)* indicates that a firm appraises its current workforce as too large given the expected demand over the next twelve months. Table 8 in Appendix B shows that this appraisal is associated with lower employment growth rates and a lower probability of positive growth rates in subsequent months. In *t+12* the fraction of firms appraising their workforce as too large is significantly higher among constrained firms than among unconstrained ones. The explicit reference to demand is a caveat of this question when it comes to the relation to credit constraints. We deal with this issue by applying regression-adjusted propensity score matching in Section 5.1.

Differences in employment growth may understate the effects of restrictive bank lending on employment because firms can also apply for short-time work in response to credit constraints. The idea of short-time work is to reduce working hours of employees instead of laying them off.[7] This labour market instrument allows firms to save costs for recruitment and on-the-job training because employees are kept tied to the firm and can easily return to full-time work.

The consideration of short-time work is an important part of our analysis because short-time work comes at costs for employees, firms and the state. First, in addition to being only partly compensated for income losses, employees working short-time might suffer from a loss of variable income components. Second, firms often top-up the government compensation of employees[8] so that they have to make payments to employees who are not working. Finally, the government partly compensates workers for the income loss,

[6]Non-parametric tests indicate that the distribution Δ*Empl* differs significantly between constrained and unconstrained firms.

[7]We provide a summary of short-time work regulations in Germany in Appendix A.

[8]These payments are often part of compromises because for the short-time work application firms need the approval of the workers' council. If no workers' council is established, all employees have to approve the short-time work.

Table 2: Descriptive statistics

	Constrained	*Unconstrained*	t-test
Constrained	1	0	
N	315	4698	
Δ*Empl*	3.57%	4.49%	0.73
N	297	4416	
Hcount (too large)	30.34%	13.52%	0.000
N	290	4394	
Short-time	28.67%	13.55%	0.000
N	279	4200	

which is a burden on public budgets.

Although in practice short-time work is mainly used as an instrument to respond to demand fluctuations, a firm could also apply for short-time work in response to credit constraints for two reasons. First, even if a firm has to reduce its workforce because it cannot finance its business activities, layoffs may not be possible immediately because of employees' protection against dismissals. In this concern, working short-time would be an opportunity to reduce working hours and cut costs immediately. Second, if a firm expects credit constraints to be only temporary, short-time work would allow employees to return to work immediately when financing is available again.

Table 2 shows that credit constrained firms are more likely than unconstrained ones to apply for short-time work in *t+12*. The difference is highly significant.

4 Propensity score matching

The correlation between credit constraints and employment variables in Section 3.3 cannot be interpreted as a causal effect of restrictive bank lending because there is potential selection bias arising from firm heterogeneity as discussed in Section 2. To rule out this bias, we apply propensity score matching based on the following procedure.

To estimate the propensity score for every firm in our sample, we first apply a probit model with the variable *Constrained* as defined in Section 3.2 as the dependent. All explanatory variables are measured in the pre-treatment month *t-1*. Since the identifying assumption in Section 2 requires potential employment growth and the assignment of restrictive bank lending to be independent conditional on relevant firm characteristics, the decision on the explanatory variables in the model is driven by the goal to capture a broad range of firm characteristics that determine both credit constraints and future employment growth.

In Table 3, all matching variables are listed. For the unmatched sample, we find significant pre-treatment differences in key firm characteristics between constrained and unconstrained firms. The right half of Table 3 shows, however, that the firms characteristics in both groups are balanced in the matched sample.[9]

First of all, we match firms on the lagged outcome variables measured in *t-1* because post-treatment differences in these variables could be a follow-up of pre-treatment differences. Δ*Empl, Hcount (too large)* and *Short-time* might also be strong indicators of the general situation of the firm and therefore its likelihood of experiencing restrictive bank lending. We also match in the logarithm of the total number of employess in *t-1* because the the size of a firm is a strong predictor of credit constraints and growth.

We complement variables that capture the past and current employment situation of a firm with a set of forward-looking employment variables, namely the expected employment growth over the next six months *(Empl expect (+)* and *Empl expect (-))* and a dummy variable indicating that a firm plans to work short-time in the next three months *(Short-time expect)*. Post-treatment differences in employment growth that are already anticipated before a firm is treated predict creditworthiness as well as future employment growth. Matching on these variables is particularly suited to reduce selection bias and to increase the validity of the estimated treatment effect.

We also match on balance sheet variables for a firm's solvency *(Equity / Assets)*, its liquidity *(Cash Flow / Assets)* and its profitability *(Gross profit / Assets)* in *t-1* because the variable distributions in the unmatched sample clearly indicate that a firm with a lower creditworthiness is more likely to be credit constrained. Moreover, *Fixed assets / Assets* can be interpreted as a firm's ability to pledge collateral, which could lower its

[9]The balancing properties are only reported for ten nearest neighbour matching for the sake of conciseness. In Section 5, we provide further details on the matching algorithm and estimate the treatment effects for alternative matching algorithms.

probability of being credit constrained. On the other hand, a high fraction of fixed assets could be interpreted as a sign of a lack of liquidity because current assets are more liquid.

When it comes to banks' decisions on granting credit to firms, the information content of credit scores based on balance sheet information is limited, especially if they are backward-looking only (see discussion in Section 3.1). The loan manager's general assessment of a firm's current and expected future business situation determines whether a firm becomes credit constrained. Since we do not have bank-level data, we match on the firm's self-assessment according to the Ifo Business survey. This does not perfectly substitute the loan officer's assessment, but it contains information about the firm beyond its balance sheet and provides a reasonable proxy for the information provided to banks.

Appraising its current state of business, a firm reports a "good" situation *(State (good))*, a "bad" situation *(State (bad))* or a "satisfactory situation". Due to its open character, this appraisal covers a lot of information about a firm's business. It is therefore a particularly strong predictor of credit constraints and a large source of bias reduction in our matching procedure. In addition to this contemporaneous appraisal, we match on a firm's business expectations for the next six months *(Business expect (+)* and *Business expect (-))* to reduce bias from forward-looking firm characteristics.

As we already claimed in Section 3.1, banks take into account the demand for a firm's products when judging on its creditworthiness. Since information about the current demand is not captured in balance sheets, we use survey data to match on a firm's demand appraised in the current month relative to the previous one *(Demand (+)* and *(Demand(-))* as well as the appraisal of its current stock of orders *(Orders (too small)* and *Orders (large))*.

In the matched sample all observable firm characteristics are balanced between treated firms and their matching firms. The assignment of restrictive bank lending can be considered as random conditional on these characteristics, which rules out that the latter drive the post-treatment differences in employment variables between constrained and unconstrained firms.

Since the validity of our results hinges on the conditional independence assumption,

Table 3: Balancing properties

	Unmatched	t-test	Bias	Matched	t-test	Bias
t-1						
Propensity score	20.77%	0.000	0.96	0.12%	0.93	0.01
$\Delta Empl$	-1.88%	0.55	0.04	1.85%	0.67	0.04
Hcount (too large)	8.12%	0.001	0.18	-0.49%	0.90	0.01
Hcount (too small)	1.34%	0.27	0.06	1.24%	0.51	0.05
Short-time	4.51%	0.02	0.12	2.62%	0.35	0.07
Empl expect (+)	0.53%	0.69	0.02	1.57%	0.42	0.07
Empl expect (-)	8.20%	0.000	0.19	0.35%	0.92	0.01
Short-time expect	3.00%	0.18	0.08	2.29%	0.48	0.06
log(Empl)	1.65%	0.83	0.01	-4.66%	0.68	0.03
State (good)	-1.69%	0.50	0.04	1.57%	0.67	0.04
State (bad)	8.38%	0.001	0.18	0.33%	0.93	0.01
Business expect (+)	3.47%	0.10	0.09	-1.46%	0.65	0.04
Business expect (-)	5.40%	0.02	0.13	1.16%	0.75	0.03
Demand (+)	1.34%	0.54	0.04	-0.39%	0.90	0.01
Demand (-)	4.38%	0.09	0.10	-1.10%	0.78	0.02
Orders (high)	-1.39%	0.46	0.05	0.73%	0.79	0.02
Orders (too small)	7.62%	0.006	0.15	0.92%	0.83	0.02
Equity / Assets	-5.19%	0.000	0.20	-0.97%	0.62	0.04
Gross profit / Assets	-5.34%	0.09	0.11	-0.35%	0.94	0.01
Cash flow / Assets	-2.65%	0.002	0.22	-0.37%	0.78	0.03
Fixed assets / Assets	-0.32%	0.78	0.02	-1.57%	0.36	0.08

and therefore on the completeness of the set of firm characteristics which we are matching on, it is important to highlight that the combination of balance sheet data, survey-based assessments of a firm's situation and in particular its expectations for future business activity and employment provides us with a strong tool to rule out that pre-treatment differences in firm characteristics drive our results.

5 Results

5.1 Treatment effect on employment growth

In our baseline specification, we construct the untreated counterfactual for every treated firm based on its ten nearest neighbours in terms of the propensity score. While a caliper of 0.2σ restricts the maximum distance of a treated and its potential matches. The estimated treatment effect of restrictive bank lending on employment growth is presented in Panel A of Table 4. Twelve months after the treatment, year-on-year employment growth is on average 6.5 percentage points smaller for credit constrained firms compared to unconstrained matching firms. The effect is statistically significant at the five per cent level. The median difference is 4.7 percentage points. We test the equality of the medians using a Sign test.[10] This non-parametric test, which is applied to account for non-normality in our data, indicates that the differences in the medians of employment growth are highly statistically significant.

To check the robustness of these findings with respect to the matching algorithm, we lower the number of nearest neighbours to five and also show results for two radius matchings and a kernel-based matching. The average treatment effect is fairly stable between 5.8 and 6.8 percentage points. We find that in all four approaches, both the average and the median treatment effect are highly statistically significant. This can be taken as evidence for a significant causal effect of restrictive bank lending on a firm's employment growth that cannot be explained by the pre-treatment firm characteristics accounted for in our propensity score estimation.

Still, potential bias in the estimated treatment effect could stem from different post-treatment developments of constrained and unconstrained firms that are not actually caused by the treatment itself. For example, differences in the demand for a firm's products can be considered as independent of restrictive bank lending. If by chance the constrained firms face less demand than unconstrained firms, our estimated treatment effects would reflect these differences rather than the causal effect of restrictive bank lending. We account for this potential bias by estimating regression-adjusted treatment effects based on the following model.

[10] In contrast to other studies, we do not use a Wilcoxon Signed-Rank test here, because we would have to assume a symmetric distribution of employment growth rates, which is not supported by the data.

Table 4

Panel A: Treatment effect (TE) for $\Delta Empl$ in *t+12*

	NN 10	NN 5	Radius (0.2σ)	Radius (0.25σ)	Epanechnikov
Average TE	-6.50%	-6.72%	-6.81%	-6.28%	-5.82%
s.e.	(0.0297)	(0.0344)	(0.0210)	(0.0200)	(0.0184)
t-test	0.03	0.05	0.001	0.002	0.002
Median TE	-4.69%	-3.22%	-5.10%	-5.21%	-5.72%
Signed-rank test	0.000	0.002	0.000	0.000	0.000
No. of treated	266	266	266	272	279
No. of matchings	1137	789	2589	2727	3123

Panel B: Regression-adjusted treatment effect (TE) for $\Delta Empl$ in *t+12*

	NN 10	NN 5	Radius (0.2σ)	Radius (0.25σ)	Epanechnikov
Average TE	-6.45%	-6.82%	-6.67%	-6.12%	-5.56%
s.e.	(0.0319)	(0.0370)	(0.0225)	(0.0213)	(0.0197)
t-test	0.04	0.07	0.003	0.004	0.005
No. of treated	266	266	266	272	279
No. of matchings	1137	789	2589	2727	3123

$$\begin{aligned} \Delta Empl_{t+12} &= \beta_0 + \beta_1 Constrained \\ &+\beta_2 \overline{Demand(+)} + \beta_3 \overline{Demand(-)} \\ &+\beta_4 \overline{Orders(high)} + \beta_5 \overline{Orders(toosmall)} \end{aligned} \tag{5}$$

where four monthly demand indicators are averaged over the twelve months between the treatment and the measurement of the outcome variable $\Delta Empl_{t+12}$. Panel B of Table 4 shows that the regression-adjusted average treatment effects only slightly differ from the unadjusted ones. The employment effect is above six percentage points except in the Kernel-based setup and is highly significant throughout.

To double-check our findings based on the quantitative measure $\Delta Empl$, we test for employment effects of credit constraints using the qualitative variable *Hcount (too large)*, which is available on a quarterly basis. We consider the treatment effect in this variable in *t+3, t+6* and *t+9* , since it is a forward looking indicator referring to employment changes about 12 months ahead (see Table 8 in Appendix B.)

Table 5

Panel A: Treatment effect (TE) for *Hcount*

	t+3	*t+6*	*t+9*
Average TE	7.17%	9.13%	6.36%
s.e.	(0.0275)	(0.0274)	(0.0267)
t-test	0.009	0.001	0.02
No. of treated	290	287	280
No. of matchings	846	844	832

Panel B: Regression-adjusted treatment effect (TE) for *Hcount*

	t+3	*t+6*	*t+9*
Average TE	3.74%	5.27%	2.93%
s.e.	(0.0258)	(0.0247)	(0.0244)
t-test	0.15	0.03	0.23
No. of treated	290	287	280
No. of matchings	846	844	832

For the ten nearest neighbour matching algorithm[11], we find according to Panel A of Table 5 that the fraction of firms with a too large workforce given the demand prospects is significantly higher among contrained firms in all three quarters after the treatment. The effect is strongest in *t+6*. Controlling for the realized demand after the treatment leads to smaller and less significant treatment effects in Panel B of Table 5. In *t+6*, however, we still find a significant treatment effect of credit constraints. Six months after the treatment, constrained forms are significantly more likely to consider their workforce as too large compared to their unconstrained matching firms, even after matching on firm characteristics and controlling for post-treatment demand. This assessment is likely to be followed by lower employment growth. Furthermore, the finding for the qualitative measure *Hcount (too large)* supports the results found for the quantitative measure $\Delta Empl$ and suggests that the estimated quantitative effect is not a merely caused by extreme values in employment growth rates.

[11]The results are robust to other algorithms. The results shall not be reported here for the sake of conciseness, but are available upon request.

5.2 Treatment effect on short-time work

Instead of laying off employees, firms also can apply for short-time work to reduce their employees working hours (see Discussion in Section 3.3). Reduced working hours, however, would not be reflected in year-on-year employment growth. Therefore, we expand our analysis of the causal effect of restrictive bank lending to the variable *Short-time*, which indicates that a firm is currently working reduced hours.

In Panel A of Table 6, we show that twelve months after the treatment the probability of short-time work is about 7 percentage points higher for constrained firms than for unconstrained matching firms for the ten nearest neighbours matching with a caliper of 0.2σ. This finding is robust to changes in the matching algorithm, but not to the regression-adjustment presented in Panel B of Table 6, which is calculated analogue to Equation (3). The treatment effect decreases to 2 percentage points in the ten nearest neighbours matching and is no longer statistically significant. For the radius matching the treatment effect remains significant at the ten per cent level and for the Kernel-based matching at the five per cent level. The size of the effect, however, is small between 2.4 and 3 percentage points.

To get a more precise view on the impact of restrictive bank lending on *Short-time*, we measure the outcome three, six and nine months after the treatment. The effects based on the ten nearest neighbours matching are shown in Table 7. In *t+3* credit constrained firms are significantly more likely to apply for short-time work. The effect becomes smaller and less significant in later periods. Again, regression-adjustment eliminates most of the effects.

We conclude that firms use short-time work to adjust to short-run changes in the demand for their products, but only to a small extent as a response to credit constraints. We find the strongest treatment effect of credit constraints on *Short-time* three months after the treatment. The sensitivity of the estimated effect to the regression-adjustment underlines the importance of complementing the propensity score matching with a regression analysis controlling for post-treatment developments. Additionally, it makes an even stronger case for the robustness of the results for employment growth in Section 5.1.

Table 6

Panel A: Treatment effect (TE) for *Short-time* in *t+12*

	NN 10	NN 5	Radius (0.2σ)	Radius (0.25σ)	Epanechnikov
Average TE	7.24%	6.94%	7.66%	7.62%	8.15%
s.e.	(0.0238)	(0.0275)	(0.0167)	(0.0161)	(0.0151)
t-test	0.002	0.01	0.000	0.000	0.000
No. of treated	242	242	242	248	255
No. of matchings	1037	721	2340	2485	2846

Panel B: Regression-adjusted treatment effect (TE) for *Short-time* in *t+12*

	NN 10	NN 5	Radius (0.2σ)	Radius (0.25σ)	Epanechnikov
Average TE	2.02%	2.11%	2.42%	2.71%	2.98%
s.e.	(0.0211)	(0.0244)	(0.0149)	(0.0143)	(0.0135)
t-test	0.34	0.39	0.10	0.06	0.03
No. of treated	242	242	242	248	255
No. of matchings	1037	721	2340	2485	2846

6 Conclusion

Credit constraints restrain firms from taking advantage of growth opportunities or even from continuing their business. The appropriate policy response, however, depends on the cause of the constraints. If credit supply is restrained by weak bank rather than firm balance sheets, subsidies for the banking sector should be favored over those for firms to overcome a “credit crunch”. Our analysis focuses on a representative panel of German manufacturing firms. From 2003 to 2011 38% of these firms experineced a deterioration in bank lending conditiones and perceived bank lending to be restrictive.

We assess the effect of restrictive bank lending on the firms’ employment growth after ruling out the effect of firm heterogeneity. If restrictive bank lending reflected firm characteristics only, constrained firms would not differ in growth rates from firms that are unconstrained but otherwise similar in all relevant firm characteristics. Our results, however, reveal that even after ruling out firm heterogeneity, employment growth is significantly slower for constrained firms compared to unconstrained matching firms one year after the treatment. A complementary analysis based on a qualitative assessment of the

Table 7

Panel A: Treatment effect (TE) for *Short-time*

	t+3	*t+6*	*t+9*
Average TE	5.05%	3.73%	4.41%
s.e.	(0.0240)	(0.0263)	(0.0274)
t-test	0.04	0.16	0.11
No. of treated	281	279	263
No. of matchings	824	816	804

Panel B: Regression-adjusted treatment effect (TE) for *Short-time*

	t+3	*t+6*	*t+9*
Average TE	2.94%	0.56%	-1.01%
s.e.	(0.0234)	(0.0237)	(0.0241)
t-test	0.21	0.81	0.67
No. of treated	281	279	263
No. of matchings	824	816	804

firm's current workforce supports this finding. Constrained firms are significantly more likely to appraise their workforce as being too large six months after the deterioration in bank lending conditions.

We provide evidence for a real effect of restrictive bank lending at the firm-level. From this finding we deduce that in times of bank distress political measures for the banking sector might be necessary to circumvent the dampening effects of restrictive bank lending on real economic activity. In contrast, political measures focusing at the firm-level (such as public guarantees or other subsidies for private firms) are likely to be insufficient for the resolution of credit constraints. In this context, we only find weak evidence that firms utilize subsidized short-time work arrangements to adjust to restrictive bank lending by reducing the hours worked per employee.

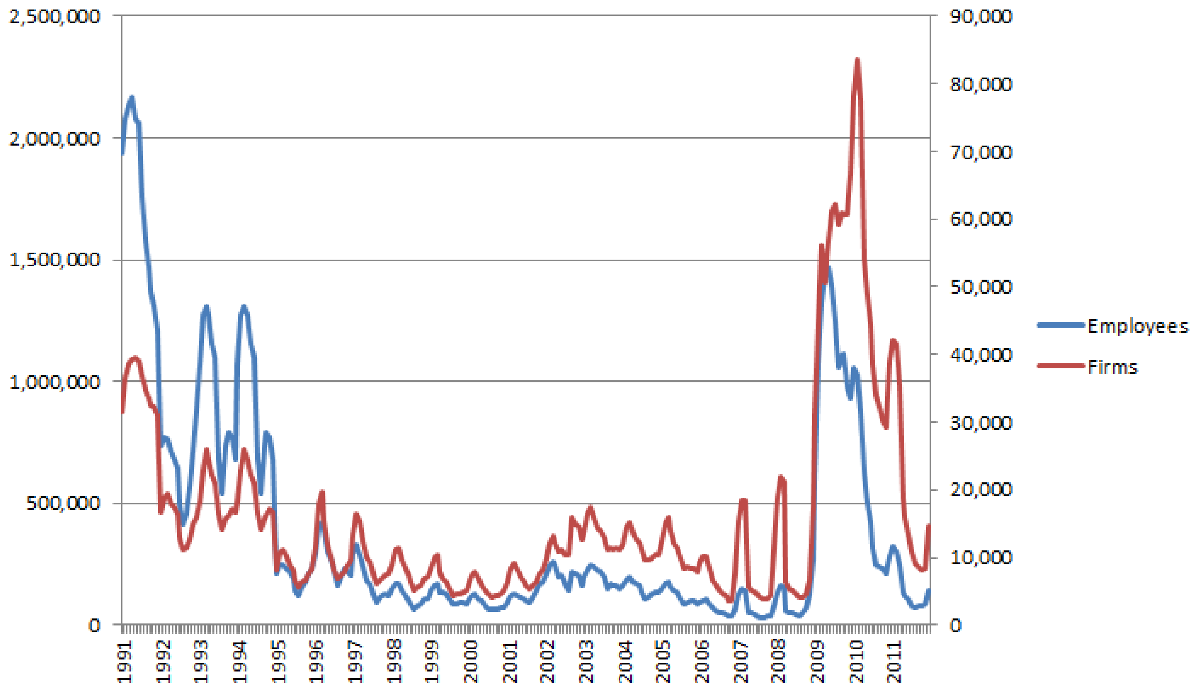
Appendix A

Short-time work in Germany

German labour market regulation contains three kinds of short-time work. Seasonal short-time work is available for firms in the construction sector where labour demand is affected by seasonal volatility. Since we only look at firms from the manufacturing sector, this kind of short-time work is irrelevant for our analysis. Transfer short-time work (§216b SGB III) is relevant for restructuring due to which a permanent decline in labour demand occurs. Transfer short-time work is supposed to avoid immediate lay-offs, thereby improving the likelihood of successful placement service to workers. Business cycle short-time work (§170 SGB III) comes into play when economic causes or unavoidable events cause a reduction of working hours. Originally, the period of short-time work was limited to six months. Firms can apply for short-time work repeatedly if three months have passed since the last short-time work payment and if the conditions for short-time work are again fulfilled. In the light of the financial crisis, the maximum length of a short-time work period was extended to 18 months in January 2009 and further increased to 24 months in July 2009. This increase was only temporary and the limit was brought back down to 18 months for short-time work applications in 2010 and 12 months for those in 2010. In addition to these regulatory adjustments, the minimum requirements for short-time work were eased in February 2009. The German Federal Employment Agency supports employees by paying 60 or 67 per cent (depending on whether the employee has a child) of the income that is lost because of reduced working hours.

To illustrate the importance of short-time work in Germany, Figure 1 shows the number of firms working short-time and the number of affected employees. In the early 1990s short-time work was used to dampen the repercussions of the German reunification in 1989 and the recession in 1993. Afterwards, short-time work remained at a lower level, showing seasonal fluctuations. There has been a slight increase in short-time work in the early 2000s and a sharp increase after the wake of the financial crisis in 2008. Short-time work is a highly relevant phenomenon during our sample period from 2003 to 2011. Looking

Figure 1: Development of short-time work in Germany



at employment growth only could therefore understate employment effects of restrictive bank lending because this does not take into account the number of employees working short-time.

Appendix B

Tables and Figures

Table 8: Correlation between *Hcount (too large)* and employment growth

	Hcount (too large)	p-value	*Sign of Hcount (too large)*	p-value
ΔEmpl				
t	-0.0032	0.200	-0.0892	0.000
t+3	-0.0124	0.000	-0.1093	0.000
t+6	-0.0205	0.000	-0.1282	0.000
t+9	-0.0289	0.000	-0.1434	0.000
t+12	-0.0350	0.000	-0.1469	0.000
t+15	-0.0341	0.000	-0.1353	0.000
t+18	-0.0301	0.000	-0.1154	0.000
t+21	-0.0239	0.000	-0.0917	0.000
t+24	-0.0203	0.000	-0.0689	0.000

Table 9: Descriptive statistics

	N	Mean	SD	Min	Max
Treatment variable (Survey)					
Constrained	29216	0.05	0.22	0	1
Outcome variables (Survey)					
Empl	208097	368.36	1611.27	1	51500
Δ*Empl*	172260	0.06	0.60	-1	17
Hcount (too large)	65423	0.21	0.41	0	1
Hcount (enough)	65423	0.72	0.45	0	1
Hcount (too small)	65423	0.07	0.26	0	1
Short-time	75035	0.09	0.28	0	1
Expected employment (Survey)					
Empl expect (+)	230915	0.07	0.26	0	1
Empl expect (=)	230915	0.76	0.43	0	1
Empl expect (-)	230915	0.17	0.37	0	1
Short-time expect	70766	0.13	0.33	0	1
Balance sheet information					
Equity / Assets	7106	0.32	0.28	-2.22	1.00
Gross profit / Assets	7115	0.79	1.37	-0.22	35.93
Cash flow / Assets	5248	0.09	0.10	-0.88	1.05
Fixed assets / Assets	7067	0.36	0.22	0.00	1.00
Business indicators (Survey)					
State (good)	230915	0.24	0.43	0	1
State (satisfactory)	230915	0.53	0.50	0	1
State (bad)	230915	0.23	0.42	0	1
Business expect (+)	230915	0.20	0.40	0	1
Business expect (=)	230915	0.61	0.49	0	1
Business expect (-)	230915	0.19	0.39	0	1
Demand for a firm's products (Survey)					
Demand (+)	230915	0.21	0.41	0	1
Demand (=)	230915	0.57	0.49	0	1
Demand (-)	230915	0.21	0.41	0	1
Orders (high)	230915	0.12	0.33	0	1
Orders (enough)	230915	0.52	0.50	0	1
Orders (too small)	230915	0.36	0.48	0	1

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