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

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

Michael Kleemann
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Abstract

This paper advocates for incorporating timely measures of firms' current situation and future expectations when disentangling real effects of credit supply from demand-side factors. Identification of supply-side effects in firm-level analyses often relies on balance sheet variables to control for firm heterogeneity. While balance sheets mirror past business, bias from contemporaneous and forward-looking firm-side factors may persist. Using German firm-level survey data from 2003 to 2011, we show that controlling for firms' current situation and future expectations reduces upward bias in estimated credit supply-side effects on firm-level production. This is particularly important when market data is unavailable for firms.

JEL Code: G21, E23, L25.

Keywords: Credit constraints; production; employment; matching estimator.

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1. Introduction

Designing policy measures in response to credit constraints requires an understanding of whether they are caused by credit supply-side factors (e.g., bank liquidity shocks) or firm-side factors (e.g., shocks to the firms' asset values). During the financial crisis of 2007-09, for example, banks in many countries faced severe liquidity shocks and reduced their lending. Lacking access to credit, firms postponed investment and reduced their business activity. Therefore, credit supply-side factors caused a slowdown in real economic activity.¹ In other countries, however, banks weathered the financial crisis quite well, yet non-financial firms nevertheless experienced credit constraints as a reflection of their deteriorating creditworthiness when the worldwide economic slowdown hit demand for their products.² According to this narrative, credit constraints and the economic slowdown were not caused only by credit supply-side, but also by firm-side factors.

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

We address the question of whether not controlling for firms' current situation and future expectations, in addition to balance sheet data, leads to biased estimations of the real effects of credit supply-side disruptions. Our analysis is based on data from the "EBDC Business Expectations Panel" for Germany between 2003 and 2011, which

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

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

combines firms' balance sheets with survey-based appraisals of their current situation and future expectations.³ The data set provides a timely treatment variable indicating the experience of constrained credit supply at the firm-level based on monthly panel data on firms' perceptions of bank credit supply. Applying ordinary least squares (OLS) and propensity score matching (PSM) approaches, we estimate the treatment effect of constrained credit supply on monthly changes in firm-level production.

We find that the sole reliance on firm balance sheet data to measure credit demand-side factors leads to an overestimation of the real effects of credit supply. More specifically, when controlling for balance sheet variables only, OLS and PSM estimations suggest that constrained credit supply significantly increases the probability that a firm lowers its production during the post-treatment year. However, controlling for survey-based appraisals of firms' current situation and future expectations significantly lowers the OLS estimators and PSM estimators even turn insignificant. Our findings are confirmed when using firm-level employment growth as an alternative outcome variable and when ruling out that the anticipation of constrained credit supply in the future is driving our results. We conclude that the empirical literature on real effects of credit supply needs to develop adequate approaches to rule out firm heterogeneity in current situation and future expectations, which is insufficiently captured by balance sheets. Furthermore, our findings raise doubts about the importance of real effects of credit supply disruptions in Germany during the financial crisis.

³German data is particularly suited to study the real effects of credit constraints. The German financial system is bank-based and characterised by a significant fraction of the firms having limited access to public market finance as a substitute for bank credit. Therefore, credit constraints are more likely to be binding and may have larger real effects. For a review of the vast literature comparing financial systems, see, for example, Allen and Gale (2000).

This analysis contributes to the literature on the real effects of credit supply.⁴ More precisely, we address the question of how to rule out credit demand-side factors to identify credit supply-side effects at the firm-level.⁵ In the following, we therefore provide a review of the control variables used in previous firm-level studies.

First, studies based on listed firms combine balance sheet and market data to rule out firm-side factors and identify real effects of credit supply (e.g., on firms' investments).⁶ In all of these studies different sets of balance sheet variables such as firm size, cash flow, cash holdings, leverage, and profitability are used to control for firm heterogeneity. Since balance sheets are backward-looking, these studies additionally control for at least one market-based variable (e.g., market-to-book value), assuming that market prices contain all relevant contemporaneous and forward-looking information about a firm.⁷

Second, Chodorow-Reich (2014) and Campello, Graham, and Harvey (2010) also include non-listed firms for which market data is unavailable. Instead, Chodorow-Reich (2014) controls for firms' borrowing patterns, size, age, access to public bond markets, and a set of variables from the Dealscan database when estimating the effect of bank health on U.S. firm-level employment.⁸ Campello, Graham, and Harvey (2010) show that

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

⁵Our findings may also be relevant for microeconomic analyses of credit supply-side effects based on macroeconomic data (Peek and Rosengren, 2000) or at the sector-level (see, for example, Kroszner, Laeven, and Klingebiel (2007), Dell'Ariccia, Detragiache, and Rajan (2008), and Duygan-Bump, Levkov, and Montoriol-Garriga (2011), who follow Rajan and Zingales (1998) by using a sector's external finance dependence as a measure of financial constraints.)

⁶Gan (2007), Duchin, Ozbas, and Sensoy (2010), Almeida, Campello, Laranjeira, and Weisbenner (2011), Chava and Purnanandam (2011), and Lin and Paravisini (2012) use financial crises to identify credit supply-side shocks, whereas Amiti and Weinstein (2013) measure bank shocks directly.

⁷In a related but distinct strand of the literature market data is also used to estimate the impact of bank health on firms' market values, for example by Yamori and Murakami (1999), Kang and Stulz (2000), Bae, Kang, and Lim (2002), and Ongena, Smith, and Michalsen (2003).

⁸In a robustness test, Chodorow-Reich (2014) also applies a within-firm approach as applied by Khwaja and Mian (2008). This approach, however, does not capture firms' current situation and future expectations as these are potentially time-variant.

financial constraints adversely affect several firm-level outcome variables (e.g., planned employment cuts). In a first step, they control for firm size, ownership, industry, and rating category to rule out firm heterogeneity. However, when they account for contemporaneous and forward-looking firm-side factors by including control variables for profitability, growth prospects, and dividend payer status, their estimated real effects of financial constraints turn out slightly smaller.

Our contribution to this literature is as follows:⁹ First, we employ survey-based measures of firms' current situation and future expectations to disentangle real effects of credit supply from credit demand-side factors. This enables us to test whether the sole reliance on balance sheet data leads to an overestimation of these effects. Our results are also of interest for the estimation of credit supply-side effects on loan outcomes without estimating real effects.¹⁰ Second, we use monthly panel data to identify a direct and timely treatment variable for constrained credit supply and estimate credit supply-side effects on monthly changes in firm-level production and employment. Thereby, our approach is similar to the one that Campello, Graham, and Harvey (2010) apply to cross-sectional data. Finally, we estimate credit supply-side effects on annual employment growth, which is regularly prohibited by a lack of firm-level employment data.¹¹

⁹Note that our conclusions apply to microeconomic analyses of the bank lending channel, which are distinct from common macroeconomic approaches to disentangle credit supply and demand shocks based on structural vector autoregressive models; such as Hristov, Hülsewig, and Wollmershäuser (2012) for the Euro Area and De Nicolò and Lucchetta (2011) for the G-7 countries.

¹⁰For example, in their analysis of bank-side effects on loan outcomes using Spanish data, Jiménez, Ongena, Peydro, and Saurina (2012) control for firm size, leverage, liquidity, firm age, and credit history. Analysing bank-side effects on credit rejections in Eastern Europe, Popov and Udell (2012) control for firm size, age, ownership, export status, and external auditing. Santos (2011) also controls for market-based firm characteristics when analysing loan pricing during the recent subprime crisis. Explicit measures of firms' current situation and future expectations could amend this line of research.

¹¹A notable exception is the study by Chodorow-Reich (2014), who uses firm-level employment data. In addition, Campello, Graham, and Harvey (2010) estimate the effects of financial constraints on planned employment cuts but without observing whether these plans are implemented. Duygan-Bump, Levkov, and Montoriol-Garriga (2011) estimate employment effects of financing constraints at the sector-level by using workers' employment status from the U.S. Current Population Survey (CPS).

The paper is structured as follows. Section 2 derives a testable hypothesis. Section 3 describes the data set, the treatment definition, and the control variables. Section 4.1 explains the empirical strategy. Results are presented in Sections 4.2 and 4.3. Section 5 provides robustness checks. Section 6 discusses the results and Section 7 concludes.

2. Hypothesis

If constrained credit supply was randomly assigned to firms, observed differences between constrained and unconstrained firms can be interpreted as caused by credit supply-side factors. Theoretical literature provides two potential explanations for such a random assignment: First, according to Stiglitz and Weiss (1981), credit rationing induces the possibility that a firm is granted bank credit while the credit application of an identical other firm is rejected. Credit constraints may then be considered randomly assigned. Second, the customer relationship model of banking by Sharpe (1990) explains differences in credit supply between otherwise similar firms by their different bank relationships. Empirical studies support this by showing that banks' health affects corporate clients,¹² that banks with different characteristics are unequally inclined to transmit monetary policy to the real sector,¹³ and that their lending behaviour differs during financial crises.¹⁴

The empirical identification of the real effects of credit supply, however, is complicated as banks' credit granting decisions are not random, but depend on firms' creditworthiness and expected future performance. If a firm is hit by a negative product demand shock, it is more likely to experience constrained credit supply. At the same time it is more likely to reduce its production, but does so in response to the product demand shock rather than credit supply-side disruptions. Consequently, observed differences between constrained

¹²Almeida, Campello, Laranjeira, and Weisbenner (2011), Santos (2011), and Chodorow-Reich (2014).

¹³Kashyap and Stein (2000), Kishan and Opiela (2000), Gambacorta (2005), Kishan and Opiela (2006), and Jimenez, Ongena, Peydro, and Saurina (2012).

¹⁴Albertazzi and Marchetti (2010), Ivashina and Scharfstein (2010), and Puri, Rocholl, and Steffen (2011).

and unconstrained firms would overstate the real effects of credit supply. Alternatively, observed differences could understate credit supply-side effects if firms with favourable growth potentials share certain features that make them prone to credit constraints. This could be the case for businesses that are young, small, and risky.

Therefore, controlling for firm heterogeneity is crucial. Balance sheets provide accurate measures of heterogeneity in firms' financial conditions, but they mirror only past business. Firms' current situation and future expectations, however, predict real economic activity and are considered in banks' credit granting decision (e.g., by looking at order books, interim financial statements, or business plans). As they are not captured by balance sheets, they are likely to induce estimation bias.

Hypothesis: *Not controlling for firms' current situation and future expectations leads to biased estimations of real effects of credit supply-side factors.*

3. Data

3.1. The Data Set

For our analysis, we rely on the German "EBDC Business Expectations Panel". This data set links firms' balance sheets from the Bureau van Dyk (BvD) Amadeus database¹⁵ and the Hoppenstedt database¹⁶ to panel data from the Ifo Business Survey.¹⁷ The latter is a monthly survey asking 3,600 plants from the German manufacturing sector for appraisals of their current situation and expectations for their future business. All variables used in this analysis are described in Table 1.

¹⁵The BvD Amadeus database contains balance sheet data for about 1 million mainly non-listed German firms. Its primary source for Germany is Creditreform, a rating agency.

¹⁶Hoppenstedt has almost full coverage of publicly available financial statements for German firms. The public press and commercial registries are its main data sources.

¹⁷When linking annual balance sheet and monthly survey data, which is done based on the name and postal address of the firms, the following issue arises. The fiscal years of some firms in the sample do not coincide with calendar years. We use the monthly frequency of the data and put the most recently published balance sheet into every monthly observation. Thereby, we assume that a firm's balance sheet is made available in the credit application process immediately at the end of the fiscal year.

Table 1: Variable descriptions

| Variable | Description | Type | Frequency |
|----------------------------|---|------------|-----------|
| Treatment | | | |
| <i>Constrained</i> | Change in perception of bank lending from accommodating or normal to restrictive | Dummy | Monthly |
| Outcomes | | | |
| <i>Slowdown</i> | Production decreased at least once in the last 12 months | Binary | Monthly |
| <i>Slowdown_avg</i> | Fraction of last 12 months in which production decreased | Continuous | Monthly |
| $\Delta Empl$ | Year-on-year employment growth rate | Continuous | Annual |
| <i>Empl_decrease</i> | Negative $\Delta Empl$ | Binary | Annual |
| Firm size | | | |
| <i>Empl</i> | Number of employees at company-level | Continuous | Annual |
| <i>Assets</i> | Total assets | Continuous | Annual |
| Balance sheet data | | | |
| <i>Equity</i> | Equity / Assets | Continuous | Annual |
| <i>Cash</i> | Cash holdings / Assets | Continuous | Annual |
| <i>Long-term_debt</i> | Long-term debt / Assets | Continuous | Annual |
| <i>Short-term_debt</i> | Short-term debt / Assets | Continuous | Annual |
| <i>Cash_flow</i> | Cash flow / Assets | Continuous | Annual |
| <i>ROA</i> | Operating profit / Assets | Continuous | Annual |
| <i>Interest_coverage</i> | Operating profit / Interest expenses | Continuous | Annual |
| Current situation | | | |
| <i>State</i> | Appraisal of current business situation (good / satisfactory / bad) | Ordinal | Monthly |
| <i>Orders</i> | Appraisal of stock of orders (high / enough / too small) | Ordinal | Monthly |
| <i>Production</i> | Production compared to previous month (increased / unchanged / decreased) | Ordinal | Monthly |
| <i>Short-time</i> | Firm is currently working short-time | Binary | Quarterly |
| <i>Export</i> | Firm is exporting | Binary | Quarterly |
| Future expectations | | | |
| <i>State_exp</i> | Business expectations, 6-months horizon (improvement / no change / worsening) | Ordinal | Monthly |
| <i>Empl_exp</i> | Employment expectations, 3-months horizon (increase / no change / decrease) | Ordinal | Monthly |
| <i>Headcount</i> | Appraisal of employment given demand, 12-months horizon (too few / enough / too many) | Ordinal | Quarterly |
| <i>Short-time_exp</i> | Expectation to work short-time, 3-months horizon | Binary | Quarterly |

The Ifo Business Survey provides the basis for the Ifo Business Climate Index, a widely recognised indicator of economic activity in Germany. For its calculation, the Ifo Institute continuously ensures representativeness of the German manufacturing sector.¹⁸

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

3.2. *The Treatment of Constrained Credit Supply*

From the panel data, we derive a variable indicating that a firm receives the treatment of constrained credit supply in a particular month. Since 2003, firms in the Ifo Business Survey are asked how they perceive “banks’ willingness to supply credit”. Possible answers are “restrictive”, “normal”, and “accommodating”. This enables us to directly measure a perceived change in credit supply to a firm. We define a firm as *constrained* or *treated* in month t ($Constrained_{i,t} = 1$) if it reports “restrictive” bank lending in month t after reporting “normal” or “accommodating” bank lending in the previous twelve months. In contrast, a firm is defined as *unconstrained* or *untreated* in t ($Constrained_{i,t} = 0$) if it reports “normal” or “accommodating” bank lending in twelve subsequent months and does not switch to reporting “restrictive” bank lending in month t . This allows a treatment effect estimation without bias from possible previous

¹⁸A more detailed description of the data set is provided by Becker and Wohlrabe (2008). Kipar (2011) uses this data set to test the impact of credit constraints on firm-level innovation activity.

treatments, but comes at the cost of using only a fraction of the original data set.

Estimating the treatment effect on a monthly basis requires an assumption on the exact treatment month for the observations prior to November 2008, when the frequency of the survey question on credit supply was changed from bi-annual (March and August) to monthly. If a firm switches to reporting “restrictive” bank lending between March and August or vice versa before November 2008, we assume that the treatment occurs in the first month after a firm has reported “normal” or “accommodating” the last time. This assumption is further illustrated and discussed in Appendix A.

The final sample consists of 333 treated and 5,061 untreated firm-month observations after conditioning on the availability of all control variables in $t-1$, the pre-treatment month. Figure 1 in Appendix C shows the distribution of treated firms over time. As expected, their number increases sharply in the wake of the financial crisis.

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

3.3. Measuring Firm-Level Real Economic Activity

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

The main dependent variable in the following analysis is $Slowdown_{i,t+12}$, which indicates that a firm reports a decrease in production at least once during the twelve post-treatment months. Our analysis focuses on this negative outcome because we are primarily concerned about restraining effects of credit supply. As a robustness check, we run estimations using the outcome variable $Slowdown_avg_{i,t+12}$, which measures the fraction of the twelve post-treatment months in which a firm reports a decreasing production. Furthermore, we estimate the treatment effect on a firm’s annual employment growth rate $\Delta Empl_{i,t+12}$, which is calculated as the symmetric growth rate

$$\Delta Empl_{i,t+12} = \frac{Empl_{i,t+12} - Empl_{i,t}}{0.5(Empl_{i,t+12} + Empl_{i,t})} \quad (1)$$

of Davis, Haltiwanger, and Schuh (1996), which is used by Chodorow-Reich (2014).¹⁹

A caveat in the employment data is that firms report their numbers of employees in the Ifo Business Survey every year in November. As the treatment variable is defined on a monthly basis, this impedes the distinction between post-treatment and pre-treatment employment changes. Appendix B contains a discussion of this issue and presents our approach to deal with it. Nevertheless, the exact level of the estimated employment effects should be interpreted with caution and $Empl_decrease_{i,t+12}$, indicating a negative post-treatment employment growth rate, is used as an alternative dependent variable.

According to Table 2, constrained firms show significantly lower post-treatment real economic activity than unconstrained firms in all four outcome variables. They are significantly more likely to report a decreasing production at least once during the post-treatment year. On average, they face decreasing production in almost a quarter of the

¹⁹This growth measure is a second order approximation of the log difference around zero (Haltiwanger, Jarmin, and Miranda, 2013). It shares the log difference’s useful property of symmetry and has become a standard in the analysis of firm dynamics. By being bounded between -2 and 2, it also reduces the impact of extreme values. However, due to survey response behaviour, the data still contains implausible large values. Therefore, $\Delta Empl_{i,t+12}$ is cut by one percent from both sides of the distribution.

twelve post-treatment months while this figure is 10 percentage points lower for unconstrained firms. Employment growth rates are negative for constrained firms, but positive for unconstrained ones. Finally, the probability of a decreasing employment is 15 percentage points higher for constrained firms.

Table 2: Firms' post-treatment business activity

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

Notes: The table shows the descriptive statistics of post-treatment outcome variables separately for treated and untreated firms; treatment status $Constrained_{i,t}$ is defined as described in Section 3.2; p-values are reported for a two-group mean comparison t-test with $H_0: \bar{X}_C = \bar{X}_U$; $\Delta Empl$ is cut by one percent from both sides of the distribution to deal with extreme values; these observations are kept in *Empl_decrease*; the samples contain only observations for which all pre-treatment control variables listed in Table 3 and Table 4 are available; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.4. Heterogeneity in Firms' Balance Sheets

Table 3 illustrates how constrained and unconstrained firms differ in pre-treatment size and balance sheet variables $B_{i,t-1}$. Firm size is widely used as a predictor of financial constraints because large firms tend to be older and more transparent, which may facilitate access to credit. In Table 3, however, constrained and unconstrained firms do not differ significantly in $\log(Empl)$ or $\log(Assets)$.

The other variables in Table 3 are in line with existing literature showing that constrained firms are in a worse financial condition than unconstrained ones. They have significantly lower equity ratios, fewer cash holdings, more long-term debt, lower cash

flows, and lower interest coverage ratios.²⁰ They also have more short-term debt and a lower operating profitability (*ROA*), but these differences are statistically insignificant.

Table 3: Firms' pre-treatment size and balance sheets

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

Notes: The table shows descriptive statistics of pre-treatment variables $B_{i,t-1}$ separately for treated and untreated firms; treatment status $Constrained_{i,t}$ is defined as described in Section 3.2; p-values are reported for a two-group mean comparison t-test with $H_0: \bar{X}_C = \bar{X}_U$; *Interest_coverage* is set to zero if values are negative and is winsorized at the 95 percentile; the samples contain only observations for which all pre-treatment control variables listed here and in Table 4 are available; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.5. Heterogeneity in Firms' Current Situation and Future Expectations

Beyond information from balance sheets, Table 4 shows that constrained and unconstrained firms differ in their pre-treatment current situation $C_{i,t-1}$ and future expectations $F_{i,t-1}$. First, firms are asked in the Ifo Business Survey how they appraise their current business situation. Constrained firms are significantly less likely to report a “good” situation (*State (+)*), but more likely to report a “bad” one (*State (-)*).²¹ Second, constrained

²⁰*Interest_coverage* is set to zero if it takes on negative values and winsorised at the 95 percentile.

²¹Throughout this paper, only good and bad appraisals or assessments are reported for variables from the Ifo Business Survey. Neutral categories constitute the baseline categories in all estimations.

firms are more likely to report their current stock of orders being “too small” (*Orders* (-)). Third, they are more likely to report a decreasing production in $t-1$ (*Production* (-)), which suggests a pre-treatment trend in the outcome variable. Finally, variables indicating that a firm is currently using short-time work and export status do not show any significant differences.²²

Table 4: Firms’ pre-treatment current situation and future expectations

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

Notes: The table shows descriptive statistics of pre-treatment variables $C_{i,t-1}$ and $F_{i,t-1}$ separately for treated and untreated firms; treatment status $Constrained_{i,t}$ is defined as described in Section 3.2; p-values are reported for a two-group mean comparison t-test with $H_0: \bar{X}_C = \bar{X}_U$; the samples contain only observations for which all pre-treatment control variables listed here and in Table 3 are available; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

²²Short-time work is a labour market instrument widely used by German firms to adjust their capacities to business cycle or seasonal demand fluctuations. It was particularly widespread during the financial crisis. Export status is important because foreign demand in Germany was severely affected by the financial crisis, which could affect both a firm’s access to credit and its business.

Forward-looking firm characteristics in the data show further heterogeneity between constrained and unconstrained firms that might not be captured by backward-looking balance sheets. As shown in Table 4, constrained firms are significantly less likely to expect business to “improve” over the next six months (*State_exp (+)*), but more likely to expect business to “worsen” (*State_exp (-)*). This pattern is confirmed for the expectation of decreasing employment over the next three months (*Empl_exp (-)*) and the variable indicating that a firm considers its workforce too large for the product demand over the next twelve months (*Headcount (-)*). There is no such difference in the expectation of short-time work (*Short-time_exp*).

In sum, the data shows how variables for firms’ current situation and future expectations predict constrained credit supply. As they also predict a firm’s production and employment decisions, heterogeneity in these variables must be controlled for to ensure unbiased estimations of the real effects of credit supply. Survey data could provide a key contribution in this concern.

4. Methodology and Results

4.1. Methodology

For every firm i in the panel data set, we observe whether it receives a treatment of constrained credit supply in month t ($Constrained_{i,t} = 1$) and estimate the effect of this treatment on the likelihood of a decrease in production during the subsequent twelve months ($Slowdown_{i,t+12} = 1$).²³

²³We illustrate the empirical strategy based on the notation of an OLS estimation. Although the dependent variable in our baseline estimation is binary, we apply a linear probability model instead of non-linear estimation approaches in order to test the equality of coefficients as suggested by Clogg, Petkova, and Haritou (1995). In Section 4.3, we also test the hypothesis using PSM estimation.

In a first step, we estimate

$$E[Slowdown_{i,t+12}|B_{i,t-1}] = \beta_0^B + \beta_1^B Constrained_{i,t} + \beta_2^B B_{i,t-1} + \epsilon_i \quad (2)$$

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

$$E[Slowdown_{i,t+12}|B_{i,t-1}, C_{i,t-1}, F_{i,t-1}] = \beta_0^{BCF} + \beta_1^{BCF} Constrained_{i,t} + \beta_2^{BCF} B_{i,t-1} \quad (3)$$

$$+ \beta_3^{BCF} C_{i,t-1} + \beta_4^{BCF} F_{i,t-1} + \zeta_i$$

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

$$Diff^{CF} = \beta_1^B - \beta_1^{BCF} \quad (4)$$

which measures the degree to which the estimated real effects of credit supply are biased when controlling for balance sheet variables only.

4.2. Results: Ordinary Least Squares Estimations

Pooled OLS estimation provides a first test of our hypothesis. In line with Equation (2) in Section 4.1, the dummy variable $Slowdown_{i,t+12}$ is regressed on the treatment status $Constrained_{i,t}$, a set of balance sheet variables $B_{i,t-1}$, and months and industry dummy variables based on the two-digit WZ 2008 industry classification. The results in Estimation (1) in Table 5 suggest that constrained credit supply increases the probability of decreasing production between t and $t+12$ by 8.65 percentage points.

When additionally controlling for firms' current situation $C_{i,t-1}$ and future expectations $F_{i,t-1}$ in Estimation (2), this effect is reduced to 7.01 percentage points. The third

column shows that the difference between the two estimated coefficients is highly statistically significant.²⁴ The adjusted R^2 statistics further show that the inclusion of the control variables $C_{i,t-1}$ and $F_{i,t-1}$ substantially increases the explanatory power.

Table 5: OLS estimations using $Slowdown_{i,t+12}$

| | (1) | (2) | $\beta_1^B - \beta_1^{BCF}$ |
|---------------------|---------------------|---------------------|-----------------------------|
| <i>Constrained</i> | 8.65%*** (0.027) | 7.01%*** (0.026) | 1.64%** |
| Firm size | Yes | Yes | |
| Balance sheet data | Yes | Yes | |
| Current situation | No | Yes | |
| Future expectations | No | Yes | |
| Time | Yes | Yes | |
| Industry | Yes | Yes | |
| p>t | 0.002 | 0.008 | |
| Upper bound | 13.14% | 11.36% | |
| Lower bound | 4.16% | 2.67% | |
| Adj. R^2 | 0.167 | 0.220 | |
| Treated obs. | 316 | 316 | |
| Untreated obs. | 4,877 | 4,877 | |

Notes: The table provides results for OLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Constrained_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; “Firm size”, “Balance sheet data”, “Current situation”, and “Future expectations” are sets of control variables as listed in Table 1; time effects are controlled for by including months dummy variables; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples contain only those observations for which all control variables of Estimation (2) are available; the third column provides the difference between the two estimated coefficients; its significance is tested using a t-test with $H_0: \beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); upper and lower bounds are reported for the 95 percent confidence interval of each estimator; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Therefore, OLS estimation supports the hypothesis that the sole reliance on balance sheet data leads to a significant overestimation of the impact of credit supply-side factors

²⁴Reported test statistics are derived following the procedure suggested by Clogg, Petkova, and Haritou (1995), which accounts for the fact that the coefficients are based on different estimations.

on real economic activity at the firm-level. The inclusion of measures for firms' current situation and future expectations reduces the estimated effect by almost 20 percent.

4.3. Results: Propensity Score Matching Estimations

OLS estimation has two potential disadvantages in the context of our analysis. First, it risks misspecifying the functional form of $E[Slowdown_{i,t+12}]$. Second, it may lead to a comparison of observations outside the common support if distributions of control variables vary between treated and untreated firms. This is most likely to be a problem for balance sheet variables, which show large standard errors that differ between constrained and unconstrained firms in Table 3. To address these issues, real effects of credit supply can be estimated using a matching estimator, as done by Campello, Graham, and Harvey (2010) and Almeida, Campello, Laranjeira, and Weisbenner (2011).

We apply PSM estimation following Rosenbaum and Rubin (1983) because the large number of covariates, including continuous balance sheet variables in $B_{i,t-1}$, inhibits the identification of identical matching firms. Comparing constrained and unconstrained firms with similar propensity scores provides an estimated treatment effect close to the one derived from an experimental setting (Dehejia and Wahba, 1999) in which constrained credit supply is randomly assigned. The identifying assumption still hinges on the set of matching variables, which is particularly comprehensive in our data set.

Matching only on balance sheet variables $B_{i,t-1}$, we estimate a logistic regression model

$$Pr(Constrained_{i,t} = 1 | B_{i,t-1}) = \Phi(\alpha_0 + \alpha_1 B_{i,t-1} + \alpha_2 Industry_i > u_i) \quad (5)$$

where $\Phi(\cdot)$ denotes the cumulative distribution function of the logistic distribution and

$Industry_i$ is a set of industry dummy variables.²⁵ To allow for time-heterogeneous parameters, estimations are run separately within every quarter.²⁶

Table 6: PSM estimation using $Slowdown_{i,t+12}$

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

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

From the estimated models, the propensity score (i.e. the conditional probability of being treated) is predicted for every firm-month observation. Every constrained firm is then linked to unconstrained firms from the same quarter-industry cell that have a

²⁵To avoid the impact of extreme values on the estimation, the delta deviance influence statistic is extracted for every observation. Following Agresti and Finlay (2008), an observation is dropped if $\sqrt{|ddeviance|} > 3$ and the maximum likelihood estimation is re-run until no observations are dropped.

²⁶Estimations within months or quarter-industry cells are not possible due to a lack of observations.

similar propensity score based on a ten nearest neighbours matching. Exact matching on quarter-industry cells rules out macroeconomic and industry-specific effects, and deals with time- and industry-dependence of the perception-based treatment variable. Finally, the treatment effect is estimated using a Weighted Least Squares (WLS) estimation of the outcome variable $Slowdown_{i,t+12}$ on the treatment status $Constrained_{i,t}$ with weights being drawn from the PSM procedure. In a second step, matching variables for firms' current situations $C_{i,t-1}$ and future expectations $F_{i,t-1}$ are added to the estimation.

When matching only on balance sheet variables, the results of Estimation (1) in Table 6 suggest that constrained credit supply increases the probability of a decreasing production in the post-treatment year significantly by 5.10 percentage points. Estimation (2), however, shows that this treatment effect is only 2.59 percentage points and statistically insignificant when also matching on firms' current situation and future expectations.²⁷

Therefore, Estimation (2) shows no significant real effects of credit supply disruptions in Germany between 2003 and 2011 as suggested by Estimation (1). The sole reliance on balance sheet data leads to an overestimation of such effects by almost 50 percent,²⁸.

5. Robustness Checks

5.1. Potential Caveats of the Survey Data

The survey questions raise two problems in the context of our analysis. First, respondents could be in a bad mood because of their firm's general situation and future expectations, and react by reporting restrictive bank lending (e.g., to blame banks for their situation). Therefore, the lower estimated effect of constrained credit supply could

²⁷Unfortunately, the statistical significance of the difference between the two estimators cannot be tested. In contrast to the previously applied unweighted OLS estimations, a test of equality of coefficients is prohibited when observations are assigned different weights in different PSM estimations.

²⁸Table C.1 in Appendix C shows that matching on balance sheet variables only does not eliminate bias in the survey-based variables entirely. Matching on all variables for firms' current situation and future expectations improves the balancing properties.

not be attributed to survey-based variables providing information that is not captured by balance sheets. Empirical evidence by Abberger, Birnbrich, and Seiler (2009) provides strong evidence against this critique. They use data from a meta-study among respondents of the Ifo Business Survey to show that general mood and economy-wide factors are not important determinants of firms' general appraisals in the survey.

Second, forward-looking appraisals could contain expectations of constrained credit supply and its consequences on the firm's future activity. We test the robustness of our results with respect to this concern by controlling only for variables that explicitly refer to factual aspects of business. These are assessments of the current stock of orders, as well as short-time work and export status. We also keep the variables *Headcount (+)* and *Headcount (-)* as they refer to employment figures relative to future demand and should therefore be unaffected by firms' expectations of credit supply. In contrast, the general appraisals *State*, *Production*, *State_exp*, *Short-time_exp*, and *Empl_exp* are excluded because they could be affected by expectations of constrained credit supply.

Table C.2 in Appendix C shows that controlling for such a reduced set of variables in OLS estimations leads to a significant reduction of the estimated credit supply-side effects by 1.5 percentage points. The difference is only slightly smaller than in our baseline estimation in Section 4.2, and is still highly statistically significant.²⁹ The adjusted R^2 statistic increases substantially suggesting a better model fit in Estimation (2).

This is confirmed by PSM estimations with a reduced set of matching variables. Matching on balance sheet variables only leads to an estimated treatment effect of 5.74 percentage points in Estimation (1) in Table C.2 in Appendix C. Controlling for the factual assessments of firms' current situation and future expectations lowers the estimated

²⁹The effect when controlling only for balance sheets variables differs from the one shown in Table 5. Although control variables do not change compared to the estimation in Table 5, conditioning on the availability of the smaller set of control variables in Estimation (2) affects the sample composition, and therefore alters the estimated effect.

effect to 3.16 percentage points in Estimation (2) and the estimator turns insignificant.

Therefore, even if the power of general assessments of contemporaneous and forward-looking firm-side factors is not believed, the omission of more factual variables still induces an overestimation of real effects of credit supply. For both the OLS and PSM estimation the suggested overestimation is only slightly smaller than in estimations based on all survey-based variables in Section 4.2 and 4.3.

5.2. An Alternative Measure for Firm-Level Production

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

The results for OLS estimations in Table C.3 in Appendix C show that the omission of control variables for firms' current situation and future expectations lowers the estimated treatment effect of $Constrained_{i,t}$ on $Slowdown_avg_{i,t+12}$ by about 12 percent. The difference is statistically significant and the adjusted R^2 statistic indicates a much better model fit when including contemporaneous and forward-looking control variables.

When applying PSM estimation with $Slowdown_avg_{i,t+12}$ as the dependent variable, this is confirmed. Matching on balance sheet variables only, Estimation (1) in Table C.3 in Appendix C shows a highly significant credit supply-side effect on firm-level production of 3.78 percentage points. Adding matching variables for firms' current situation and future expectations in Estimation (2) substantially lowers the effect by one percentage point. In contrast to estimations using $Slowdown_{i,t+12}$, however, the effect remains significant at the ten percent level. Even when using an alternative outcome variable for firm-level production, these estimation results underline the importance of controlling for firms' current situation and future expectations in the estimation of real effects of credit supply.

5.3. *Employment Effects*

The previous estimated effects of constrained credit supply on firm-level production raise the question of whether the sole reliance on balance sheet data also affects the estimation of firm-level employment effects. Table C.4 in Appendix C shows that controlling for firms' current situation and future expectations lowers the estimated effects of constrained credit supply on a firm's annual employment growth rate. The difference is statistically significant in the OLS estimation. For PSM estimation, the effect turns insignificant and the two effects lie outside each other's 95 percent confidence intervals. As mentioned in Section 3.3, the exact level of the estimated employment effects should be interpreted with caution due to the problems in linking annual employment data to the monthly treatment variable (see Appendix B for a discussion).

A dummy variable indicating a negative annual growth rate in the post-treatment year ($Empl_decrease_{i,t+12}$) is less likely to be affected by issues arising from linking annual employment figures to monthly data. Therefore, Table C.5 in Appendix C provides OLS estimations using this variable. The inclusion of control variables for firms' current situation and future expectations lowers the estimated effect significantly and the adjusted R^2 statistic increases substantially. PSM estimations in Table C.5 in Appendix C further show that the estimated treatment effect of constrained credit supply turns insignificant when including all control variables. The two estimators lie outside each others' 95 percent confidence intervals. In sum, these results show that heterogeneity in firms' current situation and future expectations that is not captured by balance sheet data also affects the estimation of employment effects of constrained credit supply.

5.4. *Sample Selection in PSM Estimations*

The estimated treatment effects from previous PSM estimations could differ because alternative sets of matching variables may induce the two estimation samples to comprise different firms. Three factors drive the sample differences when the number of matching

variables is increased. First, the inclusion of additional binary variables in $C_{i,t-1}$ and $F_{i,t-1}$ may induce perfect predictability so that some variables and the respective treated observations are dropped. Second, an increasing number of matching variables also raises the likelihood that observations are dropped due to a violation of the common support condition. Finally, when matching on additional variables, more degrees of freedom are required within each quarter-industry cell and the logistic regression yielding the propensity score may no longer converge.

To rule out that sample selection is driving the difference between estimators, we compare the sub-samples utilized by the matching procedures on the two alternative sets of pre-treatment covariates. We focus on the comparison of the treated firms since their characteristics also determine the selection of the matching firms. Table C.6 reports a comparison of sample means. The results do not indicate any substantial difference in pre-treatment firm characteristics. More specifically, out of 24 mean comparison tests only the one for the short-time work status indicates a difference that is statistically significant at the ten percent level. However, the expectations to work short-time within the next three months are balanced. This sample comparison suggests that differences in the estimated supply-side effects of bank lending constrained are rather driven by a reduction in selection bias than by sample selection.

5.5. The Role of the Financial Crisis of 2007-09

The sample period from 2003 to 2011 covers the financial crisis during which firms were operating under extreme conditions. Therefore, firm balance sheet data from previous years turned less informative for banks. Under such circumstances, banks may base their lending decisions even more on contemporaneous and forward-looking firm characteristics, which affects the necessary control variables to identify real effects of credit supply-side factors.

As Table C.7 in Appendix C shows, controlling for firms' current situation and fu-

ture expectations lowers the estimated treatment effect of constrained credit supply on firm-level production in the financial crisis subsample as of July 2007. Therefore, the subsample analysis underlines the importance of contemporaneous and forward-looking information on firms in times of financial crisis when balance sheet data from previous years might turn less informative. For the pre-crisis sample, however, we refrain from an econometric analysis because only a few firms are treated in this period and the sample turns out too small for our estimation procedure.

6. Discussion: No Real Effects of Credit Supply?

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

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

This picture may be different for other time periods or other countries. However,

the finding that a lack of controlling for contemporaneous and forward-looking firm-side factors leads to overestimated credit supply-side effects is generally applicable.

7. Conclusion

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

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

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

side factors is further shown for estimations of the effects of constrained credit supply on firm-level employment growth.

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

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Appendix

Appendix A: The Timing of the Treatment

As of 2008, the bank lending supply question in the Ifo Business Survey is asked on a monthly basis. This allows the exact specification of the treatment month t as the month in which the firm first reports restrictive bank lending after reporting normal or accommodating bank lending in previous surveys. From 2003 to 2008, however, the question was asked only twice a year, in March and August. If, for example, a firm reports normal or accommodating bank lending in March 2004 and restrictive bank lending in August 2004, it is unclear whether the shift has occurred in August or in a month between March and August.

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

Alternatively, we could assume that the treatment occurs in the month in which the firm reports “restrictive” bank lending the first time. However, our results are found to be insensitive to a variation of this timing assumption.

Appendix B: Discussion of Annual Frequency in Employment Data

In the Ifo Business Survey firms report their employment figures in November of each year. Two extreme alternatives are available to link annual employment growth rates

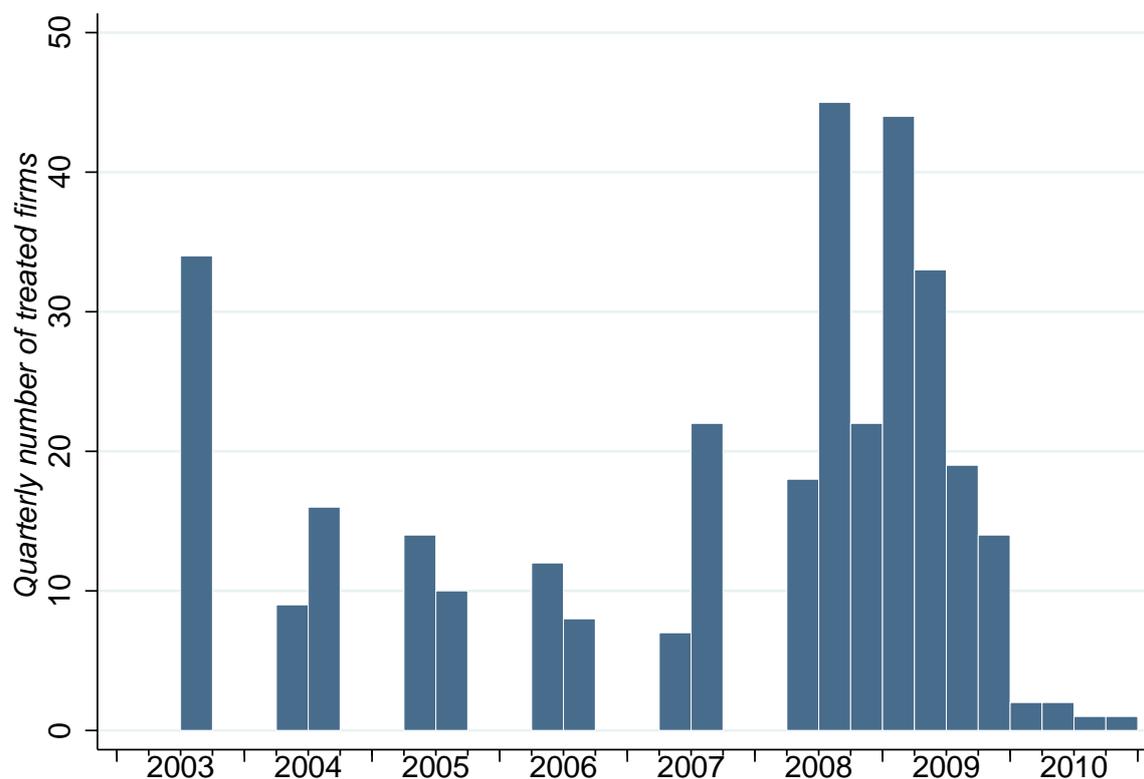
to monthly data, each leading to severe problems in the estimation of credit supply-side effects on firm-level employment growth.

According to the first alternative, we may assume the number of employees reported in November to stay constant for the subsequent eleven months until a new figure becomes available and derive the year-on-year growth rates for each month accordingly. However, this may induce estimation bias. If a firm receives the treatment of constrained credit supply in October 2008, for example, the employment growth rate in $t+12$ (i.e. October 2009) corresponds to the growth between November 2007 and November 2008, which captures a large extent of what is in fact pre-treatment growth, but only one month of post-treatment growth. Estimated credit-supply side effects on firm-level employment growth would therefore be potentially biased.

According to the second alternative, we may assume the number of employees in each of the eleven prior months to be equal to those reported for November. However, this may also induce bias. If a firm receives the treatment of constrained credit supply in December 2008, for example, the employment growth rate in $t+12$ (i.e. December 2009) corresponds to the actual growth between November 2009 and November 2010, which captures only a very small part of the post-treatment year. Estimated credit-supply side effects on firm-level employment growth would again be potentially biased.

The approach applied in this paper is a compromise between the two described alternatives. We assume the number of employees reported in November also to be true for the five previous and the six subsequent months and calculate year-on-year growth rates for every month accordingly. For some observations, this may still not capture post-treatment employment growth precisely, but this intermediate solution is expected to deal with the issue in the most conservative way.

Figure 1: Number of treated firms over time



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

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

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

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

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

| | OLS | | PSM | |
|---------------------|------------------------------|---------------------|---------------------------|------------------|
| | (1) | (2) | (3) | (4) |
| <i>Constrained</i> | 8.87%*** (0.026) | 7.37%*** (0.026) | 5.74%** (0.023) | 3.16% (0.024) |
| | difference = 1.50%*** | | difference = 2.58% | |
| Firm size | Yes | Yes | Yes | Yes |
| Balance sheet data | Yes | Yes | Yes | Yes |
| Current situation | No | Partly | No | Partly |
| Future expectations | No | Partly | No | Partly |
| Time | Yes | Yes | Yes | Yes |
| Industry | Yes | Yes | Yes | Yes |
| p>t | 0.00 | 0.00 | 0.01 | 0.19 |
| Upper bound | 13.22% | 11.66% | 9.44% | 7.12% |
| Lower bound | 4,52% | 3.07% | 2.03% | -0.80% |
| Adj. R^2 | 0.168 | 0.190 | | |
| Treated obs. | 337 | 337 | 245 | 214 |
| Untreated obs. | 5,166 | 5,166 | 1,516 | 1,298 |

Notes: Columns (1) and (2) provide results for OLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Constrained_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; columns (3) and (4) provide results for corresponding WLS estimations with weights being derived from PSM based on different sets of pre-treatment control variables; “Firm size”, “Balance sheet data”, “Current situation”, and “Future expectations” are reduced sets of control variables (excluding general appraisals) as explained in Section 5.1; time effects are controlled for by including months dummy variables in OLS estimations and matching firms only within quarters in the PSM estimations; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples analysed in Estimations (1) and (2) contain only those observations for which all control variables of Estimation (2) are available; standard errors are reported in parentheses; p-values are reported for a t-test of significance of the estimated treatment effects; upper and lower bounds are reported for the 95 percent confidence interval; differences between the estimates based on the alternative sets of pre-treatment control variables are provided; for OLS its significance is tested using a t-test with $H_0: \beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

| | OLS | | PSM | |
|---------------------|-----------------------------|---------------------|---------------------------|-------------------|
| | (1) | (2) | (3) | (4) |
| <i>Constrained</i> | 5.45%*** (0.010) | 4.77%*** (0.010) | 3.78%*** (0.012) | 2.78%* (0.015) |
| | difference = 0.68%** | | difference = 1.01% | |
| Firm size | Yes | Yes | Yes | Yes |
| Balance sheet data | Yes | Yes | Yes | Yes |
| Current situation | No | Yes | No | Yes |
| Future expectations | No | Yes | No | Yes |
| Time | Yes | Yes | Yes | Yes |
| Industry | Yes | Yes | Yes | Yes |
| p>t | 0.000 | 0.000 | 0.001 | 0.060 |
| Upper bound | 7.11% | 6.35% | 5.69% | 5.23% |
| Lower bound | 3.79% | 3.20% | 1.88% | 0.33% |
| Adj. R^2 | 0.226 | 0.304 | | |
| Treated obs. | 316 | 316 | 223 | 141 |
| Untreated obs. | 4,877 | 4,877 | 1,351 | 806 |

Notes: Columns (1) and (2) provide results for OLS estimations of $Slowdown_{avg_{i,t+12}}$ on the treatment status $Constrained_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; columns (3) and (4) provide results for corresponding WLS estimations with weights being derived from PSM based on different sets of pre-treatment control variables; “Firm size”, “Balance sheet data”, “Current situation”, and “Future expectations” are sets of control variables as listed in Table 1; time effects are controlled for by including months dummy variables in OLS estimations and matching firms only within quarters in the PSM estimations; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples analysed in Estimations (1) and (2) contain only those observations for which all control variables of Estimation (2) are available; standard errors are reported in parentheses; p-values are reported for a t-test of significance of the estimated treatment effects; upper and lower bounds are reported for the 95 percent confidence interval; differences between the estimates based on the alternative sets of pre-treatment control variables are provided; for OLS its significance is tested using a t-test with $H_0: \beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Alternative OLS and PSM estimations using $\Delta Empl_{i,t+12}$

| | OLS | | PSM | |
|---------------------|-----------------------------|---------------------|---------------------------|------------------|
| | (1) | (2) | (3) | (4) |
| <i>Constrained</i> | -1.80%*** (0.006) | -1.52%** (0.006) | -1.08%* (0.006) | 0.35% (0.008) |
| | difference = 0.25%** | | difference = 1.43% | |
| Firm size | Yes | Yes | Yes | Yes |
| Balance sheet data | Yes | Yes | Yes | Yes |
| Current situation | No | Yes | No | Yes |
| Future expectations | No | Yes | No | Yes |
| Time | Yes | Yes | Yes | Yes |
| Industry | Yes | Yes | Yes | Yes |
| p>t | 0.003 | 0.011 | 0.07 | 0.65 |
| Upper bound | -0.81% | -0.54% | -0.11% | 1.64% |
| Lower bound | -2.80% | -2.50% | -2.04% | -0.94% |
| Adj. R^2 | 0.060 | 0.085 | | |
| Treated obs. | 302 | 302 | 208 | 130 |
| Untreated obs. | 4,739 | 4,739 | 1,295 | 773 |

Notes: Columns (1) and (2) provide results for OLS estimations of $\Delta Empl_{i,t+12}$ on the treatment status $Constrained_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; columns (3) and (4) provide results for corresponding WLS estimations with weights being derived from PSM based on different sets of pre-treatment control variables; “Firm size”, “Balance sheet data”, “Current situation”, and “Future expectations” are sets of control variables as listed in Table 1; time effects are controlled for by including months dummy variables in OLS estimations and matching firms only within quarters in the PSM estimations; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples analysed in Estimations (1) and (2) contain only those observations for which all control variables of Estimation (2) are available; standard errors are reported in parentheses; p-values are reported for a t-test of significance of the estimated treatment effects; upper and lower bounds are reported for the 95 percent confidence interval; differences between the estimates based on the alternative sets of pre-treatment control variables are provided; for OLS its significance is tested using a t-test with $H_0: \beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Alternative OLS and PSM estimations using $Empl_decrease_{i,t+12}$

| | OLS | | PSM | |
|---------------------|------------------------------|---------------------|---------------------------|------------------|
| | (1) | (2) | (3) | (4) |
| <i>Constrained</i> | 9.37%*** (0.027) | 7.49%*** (0.026) | 6.23%** (0.025) | 1.24% (0.032) |
| | difference = 1.88%*** | | difference = 4.99% | |
| Firm size | Yes | Yes | Yes | Yes |
| Balance sheet data | Yes | Yes | Yes | Yes |
| Current situation | No | Yes | No | Yes |
| Future expectations | No | Yes | No | Yes |
| Time | Yes | Yes | Yes | Yes |
| Industry | Yes | Yes | Yes | Yes |
| p>t | 0.001 | 0.005 | 0.01 | 0.70 |
| Upper bound | 13.80% | 11.84% | 10.34% | 6.56% |
| Lower bound | 4.94% | 3.15% | 2.13% | -4.08% |
| Adj. R^2 | 0.088 | 0.126 | | |
| Treated obs. | 314 | 314 | 221 | 136 |
| Untreated obs. | 4,827 | 4,827 | 1,340 | 792 |

Notes: Columns (1) and (2) provide results for OLS estimations of $Empl_decrease_{i,t+12}$ on the treatment status $Constrained_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; columns (3) and (4) provide results for corresponding WLS estimations with weights being derived from PSM based on different sets of pre-treatment control variables; “Firm size”, “Balance sheet data”, “Current situation”, and “Future expectations” are sets of control variables as listed in Table 1; time effects are controlled for by including months dummy variables in OLS estimations and matching firms only within quarters in the PSM estimations; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples analysed in Estimations (1) and (2) contain only those observations for which all control variables of Estimation (2) are available; standard errors are reported in parentheses; p-values are reported for a t-test of significance of the estimated treatment effects; upper and lower bounds are reported for the 95 percent confidence interval; differences between the estimates based on the alternative sets of pre-treatment control variables are provided; for OLS its significance is tested using a t-test with $H_0: \beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Treated firm characteristics in t-1 by matching samples

| | Matching on size & balance sheet | Matching on all variables | |
|----------------------------|-------------------------------------|------------------------------|---------|
| t-1 | \bar{X}_B | \bar{X}_{BCF} | $p > t$ |
| Firm size | | | |
| <i>log(Empl)</i> | 5.41 | 5.51 | 0.45 |
| <i>log(Assets)</i> | 17.01 | 17.09 | 0.66 |
| Balance sheet data | | | |
| <i>Equity</i> | 35.91% | 36.21% | 0.90 |
| <i>Cash</i> | 9.83% | 10.78% | 0.48 |
| <i>Long-term_debt</i> | 16.48% | 16.11% | 0.87 |
| <i>Short-term_debt</i> | 30.67% | 30.78% | 0.96 |
| <i>Cash_flow</i> | 8.98% | 9.03% | 0.96 |
| <i>ROA</i> | -4.94% | -3.33% | 0.66 |
| <i>Interest_coverage</i> | 16.68 | 14.76 | 0.64 |
| Current situation | | | |
| <i>State (+)</i> | 22.62% | 21.28% | 0.76 |
| <i>State (-)</i> | 30.32% | 29.08% | 0.80 |
| <i>Orders (+)</i> | 9.05% | 08.51% | 0.86 |
| <i>Orders (-)</i> | 40.72% | 40.43% | 0.96 |
| <i>Production (+)</i> | 12.67% | 7.80% | 0.15 |
| <i>Production (-)</i> | 88.24% | 90.78% | 0.45 |
| <i>Short-time</i> | 10.86% | 05.67% | 0.09 |
| <i>Export</i> | 26.70% | 30.50% | 0.43 |
| Future expectations | | | |
| <i>State_exp (+)</i> | 16.29% | 12.06% | 0.27 |
| <i>State_exp (-)</i> | 27.60% | 30.50% | 0.55 |
| <i>Empl_exp (+)</i> | 4.52% | 2.84% | 0.42 |
| <i>Empl_exp (-)</i> | 23.53% | 19.86% | 0.41 |
| <i>Headcount (+)</i> | 19.46% | 16.31% | 0.45 |
| <i>Headcount (-)</i> | 28.96% | 27.66% | 0.79 |
| <i>Short-time_exp</i> | 5.88% | 5.67% | 0.93 |
| N | 221 | 141 | |

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

Table C.7: Financial crisis, OLS and PSM estimations using $Slowdown_{i,t+12}$

| | OLS | | PSM | |
|---------------------|----------------------------|-------------------|---------------------------|------------------|
| | (1) | (2) | (3) | (4) |
| <i>Constrained</i> | 7.31%** (0.032) | 5.87%* (0.031) | 4.97%* (0.026) | 3.84% (0.031) |
| | difference = 1.44%* | | difference = 1.14% | |
| Firm size | Yes | Yes | Yes | Yes |
| Balance sheet data | Yes | Yes | Yes | Yes |
| Current situation | No | Yes | No | Yes |
| Future expectations | No | Yes | No | Yes |
| Time | Yes | Yes | Yes | Yes |
| Industry | Yes | Yes | Yes | Yes |
| p>t | 0.023 | 0.061 | 0.06 | 0.21 |
| Upper bound | 12.61% | 11.02% | 9.25% | 8.87% |
| Lower bound | 2.02% | 0.72% | 0.70% | -1.20% |
| Adj. R^2 | 0.209 | 0.255 | | |
| Treated obs. | 210 | 210 | 160 | 116 |
| Untreated obs. | 3,105 | 3,105 | 928 | 690 |

Notes: For the subsample as of July 2007, columns (1) and (2) provide results for OLS estimations of $Slowdown_{i,t+12}$ on the treatment status $Constrained_{i,t}$ and different sets of pre-treatment control variables; standard errors (reported in parentheses) are clustered at the firm-level; columns (3) and (4) provide results for corresponding WLS estimations with weights being derived from PSM based on different sets of pre-treatment control variables; “Firm size”, “Balance sheet data”, “Current situation”, and “Future expectations” are sets of control variables as listed in Table 1; time effects are controlled for by including months dummy variables in OLS estimations and matching firms only within quarters in the PSM estimations; industry dummy variables are included based on the two-digit WZ 2008 industry classification; the two samples analysed in Estimations (1) and (2) contain only those observations for which all control variables of Estimation (2) are available; standard errors are reported in parentheses; p-values are reported for a t-test of significance of the estimated treatment effects; upper and lower bounds are reported for the 95 percent confidence interval; differences between the estimates based on the alternative sets of pre-treatment control variables are provided; for OLS its significance is tested using a t-test with $H_0: \beta_1^B = \beta_1^{BCF}$ based on Clogg, Petkova, and Haritou (1995); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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