Empirical Essays on Business Cycle Analysis and Financial Constraints

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To my parents,
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Preface

The recent financial crisis highlights the importance of unwarranted expectations and their abrupt correction for fluctuations in economic activity. In the run-up to the crisis, consumers, creditors, and professional investors' were overly optimistic about the U.S. housing market, credit ratings, and structured mortgage products (Brunnermeier 2009). However, this optimism proved to be unwarranted.

The first chapter of this dissertation more generally asks to what extent changes of expectations are an autonomous source of business cycle fluctuations. This question dates back to Pigou (1927), who discusses the possibility that "errors of undue optimism or undue pessimism" are a genuine cause of "industrial fluctuations". As another source, Pigou identifies "autonomous monetary causes", which he relates to shifts in monetary or banking policies. In the case of the recent financial crisis, both are evident. During its course, banks came under distress and reduced their lending to the real economy, suppressing investment. However, disentangling supply from demand driven credit contractions is challenging (see, for instance, Bernanke and Gertler 1995, Oliner and Rudebusch 1996, and Peek, Rosengren, and Tootell 2003). The second and third chapter of this thesis take aim at this question. Chapter 2 elaborates on the measurement of financial constraints and Chapter 3 studies the empirical identification of supply driven restrictions to bank lending.

In Chapter 1, we assess the contribution of "undue optimism", as noted by Pigou (1927), to short-run fluctuations. More recently, Beaudry and Portier (2004) explore the possibility of "Pigou cycles" in a quantitative business cycle model featuring possibly undue expectations regarding future productivity. Lorenzoni (2009), in turn, puts forward a model in which misperceptions regarding the current state of productivity turn out to be an important source of business cycle fluctuations.

In this chapter, we take up the issue empirically and investigate the contribution of undue optimism and pessimism to business cycles fluctuations. Estimating a vector autoregression (VAR) on U.S. time-series data, we seek to identify "optimism shocks", that is, changes in expectations due to a perceived change in total factor productivity which does not actually materialize. Blanchard, L'Huillier, and Lorenzoni (2013) show that this constitutes a formidable challenge, because optimism shocks or, quite generally, misperceptions are mistakes of market participants. As such they cannot be uncovered on the basis of standard time-series techniques. Instead, one may resort to estimating fully specified general equilibrium models (Barsky and Sims 2012) or exploit information not available to market participants in real time.

Our analysis is based on this insight. Specifically, our identification strategy relies on an *ex-post* measure of agents' misperceptions, namely the nowcast error regarding current output growth. Drawing on the Survey of Professional Forecasters (SPF), we compute it as the difference between actual output growth in a given quarter and the median of the predicted values in real time. A positive realization of the nowcast error thus implies that nowcasts have been too pessimistic. Nevertheless, it is important to keep in mind that, as a reduced-form measure, nowcast errors may be the result not only of optimism shocks, but of various structural innovations.

Nowcast errors play a key role in our analysis as they allow us to recover optimism shocks from actual time series data. We establish this result within a business cycle model which mimics, in a stylized way, the informational friction which gives rise to nowcast errors. The model is a version of the dispersed-information model of Lorenzoni (2009), for which we are able to obtain closed-form solutions. Using the model, we also derive the identification restrictions on which we rely in the main part of our analysis. Specifically, drawing on earlier work by Galí (1999) and others, we estimate a VAR model on time-series data for labor productivity, employment, and the nowcast error. In order to identify the distinct contributions of optimism and productivity shocks to short-run fluctuations, we assume, in line with our theoretical results, that nowcast errors may emerge only as a result of optimism or productivity shocks. Yet optimism shocks, in contrast to productivity shocks, have no bearing on labor productivity in the long run.

According to the estimated VAR model, optimism shocks—as predicted by theory—induce a *negative* nowcast error, yet significantly boost economic activity at the same time. This result is noteworthy, because we do not restrict the response of the nowcast error to optimism shocks. Moreover, as the unconditional correlation between nowcast errors and economic activity is positive, the change of the correlation conditional on optimism shocks lends additional support to our identification strategy. Instead, productivity shocks induce a *positive* nowcast error while also stimulating economic activity. These results are robust across a range of alternative specifications, including alternative measures of the nowcast error. Finally, computing a forecast error variance decomposition, we find that optimism shocks account for up to 30 percent of output fluctuations.

Turning to Pigou's (1927) "autonomous monetary causes" of industrial fluctuations, Chapter 2 studies the measurement of financial constraints. The analysis of financial-market imperfections and their impact on firms' investment decisions occupies a prominent place in macroeconomics and corporate finance (Hubbard 1998). The measurement of financial constraints is key for the empirical strand of this research and the literature has suggested a variety of indices and sorting criteria based on firm characteristics. However, there is considerable debate about their relative merits.

Fazzari and Petersen (1988) constitute investment-cash flow sensitivities as a measure of financial constraints and motivate a large subsequent literature. However, Kaplan and Zingales (1997) call the findings of this literature into question. Examining the annual reports and 10-K filings of the sub-sample of firms, which Fazzari and Petersen (1988) identify as most financially constrained, Kaplan and Zingales (1997) find arguably less financially constrained firms to show significantly greater sensitivities. Subsequently, Lamont, Polk, and Saa-Requejo (2001) introduce the Kaplan and Zingales (KZ) index utilizing their sample and classification scheme. However, there is still considerable debate on the correct measurement of financial constraints (see, for instance, Cleary 2006, Whited and Wu 2006, and Hadlock and Pierce 2010).

We add to this debate by evaluating the KZ index as well as two more recently suggested alternatives, the Whited & Wu (WW) and the Size & Age (SA) index. Following the approach of Hadlock and Pierce (2010), we explain firms' qualitative assessments of their financing conditions by the quantitative variables employed by the indices. Subsequently, we infer from the signs and the significance levels of the regression coefficients as well as from the overall model fit on the appropriateness of the tested indicators to measure financial constraints. Moreover, we study the sensitivity of our estimates with respect to non-linear variable transformations based on fractional polynomials.

However, the value of our exercise emerges from the data. We employ a survey-based measure of financial constraints obtained from a sample of German manufacturing firms running from 1989 to 2009. This measure is not subject to the endogeneity critique put forward by Hadlock and Pierce (2010), who find the same information used to construct both, the dependent variable as well as the explanatory variables comprising the KZ index. In addition, we utilize survey-based assessments of firms sales expectations as well as of the profitability of the investment projects they face. Thus, we control for firms investment opportunities without relying on measures of q.

Despite the warranted criticism, we provide evidence that the KZ index is a valid measure of financial constraints. Our results are particularly striking given the narrow focus of the original KZ sample. For the WW and the SA index, however, evidence is mixed. Although we find the WW index to significantly outperform a random classification scheme, coefficient estimates for the comprised indicators are not in line with the original loadings. In particular, the industry sales growth variable, which loads positively on the index, is significantly negatively associated with our qualitative financial constraints indicator. Yet, Whited and Wu (2006) employ the variable in order to capture the availability of attractive investment opportunities (high industry sales growth), which are supposed to be positively associated with (binding) financial constraints. Moreover, for the SA index, we reject the hypothesis of external validity. Specifically, the index fails to outperform a random classification algorithm in identifying financially constrained firms. This result is relevant given that the authors claim the SA index to be a reasonable choice for measuring financial constraints in many contexts after having extensively studied its robustness and out of sample performance.

The final Chapter studies the impact of financial constraints on bank dependent firms. From a policy perspective, the effectiveness and efficiency of any policy response towards recessions and credit slumps crucially depends on the understanding of the extent to which credit market outcomes are driven by supply-side or demand-side factors. If undercapitalized banks are a burden on the economy, government intervention may well be justified. However, if on the contrary growth is hampered by firm-side factors (e.g. by subdued expectations or low creditworthiness) and credit volumes go down in response to weak credit demand or higher default risk, government interventions should not necessarily aim at banks.

The empirical analysis of the effects of supply-driven changes in bank lending restrictions on real economic activity is complicated by the need to rule out demand-side factors. On the firm-level, the existing literature primarily does so based on firms' balance sheet information. Balance sheets, however, are published on a low frequency and are backward-looking in nature. In particular, they contain little information on firms' expectations. In this study, we observe firms' self-perceived bank lending restrictions on an almost monthly frequency and ask the question of whether controlling for similarly high-frequent survey-based indicators of firms current states and future prospects in addition to balance sheet information impacts the inference on the identified treatment effects.

We employ a panel data of German manufacturing firms in which bank lending restrictions are mainly driven by the 2007/08 financial crisis. At the beginning of our analysis, we estimate the effect of restrictive bank lending on firm-level employment growth using a matching estimator based on balance sheet variables only. The results suggest a significant supply-driven effect of bank lending restrictions on firm-level employment growth. However, this effect is not confirmed once we control for survey-based appraisals of firms' current business situations and future expectations. Specifically, treatment effects turn out to be significantly lower while balancing properties improve considerably. In contrast, balancing properties are poor in the case of matching on balance sheet variables only, revealing significant bias from unbalanced contemporaneous and forward-looking firm-specific indicators. Finally, robustness exercises confirm that our results hold irrespective of the matching algorithm or the adjustment of extreme values in employment growth rates and balance sheet variables.

Our findings indicate that estimates of firm-level effects of bank lending restrictions are sensitive to the incorporation of contemporaneous and forward-looking information on firms' credit demand. Indeed, their omission may cause considerable bias. For this reason, our results ask researchers to cautiously infer on the effects of bank lending restrictions if they rely on balance sheet information only.

Chapter 1

Growth expectations, undue optimism, and short-run fluctuations

We assess the contribution of "undue optimism" (Pigou) to short-run fluctuations. In our analysis, optimism pertains to total factor productivity which determines economic activity in the long run, but is not contemporaneously observed by market participants. In order to recover optimism shocks—autonomous, but fundamentally unwarranted changes in the assessment of productivity—from actual time series, we rely on an informational advantage over market participants. Specifically, we compute the nowcast error regarding current output growth drawing on the Survey of Professional Forecasters. Including nowcast errors in a vector autoregression model makes it possible to identify optimism shocks. Optimism shocks, in line with theory, induce a negative nowcast error but raise economic activity in the short run. They account for up to 30 percent of short-run fluctuations.

1.1 Introduction

Economic outcomes depend on expectations and vice versa. In this paper, we ask to what extent changes of expectations are an *autonomous* source of business cycle fluctuations. This question dates back to Pigou (1927) who discusses the possibility that "errors of undue optimism or undue pessimism" are a genuine cause of "industrial fluctuations." Keynes' notion of "animal spirits" is a related, but distinct concept.[1] More recently, Beaudry and

[1] Keynes' animal spirits are "a spontaneous urge to action rather than inaction", which drive economic decisions beyond considerations based "on nothing but a mathematical expectation" (Keynes 1936, pp. 161 and 162).

Portier (2004) explore the possibility of "Pigou cycles" in a quantitative business cycle model featuring possibly undue expectations regarding future productivity. Lorenzoni (2009), in turn, puts forward a model in which misperceptions regarding the current state of productivity turn out to be an important source of business cycle fluctuations.

In this paper, we take up the issue empirically and investigate the contribution of undue optimism and pessimism to business cycles fluctuations. Estimating a vector autoregression (VAR) on U.S. time-series data, we seek to identify "optimism shocks", that is, changes in expectations due to a perceived change in total factor productivity which does not actually materialize. Blanchard, L'Huillier, and Lorenzoni (2013) show that this constitutes a formidable challenge, because optimism shocks or, quite generally, misperceptions are mistakes of market participants. As such they cannot be uncovered on the basis of standard time-series techniques. Instead, one may resort to estimating fully specified general equilibrium models (Barsky and Sims 2012) or exploit information not available to market participants in real time.

Our analysis is based on this insight. Specifically, our identification strategy relies on an *ex-post* measure of agents' misperceptions, namely the nowcast error regarding current output growth. Drawing on the Survey of Professional Forecasters (SPF), we compute it as the difference between actual output growth in a given quarter and the median of the predicted values in real time. A positive realization of the nowcast error thus implies that nowcasts have been too pessimistic. Yet it is important to keep in mind that, as a reduced-form measure, nowcast errors may be the result not only of optimism shocks, but of various structural innovations.

The SPF is a widely recognized measure of private sector expectations regarding the current state and prospects of the U.S. economy. It is also a frequently used benchmark to assess forecasting models. Nevertheless, as we show in the first step of our analysis, nowcast errors can be sizable. Depending on whether we consider the first or the final release of data for actual output growth, the largest nowcast error exceeds 1 or 1.75 percentage points of quarterly output growth respectively. We also document that nowcast errors are positively correlated with economic activity and investigate the effect of well-known measures of structural innovations on nowcast errors. We find that innovations which are publicly observable, such as monetary and fiscal policy shocks or uncertainty shocks, do not cause nowcast errors. In contrast, productivity shocks have a significant effect on nowcast errors, presumably because they impact current output growth, but are not observable in real time.

Nowcast errors play a key role in our analysis as they allow us to recover optimism shocks from actual time series data. We establish this result within a business cycle model which mimics, in a stylized way, the informational friction which gives rise to nowcast errors. The model is a version of the dispersed-information model of Lorenzoni (2009), for which we are able to obtain closed-form solutions. Using the model, we also derive the identification restrictions on which we rely in the main part of our analysis. Specifically, drawing on earlier work by Galí (1999) and others, we estimate a VAR model on time-series data for labor productivity, employment, and the nowcast error. In order to identify the distinct contributions of optimism and productivity shocks to short-run fluctuations, we assume, in line with our theoretical results, that nowcast errors may emerge only as a result of optimism or productivity shocks. Yet optimism shocks, in contrast to productivity shocks, have no bearing on labor productivity in the long run.

According to the estimated VAR model, optimism shocks—as predicted by theory—induce a *negative* nowcast error, yet significantly boost economic activity at the same time. This result is noteworthy, because we do not restrict the response of the nowcast error to optimism shocks. Moreover, as the unconditional correlation between nowcast errors and economic activity is positive, the change of the correlation conditional on optimism shocks lends additional support to our identification strategy. Instead, productivity shocks induce a *positive* nowcast error while also stimulating economic activity. These results are robust across a range of alternative specifications, including alternative measures of the nowcast error. Finally, computing a forecast error variance decomposition, we find that optimism shocks account for up to 30 percent of output fluctuations.

Conceptually, our analysis relates to a number of recent studies on the role of exogenous shifts in expectations as a source of business cycle fluctuations. Angeletos and La'O (2013) develop a model where "sentiment shocks" arise, as market participants are unduly but simultaneously optimistic about their terms of trade. These shocks trigger aggregate fluctuations even if productivity is known to be constant. A number of contributions have focused on the distinction between current and anticipated productivity shocks. Evidence by Beaudry and Portier (2006) suggests that business cycles are largely driven by expected future changes in productivity (see also Beaudry, Nam, and Wang 2011, Schmitt-Grohé and Uribe 2012, and Leduc and Sill 2013), while Barsky and Sims (2011) find the role of expected productivity innovations to be limited. In any case, to the extent that anticipated shocks do not materialize as expected, a recession might ensue, which is thus caused by undue optimism (Jaimovic and Rebelo 2009). In our analysis, we allow misperceptions to pertain also to current, instead of only to future productivity.

We also stress that there are few attempts to identify optimism shocks empirically without imposing a fully structural model on the data. Blanchard (1993) provides an animal-spirits account of the 1990–91 recession focusing on consumption. Caroll, Fuhrer, and Wilcox (1994) show that consumer sentiment forecasts consumption spending—aside from the information contained in other available indicators. Yet in concluding they suggest a "fundamental explanation" based on habits and precautionary saving motives. Oh and Waldman (1990) show that "false macroeconomic announcements", identified as measurement error in early releases of leading indicators, cause future economic activity. They refrain from a structural interpretation, however. Mora and Schulstad (2007) show that once announcements regarding current growth are taken into account, the actual growth rate has no predictive power in determining future growth. Finally, Bachmann and Sims (2012) explore the importance of confidence for the transmission of fiscal shocks, but do not analyze the effect of exogenous variations in confidence.

The remainder of the paper is organized as follows: The next section introduces our measure of nowcast errors and provides a number of statistics illustrating their properties. Section 3 puts forward a simple model which allows us to clarify issues pertaining to the notion of optimism shocks and their identification. Section 4 presents our VAR model and results. A final section concludes.

1.2 A reduced-form measure of misperceptions

We eventually aim to uncover the effects of *optimism shocks*, that is, a perceived change in productivity which does not actually materialize. In this section, as a step towards this end, we consider a reduced-form measure of misperceptions by computing *nowcast errors* regarding current U.S. output growth. Nowcast errors can be the result of optimism shocks, but they may also be due to other structural innovations. Still, nowcast errors will play a key role in our identification strategy in Section 1.4 below. In what follows, we therefore describe the construction of nowcast errors and compute a number of statistics in order to illustrate their scope, possible causes, and their relation to economic activity.

1.2.1 Data

Our main data source is the SPF, initiated by the American Statistical Association and the NBER in 1968Q4, now maintained at the Federal Reserve Bank of Philadelphia.[2] The survey is conducted at quarterly frequency. Panelists receive the questionnaires at the end of the first month of the quarter and have to submit their answers by the 2nd to 3rd week of the following month. The results of the survey are released immediately afterwards. At this stage, no information regarding current output is available from the Bureau of Economic Analysis (BEA). At most, in order to nowcast output growth for the current quarter, forecasters may draw on the NIPA advance report regarding output in the previous quarter.

Predicted quarterly output growth is annualized and measured in real terms. Note that initially, within the SPF, output is measured by GNP, later by GDP. We compute nowcast errors by subtracting the survey's median forecast from the actual value reported later by the BEA.[3] We compute two measures of nowcast errors by distinguishing between the first and the final data release for actual output growth.[4] For the latter, we use the latest available data vintage. We thereby address concerns that the assessment of nowcasts or, more generally, forecasts depends on what is being used as the "actual" or realization (see, e.g., Stark and Croushore 2002).[5]

1.2.2 Nowcast errors

We compute nowcast errors as the difference between actual output growth in a given quarter and the median value of the predicted value. They are shown in the left panel of

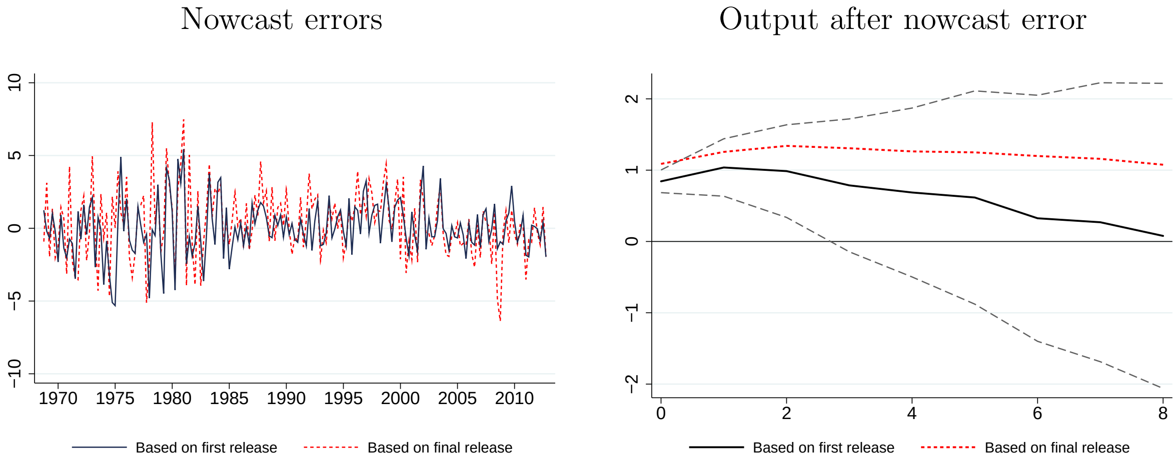
[2]Professional forecasters are mostly private, financial-sector firms. The number of participating institutions declined from 50 to fewer than 20 in 1988. After the Philadelphia Fed took over in 1990, participation rose again; see Croushore (1993). Regarding our latest observation in 2012Q4, 39 forecasters participated in the survey.

[3]For the SPF forecasts of GNP/GDP we use the series DRGDP2, which we obtain from the Real-time Data Research Center of the Philadelphia Fed. This series corresponds to the median forecast of the quarterly growth rate of real output, seasonally adjusted at annual rate (real GNP prior to 1992 and real GDP afterwards). Also note that prior to 1981Q3 the SPF asks for nominal GNP only. In this case, the forecast for the price index of GNP is applied to obtain the implied forecast for real GNP.

[4]Data are obtained from BEA and the Philadelphia Fed's Real-time Data Set for Macroeconomists. First-release data: BEA's first (advance) estimate of the quarterly growth rate of real GNP/GDP (seasonally adjusted at annual rate, with real GNP prior to 1992 and real GDP for 1992-present): ROUTPUT. Final-release data: series GNPC96 and GDPC96 which are quarterly Gross National/Domestic Product, seasonally adjusted at annual rates, chained 2005 Dollars.

[5]In fact, the authors consider a set of alternative definitions of actuals and find statistically significant differences of forecast evaluations for real output. We show below, however, that our results hold independently of the choice of first- or final-release data.

Figure 1.1: Nowcast error



Notes: Left panel: series based on first-release data (solid) and final-release data (dashed). Errors are measured in annualized percentage points (vertical axis). Right panel: cumulative impulse response of output growth to nowcast error based on local projections. Horizontal axis measures quarters, vertical axis measures percentage deviation of output from the average-growth path. Dashed lines indicate 90 percent confidence bounds implied by Newey-West standard errors.

Figure 1.1, measured in annualized percentage points. The solid (dashed) line represents results based on first-release (final-release) data. Although the two series co-move strongly (correlation: 0.55), there are perceptible differences. For instance, there are sizable negative errors in the second half of 2008 only for the measure based on final-release data. Presumably, at the beginning of the great recession the actual growth slowdown was larger not only relative to what professional forecasters predicted in real time, but also relative to what initial data suggested. Moreover, errors based on first-release data are shifted downwards relative to those based on final-release data, notably in the first half of the sample.

This is confirmed by the summary statistics reported in Table 1.1: the mean of the nowcast errors is significantly positive if we consider final-release data, but not significantly different from zero in the case of first-release data. The standard error and the largest realizations of the nowcast error are also considerably larger in the case of final-release data.[6] The difference is likely due to the revision process of the statistical office and particularly to benchmark revisions. We therefore rely on first-release data in our baseline

[6]This finding is consistent with evidence provided by Faust, Rogers, and Wright (2005) regarding GDP announcements. For G7 countries it turns out that revisions of initial announcements are significantly positive on average in their sample period. Note, however, that the mean of final-release nowcast errors becomes insignificant once we control for productivity shocks below.

VAR model in section 1.4, and consider final-release data in our sensitivity analysis. Finally, the last two columns of Table 1.1 report results of a Ljung–Box test, suggesting that there is no serial correlation in both series.

Table 1.1: Summary statistics nowcast errors

						Ljung–Box test	
	N	Mean	SD	Min	Max	Q-stat.	p-value
Final-release based	177	.35**	2.36	-6.38	7.49	2.59	.96
First-release based	177	.04	1.86	-5.31	5.43	8.68	.37

Notes: Nowcast errors computed on the basis of final-release (top row) and first-release (bottom row) data, measured in annualized percentage points; sample: 1968Q4 - 2012Q4. Means are tested against zero based on a standard t-test. '**' indicates significance at the 5% level. The last two columns report Q-statistics and p-values for a Ljung-Box test assessing the null hypothesis of zero autocorrelations up to 8 lags.

What causes nowcast errors? Assuming that the average forecaster has a correct understanding of the economy, structural innovations that are public information should not induce systematic forecast errors. On the other hand, structural innovations that are not directly observable may generate nowcast errors. To assess this hypothesis, we run regressions of nowcast errors on popular (and relatively uncontroversial) series of structural innovations. Specifically, we consider monetary policy shocks identified by Romer and Romer (2004), tax shocks identified by Romer and Romer (2010), uncertainty shocks identified by Bloom (2009), and productivity shocks provided by Fernald (2012).[7]

In each instance, we regress nowcast errors on the contemporaneous realization of the structural shock, while also including four lags of the nowcast error in the regression model. The sample varies across regressions, since we use the longest overlapping sample in each case. Results for the impact effect are reported in Table 1.2. Newey-West standard errors

[7] Following Basu, Fernald, and Kimball (2006), Fernald constructs a utilization-adjusted series of TFP at quarterly frequency. In terms of actual series we use the "utilization-adjusted TFP in producing non-equipment output" (dtfp_C_util) of Fernald (2012). For the uncertainty shocks we rely on the quarterly average of the monthly series of stock-market volatility shocks identified in the baseline VAR of Bloom (2009). In the case of monetary and tax shocks we use the quarterly average of the monthly shock series (RESID) and the "sum of Deficit-Driven and Long-Run Tax Changes" (EXOGENRRATIO) of Romer and Romer (2004) and Romer and Romer (2010) respectively.

are reported in parentheses. The top row reports results based on the final-release data, the bottom row is based on the first-release data. We find that for monetary and fiscal policy innovations, as well as for uncertainty shocks, there is indeed no significant impact on nowcast errors, in line with the hypothesis that the effect of observable innovations is relatively well understood by forecasters. Instead, it is productivity innovations that have a significant impact. Specifically, positive productivity innovations tend to raise the nowcast error contemporaneously, that is, they tend to raise the growth of economic activity beyond the expected level.

Table 1.2: Nowcast errors and structural innovations to...

	Monetary Policy	Taxes	Uncertainty	Productivity
	1969:1 - 1996:4	1968:4 - 2007:4	1963:3 - 2008:2	1968:4 - 2009:3
Final-release based	1.678	-.002	.451	.480***
	(1.044)	(1.144)	(.435)	(.049)
First-release based	1.611	-.730	.088	.140***
	(.978)	(.963)	(.260)	(.045)

Notes: Impact effect on nowcast error obtained from regressing the nowcast error on the time series for the structural innovations to monetary policy, fiscal policy (taxes), uncertainty, and productivity. The regression includes four lags of the nowcast error. Newey-West standard errors robust for autocorrelation up to four lags are reported in parentheses; time series of structural innovations to monetary policy, taxes, uncertainty, and productivity provided by Romer and Romer (2004), Romer and Romer (2010), Bloom (2009), and Fernald (2012) respectively.

1.2.3 Nowcast errors and economic activity

Nowcast errors are positive surprises regarding current activity. They are also positively correlated with output growth.[8] To explore systematically how current nowcast errors relate to economic activity, we estimate the dynamic relationship on the basis of local projections (see Jordà 2005). In particular, we relate current and future output growth to

[8]The correlation between GDP growth (final-release data) and the nowcast error is .73 and .47 for the final-release and first-release measure respectively.

current nowcast errors.[9]

The right panel of Figure 1.1 shows the cumulative impulse response of output growth to a nowcast error. The horizontal axis measures quarters, the vertical axis percentage deviation of output from the constant-growth path. Dashed lines indicate 90 percent confidence bounds implied by Newey-West standard errors. We find that nowcast errors predict a strong, mildly hump-shaped increase of economic activity. The effect is stronger for our measure based on the final-release data, yet differences are moderate relative to the one based on first-release data. The finding that (reduced-form) nowcast errors predict future activity is noteworthy in light of the results regarding effects of optimism shocks documented in Section 1.4 below. Before discussing this evidence, we provide a theoretical rationale for our empirical framework in the following section.

1.3 The model

In this section we put forward a model which allows us to formally define optimism shocks, discuss conditions under which they may affect economic activity and, importantly, clarify issues pertaining to identification. Coibion and Gorodnichenko (2012) find that models of information rigidities in general, and of noisy information in particular, are successful in predicting empirical regularities of survey data on expectations. Our model thus builds on the noisy and dispersed information model of Lorenzoni (2009), a key feature of which is that agents do not observe current output. As our goal is to derive robust qualitative predictions, we simplify the original model, notably by assuming predetermined rather than staggered prices. As a result, it is possible to solve an approximate model in closed form.

1.3.1 Setup and timing

There is a continuum of islands (or locations), indexed by $l \in [0, 1]$, each populated by a representative household and a unit mass of producers, indexed by $j \in [0, 1]$. Each household buys from a subset of all islands, chosen randomly in each period. Specifically, it buys from all producers on n islands included in the set $\mathcal{B}_{l,t}$, with $1 < n < \infty$.[10] Households

[9] To capture potential serial correlation, we apply Newey-West standard errors. The error structure is assumed to be possibly heteroskedastic and autocorrelated up to lag 4. We also include four lags of GDP growth in the regression.

[10] This setup ensures that households cannot infer aggregate productivity exactly from observed prices. At the same time, individual producers have no impact on the price of households' consumption baskets.

have an infinite planning horizon. Producers produce differentiated products on the basis of an island-specific productivity, which is determined by a permanent, economy-wide component and a temporary, idiosyncratic component. Both components are stochastic. Financial markets are complete such that, assuming identical initial positions, wealth levels of households are equalized at the beginning of each period.

The timing of events is as follows: Each period consists of three stages. During stage one of period t, information about all variables of period $t-1$ is released. Subsequently, nominal wages are determined. Finally, the central bank sets the interest rate based on expected inflation.

Shocks realize during the second stage. We distinguish between shocks which are directly observable and shocks which are not. Optimism and productivity shocks fall in the latter category. In particular, information about idiosyncratic productivity is private to each producer. Additionally, all agents observe a signal about average productivity. While the signal is unbiased, it contains an i.i.d. zero-mean component: the optimism shock.[11] In terms of observable shocks, we allow for monetary policy shocks. Yet, rather than being interested in the effects of monetary policy shocks per se, we merely aim at contrasting the effects of observable shocks on nowcast errors to those of non-observable shocks. Given these information sets, producers set prices.

During the third and final stage, households split up. Workers work for all firms on their island, while consumers allocate their expenditures across differentiated goods based on public information, including the signal, and information contained in the prices of the goods in their consumption bundle. Because the common productivity component is permanent and households' wealth and information is equalized in the next period, agents expect the economy to settle on a new steady state from period $t+1$ onwards.

1.3.2 Households

A representative household on island l maximizes lifetime utility given by

$$U_{l,t} = E_{l,t} \sum_{k=t}^{\infty} \beta^{k-t} \ln C_{l,t} - \frac{L_{l,t}^{1+\varphi}}{1+\varphi} \qquad \varphi \geq 0, \quad 0 < \beta < 1,$$

with $E_{l,t}$ being the expectation operator based on household l's information set at the time of its consumption decision (see below). $C_{l,t}$ denotes the consumption basket of household

[11]We refer to this signal component throughout as an "optimism shock" with the understanding that realizations may be positive (optimism shock) or negative (pessimism shocks).

l, while $L_{l,t}$ is its labor supply. The flow budget constraint is given by

$$E_t(\varrho_{l,t,t+1}A_{l,t})+B_{l,t}+\sum_{m\in\mathcal{B}_{l,t}}\int_0^1 P_{j,m,l,t}C_{j,m,l,t}dj \leq \int_0^1 \Pi_{j,l,t}dj+W_{l,t}L_{l,t}+A_{l,t-1}+(1+r_{t-1})B_{l,t-1},$$

where $C_{j,m,l,t}$ denotes the amount bought by household l from producer j on island m and $P_{j,m,l,t}$ is the price for one unit of $C_{j,m,l,t}$. $\Pi_{j,l,t}$ are profits of firm j on island l and $\varrho_{l,t,t+1}$ is household l's stochastic discount factor between t and $t+1$. At the beginning of the period, the household receives a payoff $A_{l,t-1}$ from its portfolio of state-contingent securities, purchased in the previous period. $B_{l,t}$ are state non-contingent bonds paying an interest rate of r_t. A complete set of state-contingent securities is traded at the beginning of the period, while state non-contingent bonds can be traded via the central bank throughout the entire period.[12] The interest rate of the non-contingent bond is set by the central bank. All financial assets are in zero net supply. The bundle $C_{l,t}$ of goods purchased by household l consists of goods sold in a subset of all islands in the economy

$$C_{l,t} = \left(\frac{1}{n}\sum_{m\in\mathcal{B}_{l,t}}\int_0^1 C_{j,m,l,t}^{\frac{\gamma-1}{\gamma}}dj\right)^{\frac{\gamma}{\gamma-1}} \qquad \gamma > 1.$$

While each household purchases a different random set of goods, we assume that the amount n of goods is the same for all households. The price index of household l is

$$P_{l,t} = \left(\frac{1}{n}\sum_{m\in\mathcal{B}_{l,t}}\int_0^1 P_{j,m,l,t}^{1-\gamma}dj\right)^{\frac{1}{1-\gamma}}.$$

1.3.3 Producers and monetary policy

The central bank follows a standard Taylor rule but sets the net interest rate r_t before observing prices, that is during stage one of period t:

$$r_t = \psi E_{cb,t}\pi_t + \theta_t \qquad \psi > 1,$$

where π_t is economy-wide net inflation, calculated on the basis of all goods sold in the economy. The expectation operator $E_{cb,t}$ conditions on the information set of the central

[12] As a result, households cannot extract additional information about aggregate variables from the prices of the securities.

bank, which consists of information from period $t-1$ only.[13]

θ_t is a monetary policy shock that is observable by producers and households alike. Producer j on island l produces according to the following production function

$$Y_{j,l,t} = A_{j,l,t} L_{j,l,t}^{\alpha} \qquad 0 < \alpha < 1,$$

featuring labor supplied by the local household as the sole input. $A_{j,l,t} = A_{l,t}$ denotes the productivity level of producer j, which is the same for all producers on island l. During stage two, the producer sets her optimal price for the current period based on a combination of private and public information (see below). Given prices, the level of production is determined by demand during stage three.

1.3.4 Productivity and signal

Log-productivity on each island, denoted by small-case letters, is the sum of an aggregate and an island-specific idiosyncratic component

$$a_{l,t} = x_t + \eta_{l,t},$$

where $\eta_{l,t}$ is an i.i.d. shock with variance σ_η^2 and mean zero. It aggregates to zero across all islands. The aggregate component x_t follows a random walk

$$\Delta x_t = \varepsilon_t.$$

The i.i.d. productivity shock ε_t has variance σ_ε^2 and mean zero. During stage two of each period, agents observe a public signal about x_t. This signal takes the form

$$s_t = \varepsilon_t + e_t,$$

where e_t is an i.i.d. optimism shock with variance σ_e^2 and mean zero. Producers also observe their own productivity. Hence, their expectations of Δx_t are

$$E_{j,l,t} \Delta x_t = \rho_x^p s_t + \delta_x^p (a_{j,l,t} - x_{t-1}),$$

[13] Pre-set prices and interest rates allow us to discard the noisy signals about quantities and inflation observed by producers and the central bank in Lorenzoni (2009), simplifying the signal-extraction setup without changing its qualitative predictions. Pre-set wages, on the other hand, guarantee determinacy of the price level. They do not affect output dynamics after optimism and productivity shocks, because good prices may still adjust.

with $E_{j,l,t}$ being the expectation of producer j on island l when setting prices (stage two). The coefficients ρ_x^p and δ_x^p are the same for all producers, where these and the following ρ and δ-coefficients are functions of the structural parameters which capture the informational friction. They are non-negative and smaller than unity (see Appendix 1.A). Finally, while shopping during stage three consumers observe a set of prices. Given that they have also observed the signal, they can infer the productivity level of each producer in their sample from her price. Consumers' expectations are thus given by

$$E_{l,t}\Delta x_t = \rho_x^h s_t + \delta_x^h \tilde{a}_{l,t},$$

where $\tilde{a}_{l,t}$ is the average over the realizations of $a_{m,t} - x_{t-1}$ for each island m in household l's sample. ρ_x^h and δ_x^h are equal across households and depend on $n, \sigma_e^2, \sigma_\varepsilon^2$, and σ_η^2. The model nests the case of complete information about all relevant variables for households and producers if $\sigma_e^2 = 0$. If $\sigma_e^2 > 0$, producers will set prices based on potentially overly optimistic or pessimistic expectations of productivity. Consumers also have complete information if $n \to \infty$.

1.3.5 Market clearing

Good and labor markets clear in each period:

$$\int_0^1 C_{j,m,l,t} dl = Y_{j,m,t} \;\; \forall j, m \qquad L_{l,t} = \int_0^1 L_{j,l,t} dj \;\; \forall l,$$

where $C_{j,m,l,t} = 0$ if households l does not visit island m. The asset market clears by Walras' law.

1.3.6 Results

We derive a solution of the model based on a linear approximation to the equilibrium conditions around the symmetric steady state, see Appendix 1.A for details. We obtain the following propositions, for which we provide proofs in Appendix 1.B.

Proposition 1 *A positive optimism shock ($e_t > 0$), a positive productivity shock ($\varepsilon_t > 0$), and a negative monetary policy shock ($\theta_t < 0$) raise output. Formally, we have*

$$y_t = x_{t-1} + \underbrace{\rho_x^h(1-\Omega)}_{>0} e_t + \underbrace{\left[(\delta_x^h + \rho_x^h)(1-\Omega) + \Omega\right]}_{>0} \varepsilon_t - \underbrace{\frac{\alpha}{\alpha + \psi(1-\alpha)}}_{<0} \theta,$$

with $0 < \Omega = \frac{n - \delta_x^h(1-\alpha)[(n-1)\delta_x^p + 1]}{n\alpha + (1-\alpha)\left\{(1-\delta_x^h)[1+\delta_x^p(n-1)] + (n-1)\gamma(1-\delta_x^p)\right\}} < 1.$

Proposition 2 *A positive optimism shock induces a negative nowcast error, while a positive productivity shock induces a positive nowcast error. This holds for nowcast errors of producers and households alike. Formally,*

$$y_t - E_{k,t} y_t = \underbrace{-\rho_x^k \left[\delta_x^h(1-\Omega) + \Omega\right]}_{<0} e_t + \underbrace{\left[\delta_x^h(1-\Omega) + \Omega\right](1 - \delta_x^k - \rho_x^k)}_{>0} \varepsilon_t,$$

with $E_{k,t}$ standing for either $E_{j,l,t}$ or $E_{l,t}$, and ρ^k, δ^k correspondingly for ρ^p, δ^p or ρ^h, δ^h.

Hence, productivity and optimism shocks raise actual output, but also lead to output misperceptions. Consider first the optimism shock. Producers expect aggregate productivity to be high—resulting in higher demand—but also observe that their own productivity is unchanged, which they attribute to a negative realization of the idiosyncratic productivity component. Consequently, they raise prices above what they expect the average price level to be. However, due to strategic complementarities in price stetting, the deviation from the expected average price level is subdued. Consumers, in turn, observe higher prices. They too attribute this increase to adverse productivity shocks suffered by those particular firms from which they buy. This allows households to entertain the notion of higher aggregate productivity and future income. Because the observed price increase relative to the expected long-run price level is muted, expenditure and, consequently, economic activity expand. Yet as each producer and each household considers itself unlucky relative to its peers, current output is actually lower than expected: a negative nowcast error obtains.[14]

After a productivity shock, on the other hand, producers do not fully trust the signal about the aggregate component and attribute some of the increased productivity to idiosyncratic factors. They lower prices below what they expect the average price level to

[14]As pointed out by Lorenzoni (2009), the optimism shock provides a possible microfoundation for the traditional concept of a demand shock: agents are too optimistic about economic fundamentals, resulting in unusually high demand.

be. Consumers expect higher income and raise consumption. However, both producers and their customers expect other producers to set higher prices and consequently underestimate actual output. A positive nowcast error obtains.

Furthermore, observe that monetary policy shocks have no impact on nowcast errors. More generally, any other shock that enters the information set of households and producers will not generate nowcast errors, as both are aware of the economic environment and hence the effect of shocks. Misperceptions about economic activity thus arise only after imperfectly observed shocks, such as innovations to productivity, or incorrect signals regarding productivity.

1.3.7 Identification

In addition to clarifying the nature of optimism shocks, the model allows us to address concerns about whether optimism shocks can be uncovered at all on the basis of an estimated VAR model. In this regard, the set of actual time series used in the estimation is crucial. Noting that we estimate our VAR in Section 1.4 on time series for nowcast errors, labor productivity, and hours worked, that is, on the following vector

$$\tilde{Y}_t' = \begin{bmatrix} \Delta y_t - E_{k,t}\Delta y_t & \Delta(y_t - l_t) & l_t \end{bmatrix},$$

we obtain the following proposition.

Proposition 3 *Given $\tilde{Y}_t$, the dynamics of the model can be represented by a VAR(1):*

$$\tilde{Y}_t = A\tilde{Y}_{t-1} + B\tilde{V}_t,$$

where

$$\tilde{V}_t' = \begin{bmatrix} \varepsilon_t & e_t & \theta_t \end{bmatrix},$$

and the matrices A and B are given in the proof.

Intuitively, we are able to cast the model dynamics in VAR form because we rely on variables that are not contemporaneously observed in the model. If, instead, one were to restrict the VAR to contain variables observed by agents in real time, the model would generally not be invertible. Proposition 3 is thus consistent with the result of Blanchard, L'Huillier, and Lorenzoni (2013), according to which optimism shocks cannot be recovered from actual time-series data by an econometrician who has no informational advantage over market participants. Yet as documented in Section 1.2, actual nowcast errors regarding output growth can be sizeable. To the extent that they can be measured *ex post*, they allow us to identify optimism shocks.

Finally, the model also provides us with specific identification restrictions, which we impose on the VAR model below. Given matrices A and B, we obtain the following corollary.

Corollary 1 *Monetary policy shocks have no impact on the nowcast error, neither in the short nor the long run. Furthermore, optimism shocks do not alter labor productivity in the long run.*

1.4 The effects of optimism shocks

We are now in a position to identify the effects of optimism shocks in actual time-series data and to quantify their contribution to U.S. short-run fluctuations. We do so within an estimated VAR model, combining long-run restrictions (see, for instance, Galí 1999) and short-run restrictions, which we impose on nowcast errors. Including a time series of nowcast errors in the VAR model is key to our identification strategy. It represents an informational advantage over market participants and allows us to isolate optimism and productivity shocks. In the following, we discuss our VAR specification and identification strategy before turning to the results.

1.4.1 VAR specification

Our VAR model includes three variables. Under the baseline specification we include the nowcast error computed on the basis of first-release data, the growth rate of labor productivity, and hours worked in the vector of endogenous variables.[15] Formally, as $\tilde{Y}_t$ is the vector containing these variables in the given order, the VAR model in reduced form reads as

$$\tilde{Y}_t = \sum_{i=1}^{L} A_i \tilde{Y}_{t-i} + \nu_t, \tag{1.4.1}$$

[15] Labor productivity is measured by (the first difference of the natural logarithm of) output per hour of all persons in the nonfarm business sector and hours worked is the natural logarithm of the corresponding hours. Both series are obtained from the Bureau of Economic Analysis. Results are robust, however, once we measure labor productivity as output per person and hours as employment, both obtained from the Bureau of Labor Statistics.

where L is the number of lags. ν_t is a vector of potentially mutually correlated innovations of which $\Omega = E\nu\nu'$ is the covariance matrix. We also include a constant in the VAR model.[16]

We estimate the model under our baseline specification on quarterly data covering the period 1980Q1–2012Q4. While our measure of nowcast errors is available since the late 1960s (see Section 1.2), we disregard observations prior to 1980 since the conduct of monetary policy arguably changed considerably after this time (Clarida, Galí, and Gertler 2000).[17] Below, we also report results of a sensitivity analysis exploring the robustness of our results with respect to estimating the model on the full sample.

Regarding the number of lags L, we account for concerns about a lag-truncation bias. Arguably, it is particularly severe in the case that long-run restrictions are imposed on the VAR model to achieve identification (Chari, Kehoe, and McGrattan 2008). De Graeve and Westermark (2013) perform Monte Carlo experiments and find that raising the number of lags may be a viable strategy to reduce the severity of the problem. Hence, for our baseline specification we set $L = 8$. We document below that the results are robust with respect to using a smaller number of lags.

We aim to identify structural shocks contained in the vector, ϵ_t, with $\nu_t = B\epsilon_t$ and $E\epsilon\epsilon' = I$. Given estimates for Ω and the A_i matrices, we identify B by simultaneously imposing short and long-run restrictions. Without loss of generality, we assume that ϵ_t contains from top to bottom the productivity shock, the optimism shock, and a third shock to which we do not attach any structural interpretation. Key to our identification strategy is the insight that nowcast errors can only be the result of the first two shocks—both in the short and the long run. To tell productivity and optimism shocks apart, we impose as a third restriction that optimism shocks do not impact labor productivity in the long run. All restrictions are consistent with the model developed in the previous section (see Corollary 1). Formally, our identification assumptions impose the following restrictions on the matrices B and A_0, which determine the contemporaneous and the long-run impact,

[16] Below, we additionally consider alternative trend specifications to address the potential non-stationarity of the time series for hours worked.

[17] Alternatively, one might consider a later starting date for the sample in order to account for the decline in business cycle volatility after 1983 (McConnell and Perez-Quiros 2000). We find that results are not sensitive in this respect.

respectively:

$$B = \begin{bmatrix} * & * & 0 \\ * & * & * \\ * & * & * \end{bmatrix}, \qquad A_0 \equiv \left(I - \sum_{i=1}^{L} A_i \right)^{-1} B = \begin{bmatrix} * & * & 0 \\ * & 0 & * \\ * & * & * \end{bmatrix}. \tag{1.4.2}$$

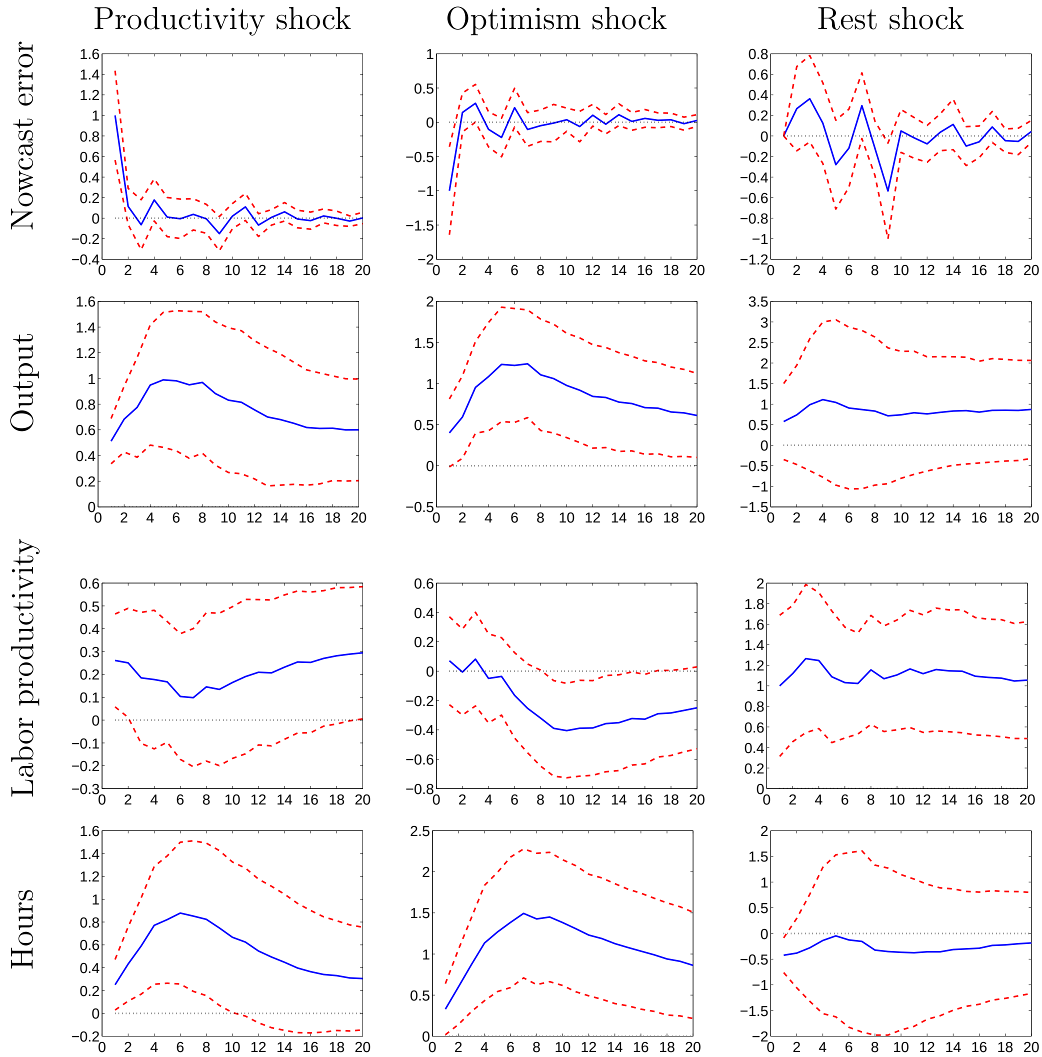
1.4.2 Results

We compute impulse response functions on the basis of the estimated model and display results in Figure 1.2. The columns (from left to right) display the responses to a productivity shock, an optimism shock, and the third shock. Solid lines represent the point estimate, while dashed lines indicate 90 percent confidence bounds obtained by boostrap sampling. The rows display the responses of the nowcast error, output (implied by those of labor productivity and hours), labor productivity, and hours respectively. In each case, horizontal axes measure time in quarters, while vertical axes measure percentage points in the case of the nowcast error and percentage deviations from steady state otherwise. To facilitate the comparison of productivity and optimism shocks, we consider in each case an expansionary shock which triggers an increase of output and normalize its size such that it induces a nowcast error equal to 1 percentage point (annualized) in absolute value.

A first noteworthy result is the joint responses of the nowcast error and output to both structural shocks. While output rises in each instant, we find that productivity shocks induce a positive response of the nowcast error and optimism shocks induce a negative response. This finding is in line with the prediction of the model developed in Section 1.3 above, even though the response of the nowcast error to both shocks has been left unrestricted. More generally, the finding that optimism shocks induce a negative co-movement of the nowcast error and output is remarkable because the unconditional co-movement of both series is positive, as established in Section 1.2. In our view, the result that the co-movement changes from unconditionally positive to negative conditional on optimism shocks lends additional support to our identification strategy.

The response of the nowcast error is short-lived, while the response of output to both shocks is quite persistent and displays hump-shaped adjustment dynamics. In fact, the short-run dynamics are fairly similar in both instances. The impact increase is approximately 0.5 percent and the peak response is reached after about 6 quarters. While the peak response is somewhat stronger in the case of an optimism shock, the response is more persistent in the case of a productivity shock, even though we still find the output response

Figure 1.2: Impulse responses to identified shocks



Notes: baseline VAR model; solid lines indicate point estimates, dashed lines 90 percent confidence bounds obtained by bootstrap sampling (1000 repetitions). Horizontal axes measure quarters. Vertical axes: percentage points in the case of the nowcast error, percent otherwise.

to an optimism shock marginally significant after 20 quarters. The third row shows the response of labor productivity. It increases in response to a productivity shock on impact, but also in the long run. In line with theory, productivity declines in response to an optimism shock, but the effect is only marginally significant. Note that optimism shocks are not allowed to impact labor productivity in the long run under our identification scheme.

The responses of hours are shown in the last row. In the short run the responses mimic that of output. It is somewhat weaker in the case of productivity shocks and somewhat stronger in the case of optimism shocks, reflecting the differential effect of these shocks on labor productivity. In the long run hours are back to the pre-shock level.

In order to contrast the transmission of optimism shocks to those of productivity shocks it is of interest to investigate their effects on variables other than those included in the baseline model. To estimate the impulse responses of these variables while economizing on the degrees of freedom, we rotate additional variables into our baseline VAR model, replacing the time series for hours worked. Figure 1.3 displays results for four additional variables of particular interest. The first two rows show the responses of consumption and investment respectively.[18] Applying the same normalization as above, we find that productivity and optimism shocks raise consumption and investment, although the effect is somewhat stronger and more persistent in the case of productivity shocks.

The third row of Figure 1.3 shows the response of core CPI inflation. We find that productivity shocks tend to be deflationary, although the response is not significant. Instead, inflation rises immediately and strongly in response to the optimism shock. Optimism shocks accordingly have the flavor of what has been traditionally referred to as a demand shock (Lorenzoni 2009).

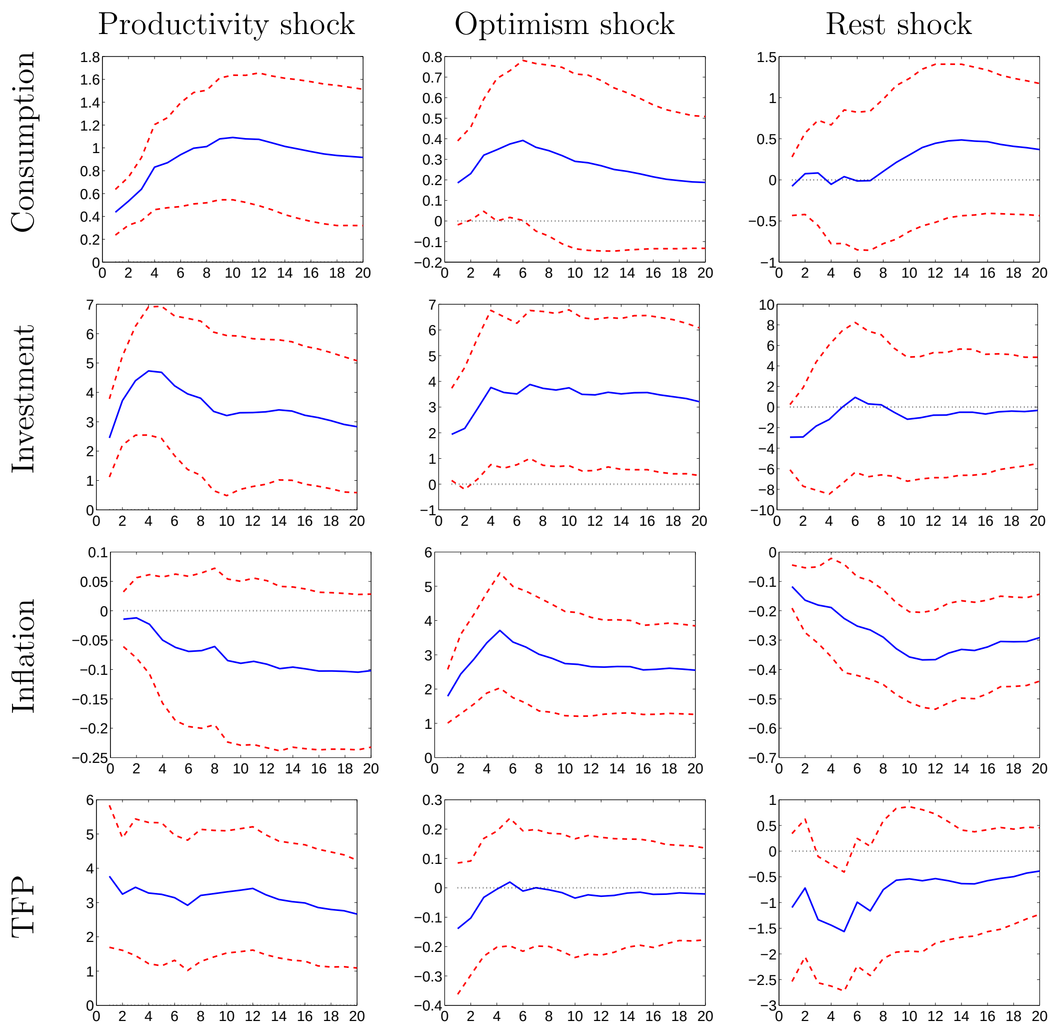
Finally, in the last row, we show the response of a direct measure of total factor productivity. Investigating its response to productivity shocks helps to assess the plausibility of our identification scheme, which relies on the absence of a long-run impact of optimism shocks on labor productivity. The time series for total factor productivity is obtained from Fernald (2012), as in Section 1.2 above.[19] Since it ends in 2009Q3, we estimate the VAR model on the longest available data series (1968Q4–2009Q3). The impulse responses show a strong increase of TFP to the productivity shock identified on the basis of long-run restrictions, but no reaction to optimism shocks—in line with the assumptions underlying our identification strategy.

Overall, we find plausible results regarding the effects of optimism shocks and thus turn to the question that motivates our analysis: namely, to what extent are optimism shocks an autonomous source of business cycle fluctuations? In order to gauge their contribution to economic fluctuations we compute a forecast error variance decomposition. Table 1.3 reports the results. We find that productivity and optimism shocks are responsible for two thirds and one third of the variation in the nowcast error respectively. This finding holds

[18]Consumption is measured by real personal consumption expenditures and investment by real gross private domestic investment, both from the Bureau of Economic Analysis.

[19]Inflation is based on the consumer price index for all urban consumers for all items less food and energy from the Bureau of Labor Statistics, and TFP is utilization-adjusted TFP in producing non-equipment output of Fernald (2012).

Figure 1.3: Impulse responses to identified shocks



Notes: each row displays the response of an additional variable replacing hours in the baseline VAR model (see Figure 1.2).

irrespective of the forecast horizon. Recall that in the short and long run nowcast errors are restricted to be driven only by these two shocks. Regarding output, productivity shocks account for the bulk of fluctuations, yet optimism shocks also contribute substantially. In the short run their contribution rises from 17 percent to almost one third after about three years, declining thereafter.

These findings are in a similar order of magnitude than those reported by Blanchard, L'Huillier, and Lorenzoni (2013). They estimate a medium-scale DSGE model featuring "noise shocks". These shocks are structurally identical to optimism shocks as defined in the

Table 1.3: Forecast error variance decomposition

		Productivity	Optimism	Rest
Nowcast Error	1	69.83	30.16	0.00
	4	66.57	30.38	3.05
	12	61.27	29.94	8.79
	40	60.78	30.02	9.20
Output	1	64.77	17.07	18.16
	4	55.29	27.82	16.89
	12	55.09	32.77	12.14
	40	53.67	25.79	20.54
Labor Productivity	1	23.41	0.75	75.84
	4	14.06	0.44	85.50
	12	9.63	8.77	81.61
	40	20.78	6.50	72.72
Hours	1	42.10	31.29	26.61
	4	50.29	45.76	3.94
	12	42.31	55.90	1.79
	40	35.26	62.88	1.85

Notes: Results are presented for the baseline VAR model. Each panel reports the decomposition of the forecast error variance for the variable of interest, considering a forecast horizon of 1, 4, 12 and 40 quarters. The contribution of the three shocks is reported in the appropriately labeled columns.

present paper and account for about 20 percent of output volatility.[20] Instead, Barsky and Sims (2012), estimating a fully specified DSGE model through indirect inference methods, find that "animal spirit" shocks account for almost none of the volatility of output. While their animal spirit shock is conceptually closely related to optimism shocks, it is restricted to pertain to future productivity (growth) only. Moreover, their analysis is centered around innovations to consumer confidence, which they find to reflect news rather than animal spirits. Once we rotate their time series of confidence innovations as a third variable into our VAR model, we find it to be mostly driven by the "rest" shock.[21] This finding is

[20]In a similar exercise, Hürtgen (2013) obtains a value of 14 percent.

[21]Specifically, we consider the series for confidence innovations of Barsky and Sims (2012), which is

consistent with the results of Barsky and Sims (2012) insofar as the rest shock will pick up anticipated productivity shocks under our identification scheme.

1.4.3 Sensitivity analysis

We conduct a number of experiments to explore the robustness of our results. First, we consider alternative measures of the nowcast error, which is key to our identification strategy. Our baseline VAR is estimated on nowcast errors computed on the basis of first-release data. Results in Section 1.2 suggest that nowcast errors based on final-release data may differ somewhat. We therefore estimate our VAR model while replacing the first-release nowcast error with the final-release nowcast error. The estimated impulse responses to productivity and optimism shocks obtained under this specification are shown in the left panel of Figure 1.4, confirming our findings for the baseline VAR model reported in Figure 1.2.

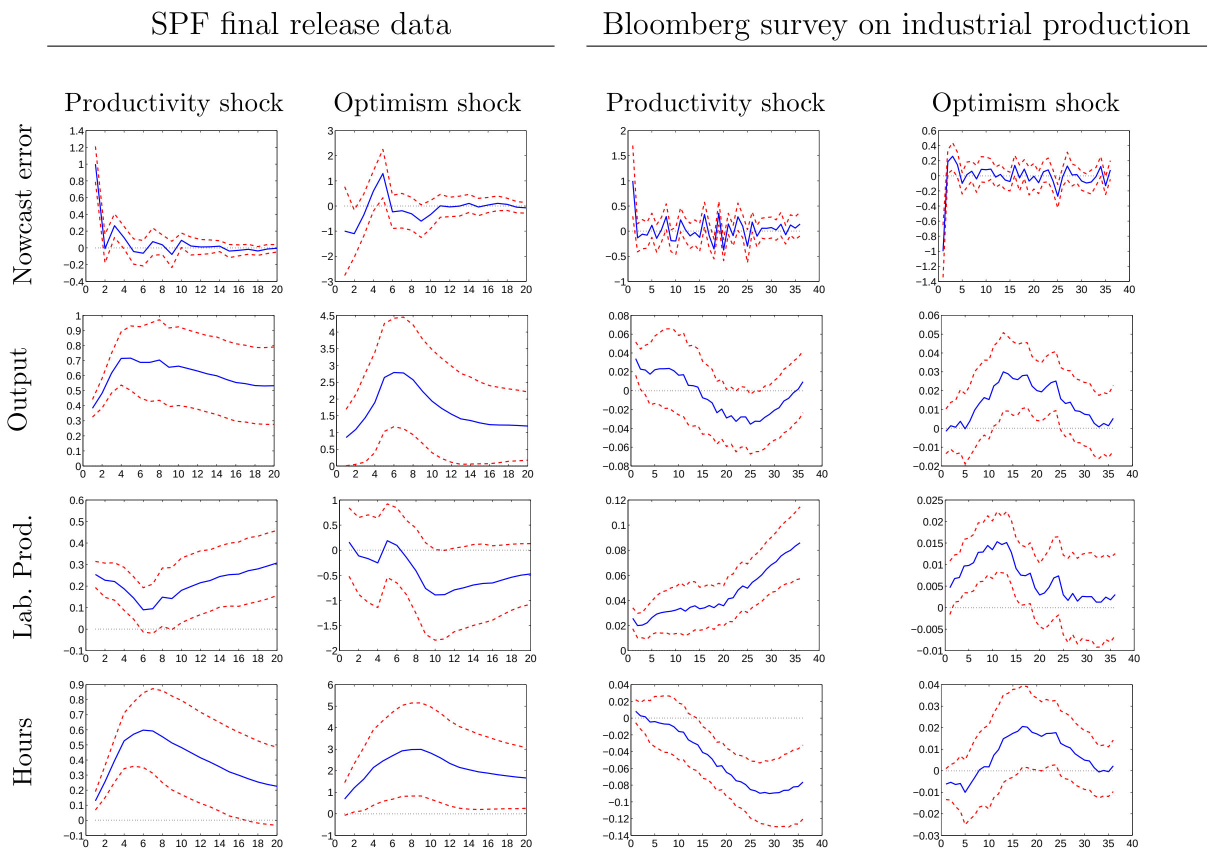
In what follows we explore to what extent results are robust once we consider a different sampling frequency, as our identification strategy relies on assumptions regarding the available information at the time forecasters are asked to predict current output growth. Specifically, forecasters are assumed to have no information regarding current innovations to productivity. Due to the frequency of releases of GDP data, our baseline VAR model is estimated on quarterly observations. In order to construct an alternative monthly measure of the nowcast error, we use data for industrial production and an appropriate survey of professional forecasters by Bloomberg.[22] Results are shown in the right panel of Figure 1.4. From a qualitative point of view, they are in line with those obtained for the baseline VAR model, despite considerable differences in the sample (1996M10–2012M12), data frequency, and the measure of economic activity.

Next, we are turning to alternative assumptions regarding our sample and the number

based on the Michigan Survey of Consumers. Confidence innovations are computed on the basis of their VAR model and their orthogonalization with confidence ordered first. We include confidence innovations in our baseline VAR model (using the longest overlapping sample), replacing the time series for hours worked. Computing a forecast error variance decomposition, we find that about two thirds of the short-run variance of confidence innovations is due to the rest shock, while the optimism shock accounts for less than 5 percent. Moreover, we find that only the rest shock has a significant impact on confidence innovations. It is positive and short-lived.

[22] The Bloomberg survey forecasts are available since 1996M10. We consider data up to 2012M12. The series on monthly hours is the index of aggregate weekly hours of production for workers in manufacturing from the Bureau of Labor Statistics, while the corresponding growth rate of labor productivity is the difference in the growth rates of the volume index of industrial production, obtained from the Federal Reserve, and hours. We estimate the VAR with 24 lags, that is, we include two years as in the baseline model estimated on quarterly data.

Figure 1.4: Impulse responses to productivity and optimism shock: Sensitivity analysis I

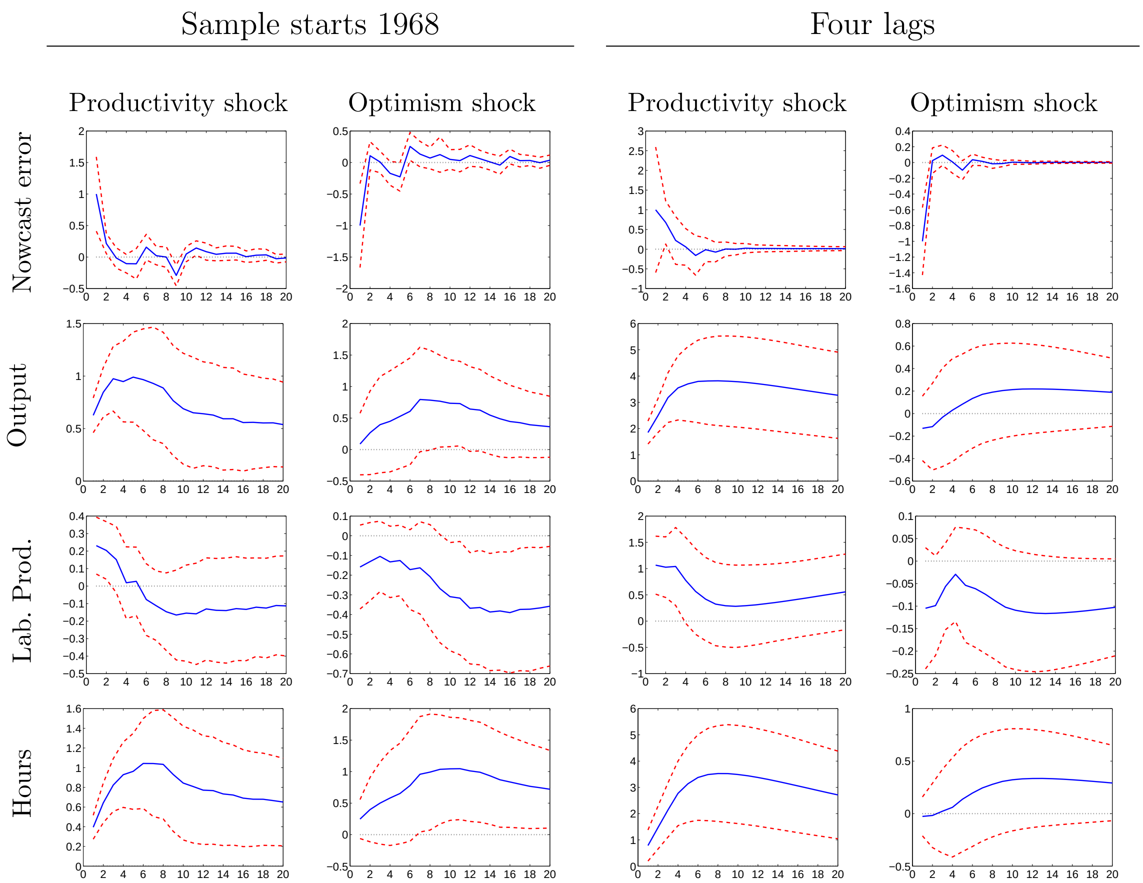


Notes: left panel shows results for nowcast error based on final-release data; right panel shows results for nowcast error based on monthly data for industrial production (sample: 1996M10–2012M12); horizontal axis measures months.

of lags included in the VAR model. Results are shown in Figure 1.5. The left panel shows results for the longest possible sample given data availability: 1968Q4–2012Q4. They are very similar to those of the baseline specification (see Figure 1.2). An exception is the response of labor productivity to the productivity shock which turns insignificantly negative after about six quarters. However, in the long run (not shown), the response is positive as in the baseline VAR. The right panel of Figure 1.5 shows results for the model estimated on four lags only. Again, results are fairly similar to those obtained under the baseline specification.

Finally, we explore the robustness of our results with respect to alternative assumptions regarding potential trends in the time series for hours worked. This issue has received considerable attention in the literature, as some studies found the trend specification to be crucial for the sign of the response of hours worked to a productivity shock. This

Figure 1.5: Impulse responses to productivity and optimism shock: Sensitivity analysis II



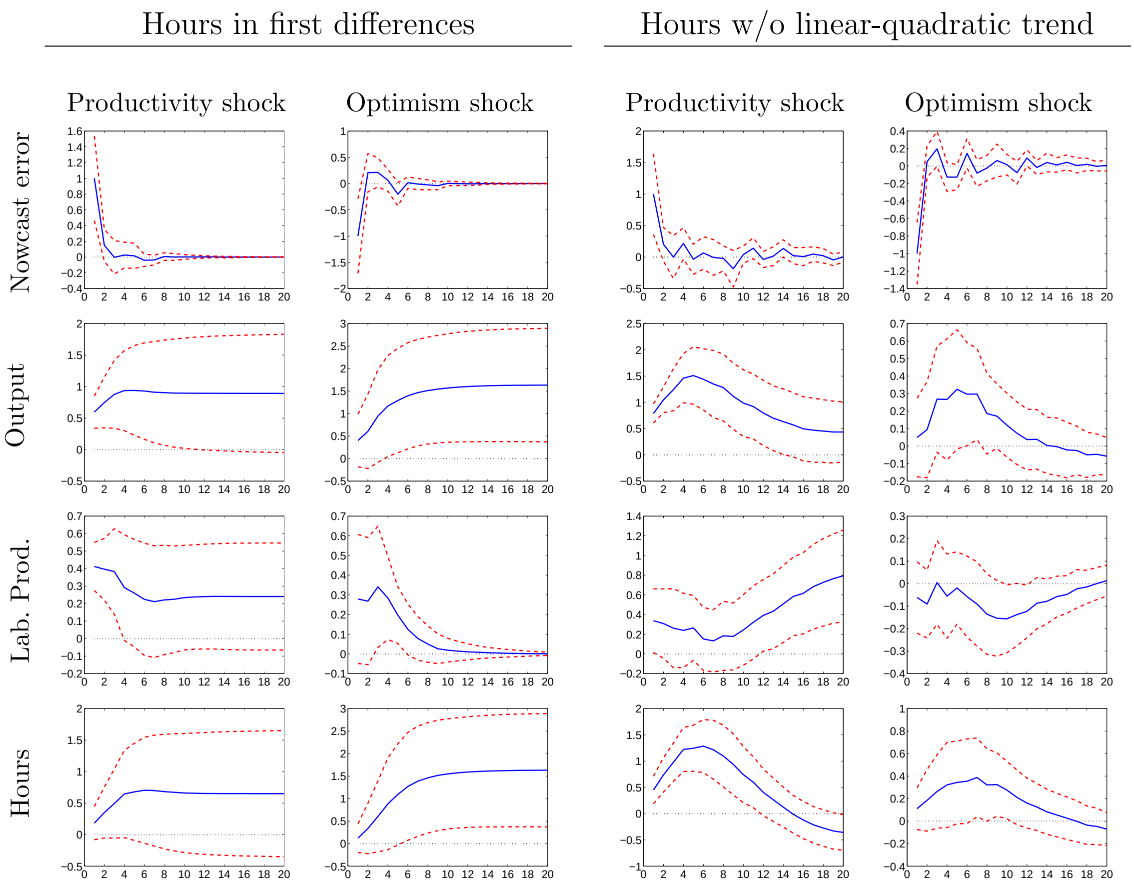
Notes: left panel shows results for baseline VAR model estimated on sample starting in 1968Q4, see Figure 1.2; right panel shows results for VAR model with four lags.

is not the case in our setup. Recall that we do not allow for a trend in hours in our baseline specification. Figure 1.6 shows results for a specification where hours enter in first differences (left panel) and for a specification where a linear-quadratic trend has been removed from hours worked prior to estimation (right panel).[23]

Our results may therefore also shed some light on the so-called “hours puzzle” (see Galí 1999, Francis and Ramey 2005, and Chari, Kehoe, and McGrattan 2008, among others). Given that hours unambiguously rise after (unexpected) productivity shocks under our identification scheme, a decline in hours documented elsewhere is likely due to productivity innovations which have no effect on nowcast errors. This, in turn, may be the result of innovations to productivity that have been anticipated.

[23]Hours entering the VAR model either in levels, first differences or detrended with a linear-quadratic trend are commonly considered to be the most plausible specifications, see Galí and Rabanal (2005). Our results are also robust to detrending hours with a one-sided HP-filter.

Figure 1.6: Impulse responses to productivity and optimism shock: Sensitivity analysis III



Notes: left panel shows results for hours in first differences, right panel for hours after a linear quadratic trend has been removed.

1.5 Conclusion

To what extent are changes of expectations an autonomous source of business cycle fluctuations? In this paper, we pursue a new approach to address this question. Barsky and Sims (2012) and Blanchard, L'Huillier, and Lorenzoni (2013) estimate fully-specified DSGE models to quantify the importance of "noise" or "undue optimism", reaching quite different conclusions. We employ a structural VAR model instead, thereby imposing less structure on the data. Yet, as shown by Blanchard, L'Huillier, and Lorenzoni (2013), identifying the effects of optimism shocks within VARs constitutes a formidable challenge.

Our empirical strategy is based on an *ex-post* informational advantage over market participants. Namely, we compute nowcast errors regarding current output growth as the difference between actual output growth and the median forecast in the Survey of Professional Forecasters. Nowcast errors are a reduced-form measure of misperceptions, which we show to respond systematically to innovations in total factor productivity. However, we find them not to be significantly affected by policy innovations or uncertainty shocks which are, in some sense, observable.

Drawing on Lorenzoni (2009), we put forward a stylized business cycle model which gives rise to nowcast errors due to productivity and optimism shocks, as agents do not observe output contemporaneously. Shocks which are common information do not generate a nowcast error. Importantly, we use this model to show that optimism shocks can be recovered from time-series data on nowcast errors.

Given these results, we estimate a VAR model on U.S. time series including the nowcast error, labor productivity, and hours worked for the period 1980Q1–2012Q4. We identify unanticipated shocks to total factor productivity and optimism shocks by combining short and long-run restrictions. Specifically, we assume that optimism shocks and productivity shocks can trigger nowcast errors, but that optimism shocks do not affect labor productivity in the long run. We find that both shocks have a sizable and persistent effect on output, yet their effect on the nowcast error differs fundamentally. We find that productivity shocks induce a positive nowcast error, that is, growth is higher than expected. Optimism shocks, on the other hand, induce a negative nowcast error, that is, growth is lower than expected. While this result is quite intuitive, it is remarkable because it implies that the correlation of nowcast errors and economic activity conditional on optimism shocks changes sign relative to the unconditional correlation.

According to the forecast error variance decomposition, the contribution of optimism shocks rises to some 30 percent of output fluctuations at a 3-year horizon and declines thereafter. In the very short run our result is close to 20 percent, the value reported by Blanchard, L'Huillier, and Lorenzoni (2013) for a 1-year horizon. Differences relative to Barsky and Sims (2012) are likely to reflect differences in the informational content of the nowcast error of current output growth on the one hand and of consumer sentiment data on the other.

Acknowledgments

I am indebted to Zeno Enders and Gernot Müller, who are co-authors of Chapter 1.

Appendix

In Appendix 1.B, we provide the proofs for Propositions 3.1-3 in Section 1.3. In a preliminary step, we outline the model solution and key equilibrium relationships in Appendix 1.A. Throughout, we consider a log-linear approximation to the equilibrium conditions of the model. Small-scale letters indicate percentage deviations from steady state.

1.A Model solution

We solve the model by backward induction. That is, we start by deriving inflation expectations regarding period $t+1$. Using the result in the Euler equation of the third stage of period t allows us to determine price-setting decision during stage two. Eventually, we obtain the short-run responses of aggregate variables to unexpected changes in productivity or optimism shocks.

Expectations regarding period $t+1$. Below, $E_{k,t}$ stands for either $E_{j,l,t}$, referring to the information set of producer j on island l at the time of her pricing decisions, or for $E_{l,t}$, referring to the information set of the household on island l at the time of its consumption decision. Variables with only time subscripts refer to economy-wide values. The wage in period $t+1$ is set according to the expected aggregate labor supply

$$E_{k,t}\varphi l_{t+1} = E_{k,t}(w_{t+1} - p_{t+1} - c_{t+1}).$$

This equation is combined with the aggregated production function

$$E_{k,t}y_{t+1} = E_{k,t}(x_{t+1} + \alpha l_{t+1}),$$

the expected aggregate labor demand

$$E_{k,t}(w_{t+1} - p_{t+1}) = E_{k,t}[x_{t+1} + (1-\alpha)l_{t+1}],$$

and market clearing $y_{t+1} = c_{t+1}$ to obtain $E_{k,t}x_{t+1} = E_{k,t}y_{t+1} = E_{k,t}c_{t+1}$. Furthermore, the expected Euler equation, together with the Taylor rule, is

$$E_{k,t}c_{t+1} = E_{k,t}(c_{t+2} + \pi_{t+2} - \psi\pi_{t+1}).$$

Agents expect the economy to be in a new steady state tomorrow ($E_{k,t}c_{t+1} = E_{k,t}c_{t+2}$) given the absence of state variables other than technology, which follows a unit root process. Ruling out explosive paths yields

$$E_{k,t}\pi_{t+2} = E_{k,t}\pi_{t+1} = 0.$$

Stage three of period *t*. After prices are set, each household observes n prices in the economy. Since the productivity signal is public, the productivity level $a_{j,l,t} = a_{l,t}$—which is the same for all producers $j \in [0,1]$ on island l—can be inferred from each price $p_{j,l,t}$ of the good from producer j on island l. Hence, household l forms its expectations about the change in aggregate productivity according to

$$E_{l,t}\Delta x_t = \rho_x^h s_t + \delta_x^h \hat{a}_{l,t},$$

where $\hat{a}_{l,t}$ is the average over the realizations of $a_{m,t} - x_{t-1}$ for each location m in household l's sample. The coefficients ρ_x^h and δ_x^h are equal across households and depend on $n, \sigma_e^2, \sigma_\varepsilon^2$, and σ_η^2 in the following way:

$$\rho_x^h = \underbrace{\frac{\sigma_\eta^2/n}{\sigma_e^2 + \sigma_\eta^2/n + \frac{\sigma_e^2\sigma_\eta^2/n}{\sigma_\varepsilon^2}}}_{\to 0 \text{ if } n\to\infty}, \qquad \delta_x^h = \underbrace{\frac{\sigma_e^2}{\sigma_e^2 + \sigma_\eta^2/n + \frac{\sigma_e^2\sigma_\eta^2/n}{\sigma_\varepsilon^2}}}_{\to 1 \text{ if } n\to\infty}. \tag{1.A.1}$$

Producers, on the other hand, only observe the signal and their own productivity. They thus form expectations according to

$$E_{j,l,t}\Delta x_t = \rho_x^p s_t + \delta_x^p (a_{l,t} - x_{t-1}),$$

with

$$\rho_x^p = \frac{\sigma_\eta^2}{\sigma_e^2 + \sigma_\eta^2 + \frac{\sigma_\eta^2\sigma_e^2}{\sigma_\varepsilon^2}} \qquad \delta_x^p = \frac{\sigma_e^2}{\sigma_e^2 + \sigma_\eta^2 + \frac{\sigma_\eta^2\sigma_e^2}{\sigma_\varepsilon^2}},$$

such that $\delta_x^h > \delta_x^p$ because of the higher information content of households' observations. Consumption follows an Euler equation with household-specific inflation, as only a subset of goods is bought. Agents expect no differences between households for $t+1$, such that expected aggregate productivity and the overall price level impact today's consumption.

Using additionally $E_{l,t}p_{t+1} = E_{l,t}p_t$ and $E_{l,t}x_{t+1} = E_{l,t}x_t$ gives

$$c_{l,t} = E_{l,t}x_t + E_{l,t}p_t - p_{l,t} - r_t. \tag{1.A.2}$$

Similar to the updating formula for technology, households use their available information to form an estimate about the aggregate price level p_t according to

$$E_{l,t}p_t = \rho_p^h s_t + \delta_p^h \hat{a}_{l,t} + \kappa_p^h w_t + \tau_p^h x_{t-1} - \eta_p^h r_t. \tag{1.A.3}$$

Combining the above this gives

$$c_{l,t} = (1 + \tau_p^h)x_{t-1} + \rho_{xp}^h s_t + \delta_{xp}^h \hat{a}_{l,t} + \kappa_p^h w_t - (1 + \eta_p^h)r_t - p_{l,t}, \tag{1.A.4}$$

where $\rho_x^h = \rho_x^h + \rho_p^h$ and $\delta_{xp}^h = \delta_x^h + \delta_p^h$. We will solve for the undetermined coefficients below.

Stage two of period t. During the second stage, firms obtain idiosyncratic signals about their productivity. Below, the index $\tilde{p}_{l,t}$ is the average price index of customers visiting island l. If customers bought on all (that is, infinitely many) islands in the economy, it would correspond to the overall price level. Since consumers only buy on a subset of islands, the price of their own island has a non-zero weight in their price index, which is taken into account further below. Firms set prices according to

$$\begin{aligned} p_{j,l,t} &= w_t + \frac{1-\alpha}{\alpha} E_{j,l,t}y_{j,l,t} - \frac{1}{\alpha} a_{l,t} \\ &\equiv k' + k_1' E_{j,l,t}\tilde{p}_{l,t} + k_2' E_{j,l,t}y_t - k_3' a_{l,t}, \end{aligned}$$

with

$$k' = \frac{\alpha}{\alpha + \gamma(1-\alpha)} w_t \qquad k_1' = \frac{\gamma(1-\alpha)}{\alpha + \gamma(1-\alpha)} \qquad k_2' = \frac{1-\alpha}{\alpha + \gamma(1-\alpha)} \qquad k_3' = \frac{1}{\alpha + \gamma(1-\alpha)}. \tag{1.A.5}$$

Evaluating the expectation of firm j about aggregate output in period t, using equation (1.A.4), results in

$$E_{j,l,t}y_t = \kappa^h + \rho_{xp}^h s_t + \delta_{xp}^h E_{j,l,t}\left(\frac{1}{n}a_{l,t} + \frac{n-1}{n}E_{j,l,t}x_t - x_{t-1}\right) - \left(\frac{1}{n}p_{j,l,t} + \frac{n-1}{n}E_{j,l,t}p_t\right),$$

where $\kappa^h = (1+\tau_p^h)x_{t-1} - (1+\eta_p^h)r_t + \kappa_p^h w_t$ is publicly available information. Furthermore, it is taken into account that productivity of island l has a non-zero weight in the sample of productivity levels observed by consumers visiting island l. Note that producers still take the price index of the consumers as given, since they buy infinitely many goods on the same island. Inserting this in the above pricing equation yields (here, p_t is the average of the prices charged by producers of all other islands, which is the overall price index as there are infinitely many locations)

$$p_{j,l,t} \equiv k + k_1 E_{j,l,t} p_t + \tilde{k} s_t - k_3 a_{l,t},$$

with

$$\Xi = 1 - \frac{1}{n}(k_1' - k_2') \qquad k = \frac{1}{\Xi}\left\{k' + k_2'\kappa^h + \frac{k_2'\delta_{xp}^h}{n}\left[(n-1)(1-\delta_x^p) - 1\right] x_{t-1}\right\} \tag{1.A.6}$$

$$k_1 = \frac{n-1}{n\Xi}\left(k_1' - k_2'\right) \qquad \tilde{k} = \frac{k_2'}{\Xi}\left(\rho_{xp}^h + \delta_{xp}^h \rho_x^p \frac{n-1}{n}\right) \qquad k_3 = \frac{1}{\Xi}\left\{k_3' + \frac{k_2'\delta_{xp}^h}{n}\left[(n-1)\delta_x^p - 1\right]\right\}.$$

Aggregating over all producers gives the aggregate price index

$$p_t = k + k_1 \overline{E}_t p_t + \tilde{k} s_t - k_3 x_t,$$

where $\int a_{l,t} dl = x_t$ and $\overline{E}_t p_t = \iint E_{j,l,t} p_t \; dj dl$ is the average expectation of the price level. The expectation of firm j of this aggregate is therefore

$$\begin{aligned} E_{j,l,t} p_t &= k + \tilde{k} s_t - k_3 E_{j,l,t} x_t + k_1 E_{j,l,t} \overline{E}_t p_t \\ &= k + \left(\tilde{k} - k_3 \rho_x^p\right) s_t - k_3 \delta_x^p a_{l,t} - k_3(1-\delta_x^p) x_{t-1} + k_1 E_{j,l,t} \overline{E}_t p_t. \end{aligned} \tag{1.A.7}$$

Inserting the last equation into (1.A.6) gives

$$p_{j,l,t} = k + k_1 k - k_1 k_3 (1-\delta_x^p) x_{t-1} + \left[\tilde{k} + k_1\left(\tilde{k} - k_3 \delta_x^p\right)\right] s_t - \left(k_3 + k_1 k_3 \delta_x^p\right) a_t^j + k_1^2 E_{j,l,t} \overline{E}_t p_t.$$

To find $E_{j,l,t}\overline{E}_t p_t$, note that firm j's expectations of the average of (1.A.7) are

$$E_{j,l,t}\overline{E}_t p_t = k - k_3(1-\delta_x^p)(1+\delta_x^p) x_{t-1} + \left(\tilde{k} - k_3\rho_x^p - k_3\delta_x^p\rho_x^p\right) s_t - k_3 \delta_x^{p\,2} a_{l,t} + k_1 E_{j,l,t} \overline{E}_t^{(2)} p_t,$$

where $\overline{E}^{(2)}$ is the average expectation of the average expectation. The price of firm j is found by plugging the last equation into the second-to-last:

$$\begin{aligned}p_{j,l,t} = &\left(k + k_1 k + k_1^2 k\right) - \left[k_1 k_3 (1-\delta_x^p) + k_1^2 k_3 (1-\delta_x^p)(1+\delta_x^p)\right] x_{t-1} \\ &+ \left[\tilde{k} + k_1\left(\tilde{k} - k_3\rho_x^p\right) + k_1^2\left(\tilde{k} - k_3\rho_x^p - k_3\delta_x^p\rho_x^p\right)\right] s_t \\ &- \left(k_3 + k_1 k_3 \delta_x^p + k_1^2 k_3 \delta_x^{p2}\right) a_{l,t} + k_1^3 E_{j,l,t}\overline{E}^{(2)} p_t.\end{aligned}$$

Continuing like this results in some infinite sums

$$\begin{aligned}p_{j,l,t} =& k\left(1 + k_1 + k_1^2 + k_1^3 \dots\right) \\ &- k_1 k_3 (1-\delta_x^p)\Big[1 + k_1(1+\delta_x^p) + k_1^2(1+\delta_x^p+\delta_x^{p2}) + k_1^3(1+\delta_x^p+\delta_x^{p2}+\delta_x^{p3}\dots)\Big] x_{t-1} \\ &+ \left[\tilde{k} + k_1\left(\tilde{k} - k_3\rho_x^p\right) + k_1^2\left(\tilde{k} - k_3\rho_x^p - k_3\delta_x^p\rho_x^p\right) + k_1^3\left(\tilde{k} - k_3\rho_x^p - k_3\rho_x^p\delta_x^p - k_3\rho_x^p\delta_x^{p2}\right) + \dots\right] s_t \\ &- k_3\left(1 + k_1\delta_x^p + k_1^2\delta_x^{p2} + k_1^3\delta_x^{p3}\dots\right) a_{l,t} + k_1^\infty E_{j,l,t}\overline{E}^{(\infty)} p_t.\end{aligned}$$

For the terms in the third line (see below for the proof that $|k_1| < 1$) we have

$$\begin{aligned}&\tilde{k} + k_1\left(\tilde{k} - k_3\rho_x^p\right) + k_1^2\left(\tilde{k} - k_3\rho_x^p - k_3\delta_x^p\rho_x^p\right) + k_1^3\left(\tilde{k} - k_3\rho_x^p - k_3\rho_x^p\delta_x^p - k_3\rho_x^p\delta_x^{p2}\right) \\ &\quad + k_1^4\left(\tilde{k} - k_3\rho_x^p - k_3\rho_x^p\delta_x^p - k_3\rho_x^p\delta_x^{p2} - k_3\rho_x^p\delta_x^{p3}\right)\dots \\ =&\tilde{k}(1 + k_1 + k_1^2 + k_1^3\dots) - \left(k_1 k_3\rho_x^p + k_1^2 k_3\rho_x^p + k_1^3 k_3\rho_x^p\dots\right) \\ &\quad - \left(\delta_x^p k_1^2 k_3\rho_x^p + \delta_x^p k_1^3 k_3\rho_x^p + \delta_x^p k_1^4 k_3\rho_x^p\dots\right) - \left(\delta_x^{p2} k_1^3 k_3\rho_x^p + \delta_x^{p2} k_1^4 k_3\rho_x^p + \delta_x^{p3} k_1^5 k_3\rho_x^p\dots\right)\dots \\ =&\tilde{k}(1 + k_1 + k_1^2 + k_1^3\dots) - k_1 k_3\left(\frac{\rho_x^p}{1-k_1} + \frac{\rho_x^p\delta_x^p k_1}{1-k_1} + \frac{\rho_x^p\delta_x^{p2}k_1^2}{1-k_1}\dots\right) \\ =&\frac{\tilde{k}}{1-k_1} - \frac{k_1 k_3\rho_x^p}{1-k_1}\left(1 + \delta_x^p k_1 + \delta_x^{p2} k_1^2\dots\right) \\ =&\frac{\tilde{k}}{1-k_1} - \frac{k_1 k_3 \rho_x^p}{(1-k_1)(1-\delta_x^p k_1)}.\end{aligned}$$

Proceeding similarly with the terms in the second line results in

$$p_{j,l,t} = \frac{k}{1-k_1} - \frac{k_1(1-\delta_x^p)}{1-k_1}\frac{k_3}{1-k_1\delta_x^p}x_{t-1} + \frac{1}{1-k_1}\left(\tilde{k} - \rho_x^p\frac{k_1 k_3}{1-k_1\delta_x^p}\right)s_t - \frac{k_3}{1-k_1\delta_x^p}a_{l,t} + \underbrace{k_1^\infty\overline{E}_t^{(\infty)}}_{\to 0}p_t.$$

Setting idiosyncratic technology shocks equal to zero in order to track the effects of aggregate shocks and observing that all firms then set the same price gives

$$p_t \equiv \bar{k}_1 + \bar{k}_2 s_t + \bar{k}_3 x_t,$$

with

$$\bar{k}_1 = \frac{1}{1-k_1}\left[k - (1-\delta_x^p)\frac{k_1 k_3}{1-k_1\delta_x^p}x_{t-1}\right] \quad \bar{k}_2 = \frac{1}{1-k_1}\left(\tilde{k} - \rho_x^p \frac{k_1 k_3}{1-k_1\delta_x^p}\right) \quad \bar{k}_3 = -\frac{k_3}{1-k_1\delta_x^p}. \tag{1.A.8}$$

To arrive at qualitative predictions for output growth and the nowcast error after the structural shocks ε_t and e_t, we need to determine the sign and the size of $\bar{k}_3$. Note that according to (1.A.5), $0 < k_1' - k_2' < 1$ because $0 < \alpha < 1$ and $\gamma > 1$. According to the definition of k_1 in (1.A.6), this implies (observe that $n > 1$)

$$0 < k_1 < 1.$$

Turning to k_3, note that, according to (1.A.6)

$$-k_3 = \delta_{xp}^h \frac{k_2' - nk_3'/\delta_{xp}^h + k_2'(n-1)\delta_x^p}{n - (k_1' - k_2')}.$$

The first nominator in the bracket is, observing (1.A.5),

$$k_2' - nk_3'/\delta_{xp}^h = \frac{1 - n/\delta_{xp}^h - \alpha}{\alpha + \gamma(1-\alpha)}.$$

Using (1.A.5) and (1.A.6) yields

$$-k_3 = \delta_{xp}^h \frac{(1-\alpha)[(n-1)\delta_x^p + 1] - n/\delta_{xp}^h}{(n-1)[\alpha + \gamma(1-\alpha)] + 1}.$$

Plugging this into the definition of $\overline{k}_3$ in (1.A.8) gives

$$\overline{k}_3 = \delta_{xp}^h \frac{\frac{(1-\alpha)[(n-1)\delta_x^p + 1] - n/\delta_{xp}^h}{(n-1)[\alpha+\gamma(1-\alpha)]+1}}{1 - \delta_x^p \frac{(n-1)(\gamma-1)(1-\alpha)}{(n-1)[\alpha+\gamma(1-\alpha)]+1}}.$$

To obtain $\delta^h_{xp} = \delta^h_x + \delta^h_p$, we need to find the undetermined coefficients of equation (1.A.3). Start by comparing this equation with household l's expectation of equation (1.A.8):

$$E_{l,t}p_t = \underbrace{\overline{k}_1 + \overline{k}_3 x_{t-1}}_{\kappa^h_p w_t + \tau^h_p x_{t-1} - \eta^h_p r_t} + \underbrace{\left(\overline{k}_2 + \overline{k}_3 \rho^h_x\right)}_{\rho^h_p} s_t + \underbrace{\overline{k}_3 \delta^h_x}_{\delta^h_p} \hat{a}_{l,t}. \tag{1.A.9}$$

Hence, $\delta^h_{xp} = \delta^h_x(1+\overline{k}_3)$. Inserting this into the above expression for $\overline{k}_3$ yields

$$\overline{k}_3 \equiv -\frac{n/\Upsilon - \delta^h_x \Psi}{\Phi - \delta^h_x \Psi}, \tag{1.A.10}$$

with

$$\Upsilon = (n-1)[\alpha + \gamma(1-\alpha)] + 1 > 0 \qquad \Psi = (1-\alpha)[(n-1)\delta^p_x + 1]/\Upsilon > 0$$
$$\Phi = 1 - \delta^p_x (n-1)(\gamma-1)(1-\alpha)/\Upsilon.$$

The signs obtain because $n > 1, 0 < \alpha < 1, \delta^p_x > 0$, and $\gamma > 1$. Observe that $\Psi\Upsilon < n$ because $\delta^p_x \leq 1$. Hence,

$$n/\Upsilon - \delta^h_x \Psi > 0$$
$$n - \underbrace{\delta^h_x}_{>0,<1} \underbrace{\Psi\Upsilon}_{<n} > 0,$$

implying that the nominator of (1.A.10) is positive. Turning to the denominator $\Phi - \delta^h_x \Psi$, observe that $\Phi - \Psi > 0$. Hence, the denominator of (1.A.10) is positive as well, and we have $\overline{k}_3 < 0$. Next, consider that $n/\Upsilon < \Phi$ and we obtain

$$-1 < \overline{k}_3 < 0.$$

This is a key result for the derivation of Propositions 3.1-3, see Appendix 1.B. Multiplying the nominator and the denominator of the fraction in equation (1.A.10) by Υ and rewriting gives the expression used in Proposition 3.1.

Stage one of period t As information sets of agents are perfectly aligned during stage one, we use the expectation operator E_t to denote stage-one expectations in what follows. Combining the results regarding expectations about inflation in period $t+1$ with the Euler equation, the Taylor rule, and the random walk assumption for x_t gives

$$E_t y_t = E_t x_t - \psi E_t \pi_t.$$

Remember that the monetary policy shock realizes after wages are set. Its expected value before wage-setting is zero. Using $E_t x_t = E_t y_t$ (which results from combining labor supply and demand with the production function), we obtain

$$E_t \pi_t = 0,$$

used in the derivations above. Nominal wages are set in line with these expectations. We thus have determinacy of the price level. The central bank, setting the interest rate after wages are determined, also expects zero inflation in the absence of monetary policy shocks. To find the effects of monetary policy shocks on the interest rate, including feedback effects via changes in expected inflation, note that according to equation (1.A.9)

$$\overline{k}_1 + \overline{k}_3 x_{t-1} = \kappa_p^h w_t + \tau_p^h x_{t-1} - \eta_p^h r_t,$$

where, observing equations (1.A.5), (1.A.6), and (1.A.8),

$$\begin{aligned}\overline{k}_1 =& \frac{1}{(1-k_1)\Xi}\left[\frac{\alpha}{\alpha+\gamma(1-\alpha)} + k_2'\kappa_p^h\right] w_t - \frac{k_2'(1+\eta_p^h)}{(1-k_1)\Xi} r_t \\ &+ \frac{1}{(1-k_1)\Xi}\left\{k_2'(1+\tau_p^h) + k_2'\delta_{xp}^h\left[\frac{n-1}{n}(1-\delta_x^p) - 1\right] - \frac{(1-\delta_x^p)k_1 k_3 \Xi}{1-k_1\delta_x^p}\right\} x_{t-1}.\end{aligned}$$

Hence,

$$-\eta_p^h = \frac{k_2'(1+\eta_p^h)}{(1-k_1)\Xi} = \frac{\alpha-1}{\alpha},$$

which is the impact of r_t on the price level. To finally determine the response of r_t, use this insight in the Taylor rule, resulting in

$$r_t = \psi\frac{\alpha-1}{\alpha} r_t + \theta_t = \frac{\alpha}{\alpha+\psi(1-\alpha)}\theta_t. \tag{1.A.11}$$

1.B Proofs

Proof of Proposition 3.1 Aggregating individual Euler equations (1.A.2) over all individuals, using (1.A.8), (1.A.9), and (1.A.11), gives

$$\begin{aligned} y_t =& E_{l,t}x_t + E_{l,t}p_t - p_t - r_t \\ =& x_{t-1} + \rho_x^h(1+\overline{k}_3)s_t + \left[\delta_x^h + \overline{k}_3(\delta_x^h - 1)\right]\varepsilon_t - \frac{\alpha}{\alpha+\psi(1-\alpha)}\theta_t \\ =& x_{t-1} + \underbrace{\rho_x^h(1+\overline{k}_3)}_{>0} e_t + \underbrace{\left[\delta_x^h + \rho_x^h - \overline{k}_3(1-\delta_x^h-\rho_x^h)\right]}_{>0}\varepsilon_t - \underbrace{\frac{\alpha}{\alpha+\psi(1-\alpha)}}_{<0}\theta_t, \end{aligned}$$

where $1-\delta_x^h-\rho_x^h > 0$ because of (1.A.1). Note that if households have full information ($n \to \infty$), we get $\rho_x^h \to 0$ and $\delta_x^h \to 1$. Defining $\Omega \equiv -\overline{k}_3$, we can write

$$y_t = x_{t-1} + \rho_x^h(1-\Omega)e_t + \left[(\delta_x^h+\rho_x^h)(1-\Omega)+\Omega\right]\varepsilon_t - \frac{\alpha}{\alpha+\psi(1-\alpha)}\theta_t. \tag{1.B.1}$$

The signs indicated above result from $0 < \Omega = -\overline{k}_3 < 1$ (derived in Appendix 1.A), completing the proof. ■

Proof of Proposition 3.2 Now consider the nowcast error, where expectations are either those of households or producers, that is, $E_{k,t}$ substitutes for either $E_{j,l,t}$ or $E_{l,t}$, and ρ^k, δ^k correspondingly for ρ^p, δ^p or ρ^h, δ^h.

$$\begin{aligned} E_{k,t}y_t =& x_{t-1} + \rho_x^h\left(1+\overline{k}_3\right)s_t + \left[\delta_x^h + \overline{k}_3(\delta_x^h-1)\right]E_{k,t}x_t - r_t \\ =& x_{t-1} + \left\{\rho_x^h(1+\overline{k}_3) + [\delta_x^h + \overline{k}_3(\delta_x^h-1)]\rho_x^k\right\}s_t + \left[\delta_x^h + \overline{k}_3(\delta_x^h-1)\right]\delta_x^k\varepsilon_t - r_t. \end{aligned}$$

$$\begin{aligned} y_t - E_{k,t}y_t =& -\rho_x^k\left[\delta_x^h + \overline{k}_3(\delta_x^h-1)\right]s_t + \left[\delta_x^h + \overline{k}_3(\delta_x^h-1)\right](1-\delta_x^k)\varepsilon_t \\ =& \underbrace{-\rho_x^k\left[\delta_x^h + \overline{k}_3(\delta_x^h-1)\right]}_{<0} e_t + \underbrace{\left[\delta_x^h + \overline{k}_3(\delta_x^h-1)\right]}_{>0}\underbrace{(1-\delta_x^k-\rho_x^k)}_{>0}\varepsilon_t, \end{aligned}$$

or

$$y_t - E_{k,t}y_t = -\rho_x^k\left[\delta_x^h(1-\Omega)+\Omega\right]e_t + \left[\delta_x^h(1-\Omega)+\Omega\right](1-\delta_x^k-\rho_x^k)\varepsilon_t. \tag{1.B.2}$$

The fact that $0 < \Omega < 1$ (see Appendix 1.A) allows us to determine the signs of the effects of the shocks on the nowcast error. ■

Proof of Proposition 3 The model can be written in the following state space system:

$$\begin{aligned}\tilde{X}_{t+1} &= C\tilde{X}_t + D\tilde{V}_t \\ \tilde{Y}_t &= F\tilde{X}_t + G\tilde{V}_t,\end{aligned}$$

with $\tilde{Y}_t$ and $\tilde{V}_t$ defined in the main text, $C = 0, D = I_3$, and

$$F = \begin{bmatrix} 0 & 0 & 0 \\ \frac{\Omega-1}{\alpha}(1-\alpha)(1-\rho_x^h-\delta_x^h) & \frac{1-\Omega}{\alpha}\rho_x^h(1-\alpha) & \frac{\alpha-1}{\alpha+\psi(1-\alpha)} \\ 0 & 0 & 0 \end{bmatrix}$$

$$G = \begin{bmatrix} \left[\delta_x^h(1-\Omega)+\Omega\right](1-\delta_x^k-\rho_x^k) & -\rho_x^k\left[\delta_x^h(1-\Omega)+\Omega\right] & 0 \\ \Omega+\frac{1-\Omega}{\alpha}\left[1-(1-\alpha)(\rho_x^h+\delta_x^h)\right] & \frac{\alpha-1}{\alpha}\rho_x^h(1-\Omega) & \frac{1-\alpha}{\alpha+\psi(1-\alpha)} \\ \frac{(\Omega-1)}{\alpha}(1-\delta_x^h-\rho_x^h) & \frac{1-\Omega}{\alpha}\rho_x^h & \frac{-1}{\alpha+\psi(1-\alpha)} \end{bmatrix}.$$

The dynamics of the model can then be represented by the following VAR (see Fernández-Villaverde, Rubio-Ramírez, Sargent, and Watson (2007) for details):

$$\tilde{Y}_{t+1} = F\sum_{j=0}^{\infty}(C-DG^{-1}F)^j DG^{-1}\tilde{Y}_{t-j} + G\tilde{V}_{t+1} = F\sum_{j=0}^{\infty}(-G^{-1}F)^j G^{-1}\tilde{Y}_{t-j} + G\tilde{V}_{t+1}.$$

The matrix FG^{-1} results as

$$FG^{-1} = \begin{bmatrix} 0 & 0 & 0 \\ 0 & 0 & 1-\alpha \\ 0 & 0 & 0 \end{bmatrix},$$

such that

$$FG^{-1}FG^{-1} = 0$$

and we obtain the final VAR(1) representation[24]

$$\tilde{Y}_{t+1} = \underbrace{FG^{-1}}_{\equiv A}\tilde{Y}_t + \underbrace{G}_{\equiv B}\tilde{V}_{t+1}.$$

■

Proof of Corollary 1 Using the equations derived in the proof of Proposition 3, the long-run impact matrix can be calculated as $\left(I_3 - FG^{-1}\right)^{-1} G$, that is

$$\begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 1-\alpha \\ 0 & 0 & 1 \end{bmatrix} \begin{bmatrix} \left[\delta_x^h(1-\Omega)+\Omega\right](1-\delta_x^k-\rho_x^k) & -\rho_x^k\left[\delta_x^h(1-\Omega)+\Omega\right] & 0 \\ \Omega+\frac{1-\Omega}{\alpha}\left[1-(1-\alpha)(\rho_x^h+\delta_x^h)\right] & \frac{\alpha-1}{\alpha}\rho_x^h(1-\Omega) & \frac{1-\alpha}{\alpha+\psi(1-\alpha)} \\ \frac{(\Omega-1)}{\alpha}(1-\delta_x^h-\rho_x^h) & \frac{1-\Omega}{\alpha}\rho_x^h & \frac{-1}{\alpha+\psi(1-\alpha)} \end{bmatrix}$$
$$= \begin{bmatrix} * & * & 0 \\ 1 & 0 & 0 \\ * & * & * \end{bmatrix},$$

where asterisks represent non-zero elements. The middle row captures the long-run impact of the shocks on the level of labor productivity. The short-run impact of θ_t on the nowcast error equals the upper-right entry of G; it is zero. ■

[24]Note that the "poor man's invertibility condition" of Fernández-Villaverde, Rubio-Ramírez, Sargent, and Watson (2007) is satisfied as the matrix $-G^{-1}F$ has rank one and therefore at most one non-zero eigenvalue. The trace equals zero, such that all eigenvalues are zero and hence strictly less than unity.

Chapter 2

Measuring Financial Constraints: New Evidence from 20 Years of German Survey Data

This study utilizes a survey-based measure of financial constraints obtained from a sample of German manufacturing firms from 1989 to 2009. A categorization scheme is developed and multinomial logistic regression models are estimated predicting financial constraints as a function of different quantitative and qualitative indicators. In contrast to the evidence provided by Hadlock and Pierce (2010), we find the Kaplan & Zingales index to be a reliable measure of financial constraints. However, our results cast serious doubts on the reliability of the recently proposed Whited & Wu and the Size and Age index. In addition, we find the inference on the validity of various financial constraints indicators to be sensitive to the linearity assumption of the indicator-logit relationship commonly made in applied empirical work.

2.1 Introduction

The analysis of financial-market imperfections and their impact on firms' investment decisions occupies a prominent place in macroeconomics and corporate finance (Hubbard 1998). The measurement of financial constraints is key for the empirical strand of this research and the literature has suggested a variety of indices and sorting criteria based on firm characteristics. However, there is considerable debate about their relative merits.

Fazzari and Petersen (1988) constitute investment-cash flow sensitivities as a measure of financial constraints and motivate a large subsequent literature. However, Kaplan and Zingales (1997) call the findings of this literature into question. Examining the annual reports and 10-K filings of the sub-sample of firms, which Fazzari and Petersen (1988) identify as most financially constrained, Kaplan and Zingales (1997) find arguably less financially constrained firms to show significantly greater sensitivities. Subsequently, Lamont, Polk, and Saa-Requejo (2001) introduce the Kaplan and Zingales (KZ) index utilizing their sample and classification scheme. Since then, the KZ index is probably the most widely used measure of financial constraints in the empirical literature. However, there is still considerable debate on the correct measurement of financial constraints.

Given that Kaplan and Zingales (1997) study a narrow sample of 49 low dividend firms in the U.S. running from 1970 to 1984, the stability of their parameter estimates for larger and more recent samples is at question. Additional concerns are raised by the work of Erickson and Whited (2000) who show that one of the variables employed by the KZ index, Tobin's q, contains considerable measurement error. Consequently, Whited and Wu (2006) suggest an alternative financial constraint index not relying on q measures, the WW index, which they derive from a structural intertemporal investment model using a large Compustat data set.

More recently, Hadlock and Pierce (2010) add to the criticism of the KZ index, specifically of the underlying classification approach. Although Kaplan and Zingales's (1997) procedure is qualitative in nature, they also rely on quantitative information, including cash holdings as well as dividend and stock repurchase policies. However, part of this information used to infer on firms financial constraint status is subsequently also employed to construct the independent variables constituting the index. Building on this critique, Hadlock and Pierce (2010) apply a categorization approach that is in the spirit of Kaplan and Zingales (1997), yet purely qualitative. However, explaining their alternative financial constraint status with the KZ index variables, Hadlock and Pierce (2010) find their estimates to differ substantially from the original index, casting serious doubt on its validity.

Moreover, Hadlock and Pierce (2010) also evaluate the WW index and find the improvement compared to the KZ index to be marginal. Consequently, the authors advocate for an alternative approach to measure financial constraints. They propose the SA index, which relies solely on firms' size and age, claiming that these are more exogenous than the surveyed alternatives. Finally, its simplicity and low information requirements are adding to the index's appeal.

We add to the debate about the relative merits of the three indices, in particular about the validity of the KZ index. Following the approach of Hadlock and Pierce (2010), we explain firms' qualitative assessments of their financing conditions by the quantitative variables employed by the indices. Subsequently, we infer from the signs and the significance levels of the regression coefficients as well as from the overall model fit on the appropriateness of the tested indicators to measure financial constraints. Moreover, we study the sensitivity of our estimates with respect to non-linear variable transformations based on fractional polynomials.

However, the value of our exercise emerges from the data. We employ a survey-based measure of financial constraints obtained from a sample of German manufacturing firms running from 1989 to 2009. This measure is not subject to the endogeneity critique put forward by Hadlock and Pierce (2010). In addition, we utilize survey-based assessments of firms' sales expectations as well as of the profitability of the investment projects they face. Thus, we control for firms investment opportunities without relying on measures of q.

Despite the warranted criticism, we provide evidence that the KZ index is a valid measure of financial constraints. Our results are particularly striking given the narrow focus of the original KZ sample. For the WW and the SA index, however, evidence is mixed. Although we find the WW index to significantly outperform a random classification scheme, coefficient estimates for the comprised indicators are not in line with the original loadings. In particular, the industry sales growth variable, which loads positively on the index, is significantly negatively associated with our qualitative financial constraints indicator. Yet, Whited and Wu (2006) employ the variable in order to capture the availability of attractive investment opportunities (high industry sales growth), which are supposed to be positively associated with (binding) financial constraints. Moreover, for the SA index, we reject the hypothesis of external validity. Specifically, the index fails to outperform a random classification algorithm in identifying financially constrained firms. This result is relevant given that the authors claim the SA index to be a reasonable choice for measuring financial constraints in many contexts after having extensively studied its robustness and out of sample performance.

Finally, we find the inference on the validity of certain indicator variables to be sensitive to the linearity assumption of the indicator-logit relationship commonly made in applied empirical work. In particular, for firms' cash flows, dividend payouts, and leverage ratios, the association with financial constraints seems to be particularly strong for small values.

With increasing indicator values, however, associations tend to become less pronounced or even to disappear.

The remainder of this paper is organized as follows: The next section reviews the literature on the measurement of financial constraints. Section 2.3 introduces our qualitative financial constraint measure and provides a number of statistics illustrating its properties. Section 2.4 discusses the empirical framework. Section 2.5 evaluates the three indices. A final section concludes.

2.2 The evolution of the literature on measuring financial constraints

In order to infer either on the existence or on the effects of financial constraints, the empirical literature usually groups firms *ex ante* according to their likelihood of facing financial constraints, and subsequently tests the cash-flow sensitivity of investment across groups. The first prominent study in this literature is Fazzari and Petersen (1988). The authors separate financially constrained and unconstrained firms based on dividend payout ratios. They show that for low dividend paying firms, which are assumed to be more likely subject to financial constraints, investment decisions are more highly correlated to cash flow than for firms with higher dividend payout ratios, taking this as evidence for the existence and relevance of financial market imperfections.

The results of Fazzari and Petersen (1988) motivate a large subsequent literature which supports their evidence applying different variables to identify financially constrained firms (for instance, Bond and Meghir 1994 and Gilchrist and Himmelberg 1995). Although Devereux and Schiantarelli (1990) already provide contradicting evidence, namely showing that larger firms show higher investment-cash flow sensitivities, it is Kaplan and Zingales (1997) who definitely question the appropriateness of investment-cash flow sensitivities to measure financial constraints. Examining the annual reports and 10-K filings of the sub-sample of firms, which Fazzari and Petersen (1988) identify as most financially constrained, Kaplan and Zingales (1997) find arguably less financially constrained firms to show significantly greater sensitivities. Subsequent studies confirm their findings and even provide evidence for a negative relationship between the sensitivity of investment to changes in cash flow and financial constraints (for instance, Kadapakkam, Kumar, and Riddick 1998, Cleary 2006, and Chen and Chen 2012).

Within the literature on the measurement of financial constraints, the discussion above can be considered as the most prominent one. However, additional attempts have been made to identify financially constrained firms in order to serve different purposes. Musso and Schiavo (2008) provide a recent review of the empirical strategies being adopted. Irrespective of the specific aim of the studies, Musso and Schiavo (2008) find that almost all of them rely on a limited list of indicators that are associated with informational asymmetries, and thus potentially constrain firms' access to external finance. In particular, this list comprises firms' size, age, dividend policies, group membership, existence of bond ratings, and concentration of ownership. However, these proxies show several weaknesses. As highlighted by Hubbard (1998), financial constraints are cyclical in nature and thus likely to vary over time. In contrast, most of the proxies for information asymmetries that firms likely face are highly persistent. In addition, studies relying on the existence of bond ratings or certain dividend policies usually focus on listed and mature firms. For the bank based continental European financial system, their results might therefore be less valid (Rajan and Zingales 1995). Finally, most studies apply a uni-dimensional approach. They rely on a single indicator and threshold to separate financially constrained firms from those that are unconstrained.

Few multivariate and time-varying measures of financial constraints were proposed by the literature. The first, by Lamont, Polk, and Saa-Requejo (2001), introduces the KZ index utilizing the sample and classification scheme of Kaplan and Zingales (1997). Applying an ordered logistic regression framework, they explain firms' financial constraint status based on five independent variables derived from firms' balance sheets and stock prices in order to build a financial constraints index using the regression coefficients. However, Kaplan and Zingales (1997) study a narrow sample of 49 low dividend firms in the U.S. running from 1970 to 1984. Accordingly, using their index coefficients for larger firm samples and different time periods is questionable. Additional concerns are raised by the work of Erickson and Whited (2000), who show that one of the variables employed by the KZ index, Tobin's q, contains considerable measurement error. Indeed, the capability of Tobin's q to approximate for investment opportunities is rather controversial. Again, this controversy lies at the core of the debate on the sensitivity of investment to cash flow as outlined above.

Although the KZ index is probably the most widely used measure of financial constraints in the empirical literature since its introduction, alternative indices have been proposed. Whited and Wu (2006) question the stability of the parameter estimates of the KZ index across firms and over time. Although they admit that Kaplan and Zingales

(1997) convincingly demonstrate a classification scheme that successfully identifies firms with characteristics associated with external financial constraints, they doubt the external validity of the KZ index because of the limited sample size. Also approaching the intractability of relying on q measures to account for investment opportunities, Whited and Wu (2006) suggest a different an alternative index. The WW index measures financial constraints by means of the shadow price of capital derived from a structural intertemporal investment model using a large Compustat data set running from 1975 to 2001.

In a more recent attempt, Hadlock and Pierce (2010) challenge the KZ index and more specifically Kaplan and Zingales's (1997) initial approach to categorize financially constrained firms. Hadlock and Pierce (2010) criticize that the categorization is based on qualitative as well as quantitative information, including a firm's cash position and its recent dividend and stock repurchase policy. They claim this to potentially induce endogeneity, because the same information is later employed to construct some of the independent variables explaining the financial constraint status and constituting the KZ index. Hadlock and Pierce (2010) further evaluate the sensitivity of Kaplan and Zingales's (1997) regression coefficients to a comparable but purely qualitative categorization approach. In particular, they categorize firms' financial constraint status based on a qualitative evaluation of annual reports and 10-K filings for a randomly selected sample of Compustat firms running from 1995 to 2004. Explaining their alternative financial constraint status with the KZ index variables based on a parallel modeling approach, Hadlock and Pierce (2010) find their estimates to differ substantially from the KZ index coefficients. Furthermore, according to their results, the original KZ index and their model predictions are approximately uncorrelated, casting serious doubt on the validity of the KZ index.

Moreover, Hadlock and Pierce (2010) also evaluate the WW index by analyzing six factors of the incorporated indicators that are created from Compustat data. Explaining their qualitative categorization based on the six indicators, they find only three of them to show significant coefficients and to agree in sign with the WW index. Given their critique on the KZ index and their inconclusive results for the WW index, Hadlock and Pierce (2010) advocate for a conservative approach to measure financial constraints. Specifically, they propose an alternative index solely relying on firms' size and age since they claim that these are more exogenous than the surveyed alternatives. However, they admit that these variables are rather persistent and thus less likely to capture the time variation in financial constraints.

According to this literature review, any attempt to assess the validity of existing financial constraints measures should come close to meet certain criteria: First, the categorization of financial constrained and unconstrained firms should not rely on any information that is subsequently incorporated in the set of explanatory variables to be evaluated. Second, in order to account for future investment opportunities q measures are controversial and should be replaced by more appropriate alternatives. Third, the measure should be able to capture cross-sectional as well as time series variation. Finally, the studied sample should be as large and representative as possible as well as not being limited to listed companies if inference shall be made that also holds in a continental European context.

2.3 Categorization of financing conditions and sample construction

2.3.1 Firms' self-assessment of financing conditions

This study combines a survey based financial constraints measure and publicly available balance sheet data to assess the capability of commonly applied indicators to measure financial constraints. Both pieces of information are linked in the German EBDC Business Investment Panel.[1] Within this merged data set firms' financial constraint status is obtained from the German IFO Investment Survey, which focuses on the quantification of firms' current investment activities and future investment plans as well as on their qualitative assessments of certain investment determinants. The semi-annual survey has been conducted in spring and autumn by the IFO Institute since 1955. It comprises firms across all industries within the manufacturing sector, aiming to provide representative aggregate figures. The survey questions on firms' self-assessment of their investment determinants were introduced in fall 1989 and were subsequently surveyed at annual frequency. However, due to disclosure considerations, the sample ends in 2009. Respondents are surveyed repeatedly and their number is held fairly constant at about 1,500. On aggregate, the surveyed firms account for about one quarter of total investment expenditures in the German manufacturing sector. Assessing the representativeness of the survey, Bachmann and Zorn (2013) show that aggregate investment figures derived from the survey are comparable to the aggregate investment statistics obtained from national accounts.

In order to derive a firm and time specific indicator of financial constraints, we rely on the firms' qualitative assessment of their investment determinants. Specifically, at

[1]For a description of the data set, see Hönig (2010).

the end of each year firms are asked to separately assess the impact of six determinants on their physical investment expenditures for that year. The six determinants comprise: (1) current and expected sales, (2) financing conditions, (3) expected profitability, (4) technical factors, (5) economic policy, and (6) others. Answers are given on an ordered scale consisting of five categories, ranging from (1) strongly favorable, over (2) weakly favorable and (3) neutral, to (4) weakly adverse and (5) strongly adverse. According to the survey question, firms report financing conditions rather than financial constraints. However, the two concepts can be mapped. Compared to a dichotomous financial constraint indicator, the two adverse financing conditions categories, (4) and (5), represent financial constraints, while unconstrained firms should select themselves into the remaining categories, (1) to (3). Also note that given the explicit reference of the survey question to the impact realization on investment, reported financial constraints can be considered as actually binding. For the sake of clarity, in the following we will rather refer to financing conditions than financial constraints. Referring to financial constraints, however, we assume the mapping outlined above.

In addition to the financial constraint status, two of the other investment determinants are utilized in the subsequent analysis; namely, firms current and expected sales as well as the profitability of potential investment projects. Firms own assessments of the influence of these two determinants on their investment decision directly measure their future investment opportunities, and thus provide an appealing alternative to conventional q measures. As discussed in Section 3.2, the KZ index has been criticized based on its reliance on average Tobin's q, measured as the market value of assets divided by the book value of assets, which is potentially observed with considerable measurement error (Erickson and Whited 2000). However, this measurement error might bias the coefficient estimates of the remaining variables comprised in the KZ index, especially those of cash flows and cash holdings. More specifically, estimates might be driven by the accelerator effect rather than the desired liquidity effect.

2.3.2 Sample construction and descriptive statistics

The EBDC Business Investment Panel links the IFO Investment Survey to firms' balance sheet information obtained from the commercial databases Amadeus and Hoppenstedt.[2]

[2]Amadeus is a product of the Bureau van Dijk Electronic Publishing GmbH. It covers annual accounts and investment data of disclosing companies in the German Commercial Register with a bank credit index of a maximum of 499, according to the Creditreform Association. The Hoppenstedt Accounting Database is a product of the Hoppenstedt Business Information GmbH and gives detailed information on financial

The merged data set comprises 613 firms and 2,961 firm-year observations for which all relevant balance sheet items and survey data are available.

Table 2.1: Summary of annual financing conditions 1989-2009 in %

Year	Strongly favorable financing conditions	Favorable financing conditions	Neutral financing conditions	Adverse financing conditions	Strongly adverse financing conditions	N
1989	3.3	17.6	68.1	6.6	4.4	91
1990	4.0	11.0	63.0	19.0	3.0	100
1991	1.8	8.3	58.7	22.9	8.3	109
1992	2.0	8.8	50.0	30.4	8.8	102
1993	2.0	12.2	61.2	17.3	7.1	98
1994	2.7	16.4	56.4	19.1	5.5	110
1995	4.1	13.2	66.9	13.2	2.5	121
1996	2.6	19.8	58.6	13.8	5.2	116
1997	3.3	15.0	65.0	12.5	4.2	120
1998	2.5	17.5	69.2	9.2	1.7	120
1999	3.6	14.4	69.4	8.1	4.5	111
2000	0.8	12.4	70.2	13.2	3.3	121
2001	0.8	10.7	61.8	19.1	7.6	131
2002	1.5	9.7	60.4	20.9	7.5	134
2003	0.8	6.8	64.7	21.1	6.8	133
2004	1.8	11.8	64.5	16.0	5.9	169
2005	1.6	9.9	71.2	12.6	4.7	191
2006	2.0	13.8	74.0	7.9	2.4	254
2007	2.5	15.2	66.0	15.7	0.5	197
2008	1.6	10.5	71.1	15.8	1.1	190
2009	1.6	7.0	57.2	22.6	11.5	243
Total	2.1	12.2	64.8	15.9	5.0	2961

The table presents cross-sectional averages of firms' assessments of their financing conditions over time. The sample is restricted to the 2,961 firm-year observations for which all subsequently analyzed survey and balance sheet information is available. N denotes the number of respondents for each year.

Table 2.1 provides an overview of firms' assessments of their financing conditions over time. The table illustrates certain sample features: The availability of financial statements

statements of German companies. Together, the two databases comprise almost all final statements of German firms published since 2005, while historical information for large firms dates back to 1987. The matching of the balance sheet information with the survey data is performed on the names and physical addresses of the firms. See Hönig (2010) for details.

from commercial providers increases over time. This is reflected by a time trend in firm-year observations in the sample. Moreover, the table shows both considerable cross-sectional and time series variation. Over the whole sample period, an average of 20.9% of the firms report that financing conditions have adverse effects on their investments, while 14.3% report favorable and 64.8% neutral financing conditions. The number of adverse assessments is particularly pronounced during the recession following the German reunification boom in the early 1990s, after the burst of the "Dot-com Bubble" in the early 2000s, and in 2009 in the follow-up of the financial crisis of 2007/08.

Turning to the balance sheet characteristics, Table 2.2 shows summary statistics of the pooled sample of the 2,961 firm-years grouped by firms' assessments of their financing conditions. The selection of variables reflects the indicators considered by Hadlock and Pierce (2010) in order to assess the validity of the KZ and the WW index. However, Tobin's q is an exception. Since our sample mainly consists of non-listed firms, we do not observe the market value of firms' assets. In contrast, we rely on self-assessed and forward looking sales and profitability measures to account for firms' investment opportunities.

In order to make our sample comparable to the original studies, we follow the variable definitions applied by Lamont, Polk, and Saa-Requejo (2001) and Whited and Wu (2006), which were subsequently adopted by Hadlock and Pierce (2010).[3] In addition, we apply an outlier adjustment that is aimed at ensuring comparability to the original studies.[4]

[3] Specifically, we compute two alternative cash flow indicators. In both cases, cash flow is defined as net income plus depreciation. However, in the first case, *Cash flow/K*, cash flow is normalized by a firm's capital stock in terms of its tangible assets at the beginning of the year, while in the second case, *Cash flow/assets*, it is scaled by lagged total assets. Furthermore, *Cash/K* refers to holdings of cash and cash equivalents including securities and is scaled by lagged tangible assets. There are also two alternative leverage ratios. On the one hand, *Debt/total capital* is defined as the sum of short-term and long-term debt over the sum of short-term and long-term debt plus equity, and on the other hand, *Long-term debt/assets* refers to long-term debt over total assets. *Dividends/K* is defined as dividends paid out normalized by lagged tangible assets. The *Industry sales growth* is computed as the average year on year growth rate of real sales within the two digit sub-categories of the German standard industry classification, while *Sales growth* refers to firm-specific real sales growth. *Investment/K* denotes expenditures on physical investments scaled by lagged tangible assets. Total amounts of *Sales*, *Tangible assets*, and *Total assets* are reported in inflation adjusted year 2005 euros, obtained by deflating nominal values with the German producer price index. Moreover, *Age* is computed as the difference between the current year and the year of incorporation. Note however, that Hadlock and Pierce (2010) measure firm age in the number of years the firm is listed with a non-missing stock price on Compustat. Finally, assessments of *Sales* and *Profit* are coded as dummy variables indicating strongly favorable, favorable, neutral, adverse, and strongly adverse impacts on investment spending respectively.

[4] In particular, the two leverage ratios are set to one if they exceed the value of one or if a firm is in negative equity. Firm-year observations with negative leverage ratios are deleted. Continuous variables are winsorized at both tails, at 2% and 98%, except those that are naturally bounded at zero and except *Cash flow/K*, which is winsorized at a value of five.

However, we find one considerable difference in sample definitions between Hadlock and Pierce (2010) and the two earlier studies. In contrast to Lamont, Polk, and Saa-Requejo (2001) and Whited and Wu (2006), Hadlock and Pierce (2010) do not restrict their sample to firms showing positive sales growth. Lamont, Polk, and Saa-Requejo (2001) and Whited and Wu (2006) exclude these firms in order to focus their analysis on firms that face constraints to obtain external finance rather than those being in financial distress. In the case of our sample, this restriction is binding for about one third of the firm-years, indicating a potentially substantial source of bias. Therefore, in the following, we report estimation results for the correspondingly restricted as well as for the unrestricted sample.

Assessing the comparability of the sample with our main reference study, Hadlock and Pierce (2010), we find a high degree of similarity. The sample employed by Hadlock and Pierce (2010) comprises 1,848 Compustat firm-years representing 356 listed firms operating during the 1995 to 2004 period, while the average size across firm-years is 783 million inflation adjusted year 2004 dollars. Evaluated by this measure, firm-years in our sample seem to be quite similar showing a book value of 764 million inflation adjusted year 2005 euros. In addition, in both samples the firm size distribution is heavily skewed with the median of total assets being about 8 times smaller than the mean. The similarity in firm size is likely driven by the construction of our sample. Specifically, conditioning on the availability of balance sheet information as well as potential survivorship bias in the IFO Investment Survey somewhat distorts our sample towards larger firms.

Comparing the financially unconstrained firms listed in columns one to three of Table 2.2 with constrained firms in columns four and five, observed differences are in line with theory. In particular, constrained firms appear to have lower cash flows and hold less cash, work with a higher leverage, pay less dividends, experience lower sales growth, report weaker demand as well as less profitable investment opportunities, and finally, invest considerably less.

2.3.3 Comparing neutral and favorable financing conditions

The summary statistics provided by Table 2.2 reveal that the relationship of firms' self-assessed financing conditions and certain financial constraints indicators is rather non-monotonic. Indeed, the only variables showing unambiguously monotonic associations in medians and means are *Investment/K*, *Sales*, and *Profit*. This likely reflects cyclicality in the indicators and the financing conditions. At the same time, this pattern suggests that reported financing conditions measure the availability of external finance rather than

Table 2.2: Firm characteristics by self-assessment of financing conditions

Indicator	Statistic	Strongly favorable financing conditions	Favorable financing conditions	Neutral financing conditions	Adverse financing conditions	Strongly adverse financing conditions
Cash flow/K	Median	0.47	0.43	0.44	0.32	0.19
	Mean	0.88	0.68	0.75	0.41	0.16
Cash flow/assets	Median	0.12	0.10	0.09	0.07	0.05
	Mean	0.11	0.11	0.10	0.07	0.04
Cash/K	Median	0.18	0.17	0.20	0.09	0.05
	Mean	1.01	1.24	1.34	0.61	0.42
Debt/total capital	Median	0.45	0.56	0.48	0.57	0.63
	Mean	0.49	0.54	0.49	0.57	0.61
Long-term debt/assets	Median	0.05	0.09	0.04	0.09	0.10
	Mean	0.11	0.11	0.09	0.12	0.14
Dividends/K	Median	0.00	0.00	0.00	0.00	0.00
	Mean	0.15	0.16	0.18	0.08	0.03
Industry sales growth	Median	0.03	0.03	0.03	0.02	-0.02
	Mean	0.02	0.03	0.02	0.01	-0.03
Sales growth	Median	0.04	0.04	0.02	0.00	-0.04
	Mean	0.03	0.05	0.03	-0.00	-0.04
Sales	Median	9.48e+07	1.18e+08	1.50e+08	1.01e+08	7.38e+07
	Mean	1.83e+08	4.38e+08	8.06e+08	5.16e+08	2.44e+08
Investment /K	Median	0.30	0.27	0.25	0.21	0.14
	Mean	1.04	0.80	0.65	0.54	0.40
Investment growth	Median	0.07	0.16	0.00	-0.11	-0.24
	Mean	0.42	0.35	0.20	0.04	-0.02
Tangible assets	Median	1.36e+07	2.13e+07	2.43e+07	1.96e+07	1.52e+07
	Mean	3.02e+07	7.48e+07	1.15e+08	8.60e+07	4.51e+07
Total assets	Median	5.12e+07	8.60e+07	1.15e+08	8.54e+07	5.26e+07
	Mean	1.52e+08	4.98e+08	8.98e+08	6.55e+08	2.55e+08
Age	Median	82.00	82.00	78.00	86.50	80.50
	Mean	84.40	90.82	85.22	88.90	83.70
Sales (++)	Mean	0.49	0.31	0.15	0.09	0.05
Sales (+)	Mean	0.38	0.45	0.37	0.28	0.20
Sales (=)	Mean	0.10	0.11	0.20	0.16	0.10
Sales (-)	Mean	0.02	0.09	0.19	0.30	0.21
Sales (- -)	Mean	0.02	0.04	0.09	0.18	0.44
Profit (++)	Mean	0.43	0.27	0.09	0.06	0.03
Profit (+)	Mean	0.48	0.52	0.40	0.28	0.20
Profit (=)	Mean	0.06	0.10	0.21	0.10	0.08
Profit (-)	Mean	0.02	0.09	0.21	0.39	0.21
Profit (- -)	Mean	0.02	0.03	0.08	0.18	0.49

The table presents sample characteristics in terms of pooled medians and means of the 2,961 firm-year observations for which all subsequently analyzed survey and balance sheet information is available. Sales, tangible assets and total assets are reported in inflation adjusted year 2005 euros.

the monetary policy stance. However, the relation to indicators of leverage and size even seems to be U-shaped or inversely U-shaped respectively. Yet in the case of leverage, non-monotonicity may well be a reflection of endogeneity as noted by several studies (see, for instance, Acharya, Almeida, and Campello 2007, Hennessy and Whited 2007, and Almeida and Campello 2007). A similar argument applies to firms' holdings of cash. On the contrary, firm size is considered to be rather exogenous and to approximate for external financial constraints arising from information asymmetries as well as from limited access to financial markets.

Overall, summary statistics suggest a multi-dimensional relationship between reported financing conditions and financial constraints as they are perceived in the literature. On the one hand, firms reporting that financing conditions have no impact on their investment decisions, column three of Table 2.2, seem to be least dependent on external sources of finance as well as to face the lowest information asymmetries. In particular, they are considerably larger in terms of total assets and show the highest cash holdings and cash flows relative to their investment spending. On the other hand, firms reporting either favorable or unfavorable financing conditions are smaller in size and their assessment of financing conditions largely reflects variation in the soundness of balance sheets. In the light of these findings, the ordering of the financing conditions categories is at question. Therefore, the next section reflects on the appropriateness of the ordered logistic regression framework, applied by our benchmark studies Lamont, Polk, and Saa-Requejo (2001) and Hadlock and Pierce (2010), in order to assess the predictive power of the financial constraints indicators.

2.4 Model choice

This section discusses the statistical properties of the data, which guide the choice of our statistical framework. Based on the ordering of the self-assessed financing conditions categories, ordered logistic regression is a natural candidate. Indeed, Lamont, Polk, and Saa-Requejo (2001) and Hadlock and Pierce (2010) apply ordered logistic regressions, but they do not discuss whether the underlying assumptions are satisfied in their data.

We consider three alternative specifications of the ordered logistic regression model, namely the continuation ratio, the adjacent categories, and the proportional odds model, which are most commonly used in applied work. In order to test their respective assumptions on the odds across categories, we perform standard likelihood ratio tests and in the last case also a Brant test (Brant 1990). However, if tested against an unrestricted multinomial logistic regression, each of the models is rejected at the 1% significance level.[5]

Turning towards statistical frameworks that do not assume a specific ordering of the dependent variable, we consider linear discriminant function analysis and multinomial logistic regression. Comparing the two, linear discriminant function analysis is surely more powerful. However, its assumptions, multivariate normality, linearity and homoscedasticity, are not met by the data.[6] In contrast, multinomial logistic regression does not make these assumptions, but relies on the independence of irrelevant alternatives, which the Hausman-McFadden test does not reject at the 1% significance level (Hausman and McFadden 1984).

Assuming the appropriateness of the multinomial logistic regression framework, we consider the reduction of the number of equations and parameters to be estimated by collapsing the number of categories in the dependent variable. In particular, the two tail categories, indicating strongly favorable and strongly adverse financing conditions respectively, together comprise only 7% of the total number of firm-year observations. Accordingly, little information is lost by collapsing the two favorable as well as the two adverse categories.[7] Furthermore, this procedure increases the feasibility of an additional assumption. By pooling firm-year observations over twenty years, we implicitly assume that the thresholds applied by individual firms assessing their financing conditions are the same across firms as well as over time. However, this assumption should be more appropriate if only favorable, neutral, and adverse financing conditions are compared.

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[5]In order to limit the impact of collinearity, we separately test the ordered logistic regression models incorporating the full set of indicators as well as specifications comprising only the indicators of the KZ, the WW, and the SA index respectively. In addition, we conduct the same series of tests on an alternatively specified dependent variable, reducing the number of categories. In particular, we collapse the two favorable as well as the two unfavorable categories. However, the restricted, ordered models are rejected throughout.

[6]According to the test developed by Doornik and Hansen (2008), multivariate normality is refused at the 1% confidence level. In addition, figures 2.1 and 2.2 document considerable heterogeneity of variances across groups.

[7]In addition, Hosmer Jr, Lemeshow, and Sturdivant (2013) provide guidelines for the minimal number of observations that should be used for multinomial logistic regression. Accordingly, at least 10 cases per category and independent variable are required, yet 20 are desired. Considering the model specification employed throughout the subsequent empirical analysis, the strongly favorable category clearly does not meet the minimum requirement, while the strongly adverse category marginally exceeds the threshold in most cases except for the sub-sample requiring positive sales growth. Note however, that for the full sample a likelihood ratio test comparing multinomial logistic regression models utilizing either the variable set of the KZ index or that of the WW index tends to refuse the constraints imposed by collapsing the two adverse categories but not the two favorable ones. Yet sensitivity analysis shows that the rejection is mainly driven by differences in cash flows.

2.4.1 Exploring non-linearity

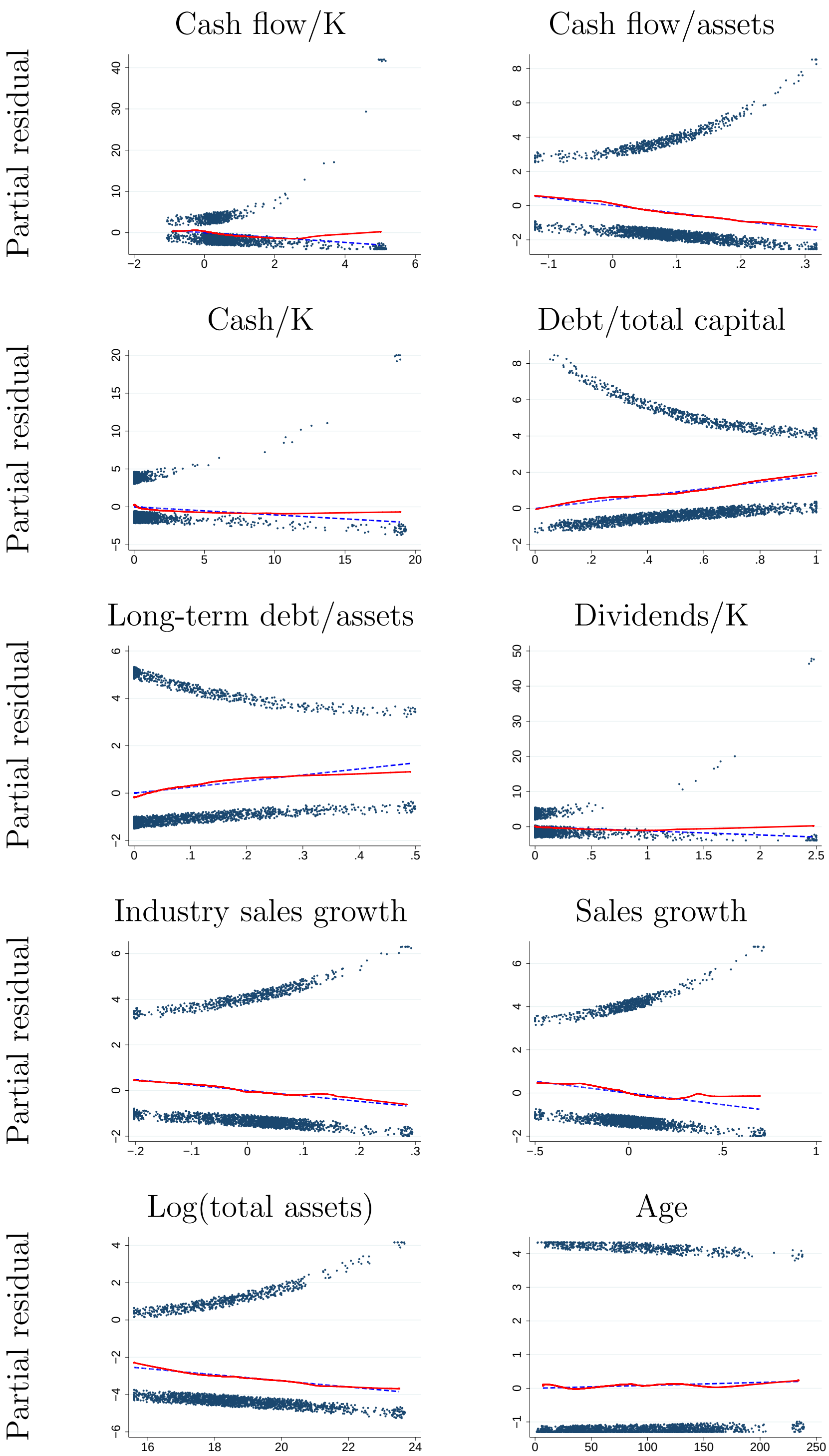
The multinomial logistic framework relies on the assumption that the relationship of the dependent and the continuous independent variables is linear in the logit. Prior to the empirical analysis, we assess this assumption based on a visual inspection of the bivariate relationships. Accordingly, Figures 2.1 and 2.2 illustrate the relationships of the indicators applied by the KZ, WW, and SA index with firms' self-assessed financing conditions by comparing adverse conditions with both neutral and favorable ones.

In Figure 2.1, all subsequently analyzed indicator variables are separately employed to discriminate between adverse financing conditions, coded one, and neutral financing conditions, coded zero, based on logistic regressions. For each indicator a scatterplot of the partial logit residuals is shown. In order to reveal potential non-linearity in the relationships a locally weighted scatterplot smoother (lowess) is displayed together with the linear model prediction (Cleveland 1979). A deviation of the lowess smoother from the model prediction hints to a violation of the linearity assumption. Figure 2.1 indicates that the linearity assumption is not severely violated for the examined bivariate relationships. However, somewhat considerable deviations of the lowess smoother from the linear model fit are found for *Cash flow/K*, *Cash/K*, *Long-term debt/assets*, *Dividends/K*, and *Sales growth*. Moreover, all bivariate relationships, except for *Age*, are found to be statistically significant at the 5% level and regression coefficients agree in sign with those applied in the KZ, WW, and SA index. There is only one exception, *Industry sales growth*, which loads positively on the WW index, is found to be negatively associated with the probability to report adverse financing conditions.

In contrast, stronger evidence for non-linearity in the bivariate relationships is found for the logistic regressions separating adverse from favorable financing conditions (see Figure 2.2). In some cases even the monotonicity of the relationship is at question. This finding also provides an explanation for the results presented by Hadlock and Pierce (2010). Applying an ordered logistic regression approach and not accounting for potential non-linearity, the authors find that estimates for *Cash/K* and *Dividends/K* flip sign across model specifications. However, the observed patterns in the bivariate relationships should be considered to be suggestive only. In particular, some of the indicator variables in Figure 2.2 show considerably higher dispersions and lowess smoothers seem to be more likely driven by the impact of outliers.

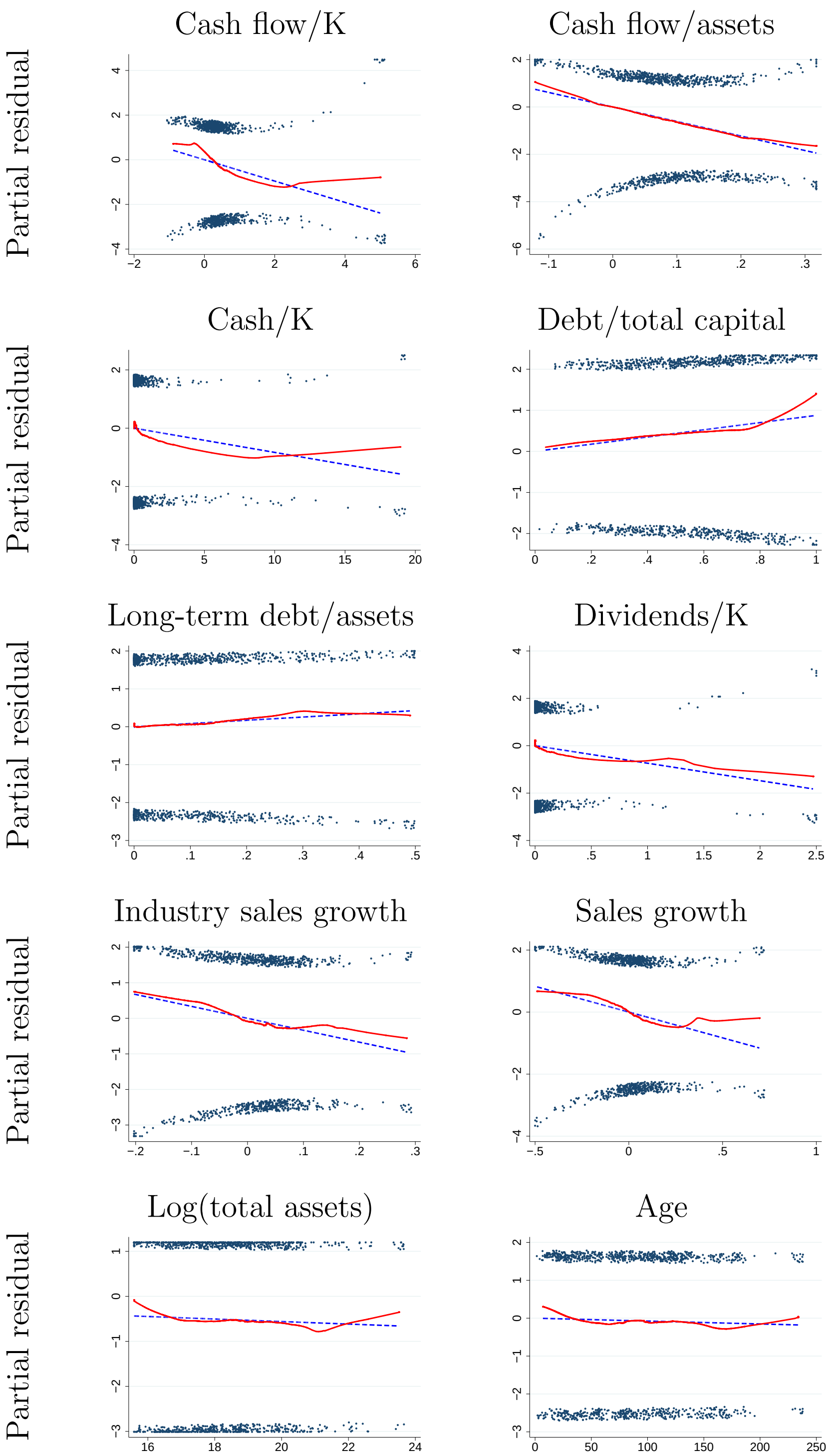
Based on this evidence, we decide to allow for potential non-linearity in the modeling approach throughout the empirical analysis. However, in order to limit the sensitivity of

Figure 2.1: Prediction of adverse vs. neutral financing conditions



Graphs illustrate jittered partial logit residuals obtained from logistic regressions employed to discriminate between adverse financing conditions, coded one, and neutral financing conditions, coded zero. Individual logistic regressions comprise the explanatory variable and a constant. Dashed lines indicate the model fit, while solid lines are lowess smoothers of the residuals based on a bandwidth of 0.8.

Figure 2.2: Prediction of adverse vs. favorable financing conditions



Graphs illustrate jittered partial logit residuals obtained from logistic regressions employed to discriminate between adverse financing conditions, coded one, and favorable financing conditions, coded zero. Individual logistic regressions comprise the explanatory variable and a constant. Dashed lines indicate the model fit, while solid lines are lowess smoothers of the residuals based on a bandwidth of 0.8.

our results to the potential impact of outliers, inference on the linearity of indicator-logit relationships is conditioned on a rigorous outlier adjustment. Specifically, we test for the robustness of our results by winsorizing 5% at both tails of the distributions of all continuous variables. Moreover, in order to parsimoniously account for potential non-linearity, we rely on fractional polynomials as suggested by Royston and Altman (1994).[8] In all empirical specifications, we select the best non-linear transformation of the explanatory variables following the closed-test procedure as described in Hosmer Jr, Lemeshow, and Sturdivant (2013). Specifically, we replace the linear variable by its non-linear transformation if the model fit is significantly improved according to the deviance statistic applying a 5% significance level.[9]

2.5 Evaluating measures of financial constraints

Following the approach of Hadlock and Pierce (2010), we explain firms' qualitative assessments of their financing conditions by the quantitative variables employed by the KZ, WW, and SA index. Subsequently, we infer from the signs and the significance levels of the regression coefficients as well as from the overall model fit on the appropriateness of the tested variables to indicate financial constraints. Moreover, we study the sensitivity of our estimates with respect to non-linear variable transformations based on fractional polynomials. In contrast to Hadlock and Pierce (2010) and Lamont, Polk, and Saa-Requejo (2001), however, we do not employ an ordered logistic regression framework, but rely on a multinomial logistic regression (see the discussion in Section 2.4). More specifically, we simultaneously estimate two logistic regressions in order to separate the three distinct categories of our dependent variable. One discriminates between adverse and neutral financing conditions, while the other separates adverse from favorable financing conditions.

[8] Fractional polynomials are an extended family of curves that comprise conventional low order polynomials but cover a larger variety of shapes. In contrast to conventional high order polynomials, fractional polynomials are also found to provide a better fit at the extreme values of the covariates. However, the best non-linear fractional polynomial transformation is not known in advance. It has to be estimated based on a systematic search from a set of given functions. We apply the standard set of functions suggested by Royston and Altman (1994), which requires the estimation of two additional parameters.

[9] P-values are obtained by referring the difference in deviances of the non-linear and the linear model to the χ^2 distribution. As Royston and Altman (1994) point out, these p-values are approximate and rather conservative.

2.5.1 The KZ index

Lamont, Polk, and Saa-Requejo (2001) create the KZ index based on the sample studied by Kaplan and Zingales (1997) and their partially qualitative measures of financial constraints. The KZ index loads positively on *Debt/total capital* and *Tobin's q*, and negatively on *Cash flow/K*, *Cash/K*, and *Dividends/K*.[10] Analyzing a sample of mainly non-listed firms, we do not obtain the market value of assets in order to compute q measures. Instead, we approximate firms investment prospects and potential accelerator effects by two qualitative indicator variables: *Sales* and *Profit* . The two variables are based on firms' self-reported assessments of the impact of current sales and future sales expectations as well as the expected profitability of available investment projects on their investment spending. Both variables are measured on a five category scale ranging from a strongly favorable to a strongly adverse impact assessment.

Table 2.3 reports regression results. Comparing our estimates with those reported by Hadlock and Pierce (2010), we find certain similarities. In particular, Hadlock and Pierce (2010) find only two out of the five components, cash flow and leverage, to be consistently significant with a sign that agrees with the KZ index. This finding is supported by our estimates for the logistic regression discriminating between adverse financing conditions and neutral financing conditions, presented in the top panel of Table 2.3. Hadlock and Pierce (2010) report that for two additional components, Tobin's q and dividends, the coefficients flip signs across estimated models and are in many cases insignificant, particularly for the dividend payments. In our sample, only in one out of six regressions dividends are significant, while estimates on Tobin's q might be less comparable. Finally, cash holdings load negatively on the KZ index while showing generally positive and significant coefficients in the models estimated by Hadlock and Pierce (2010). According to our results, cash holdings flip sign only once, yet the coefficient is neither significant nor negative throughout.

Section 2.4 presents bivariate regressions providing evidence for potential non-linearity. Therefore, we assess the linearity assumption of the logistic regression model by fitting non-linear transformations of the continuous explanatory variables (factional polynomials) into our models and subsequently evaluate the non-linear against the linear model fit. According to the results comprised in Table 2.4, for two of the KZ indicator variables, namely cash holdings and dividend payments, an estimated non-linear transformation significantly

[10]According to Lamont, Polk, and Saa-Requejo (2001), the KZ index is calculated as follows: - 1.002(Cash flow/K) + 0.283(Tobin's q) + 3.139(Debt/total capital) - 39.368(Dividends/K) - 1.315(Cash/K).

Table 2.3: Predicting financing conditions with KZ indicators (linear)

	Linear			Linear Sales growth > 0			Linear Winsorize at 5%		
Equation (1): Adverse vs. neutral									
Cash flow/K	-0.167	**	(0.084)	-0.285	**	(0.121)	-0.235	**	(0.113)
Cash/K	-0.054	**	(0.025)	-0.023		(0.033)	-0.143	***	(0.045)
Dividends/K	-0.280		(0.223)	-0.254		(0.313)	-0.846	**	(0.368)
Debt/total capital	1.413	***	(0.233)	1.007	***	(0.336)	1.303	***	(0.245)
Sales (+)	0.191		(0.190)	0.161		(0.216)	0.177		(0.191)
Sales (=)	0.214		(0.220)	0.071		(0.267)	0.201		(0.220)
Sales (-)	0.370	*	(0.224)	0.077		(0.281)	0.367		(0.224)
Sales (--)	0.435	*	(0.254)	0.642	*	(0.357)	0.409		(0.255)
Profit (+)	0.131		(0.226)	0.234		(0.273)	0.143		(0.227)
Profit (=)	-0.192		(0.264)	0.018		(0.325)	-0.164		(0.264)
Profit (-)	0.918	***	(0.250)	1.338	***	(0.309)	0.925	***	(0.251)
Profit (--)	1.376	***	(0.283)	1.541	***	(0.393)	1.370	***	(0.284)
Equation (2): Adverse vs. favorable									
Cash flow/K	-0.169		(0.106)	-0.396	***	(0.144)	-0.230		(0.144)
Cash/K	-0.063	**	(0.031)	0.001		(0.043)	-0.188	***	(0.056)
Dividends/K	-0.285		(0.263)	-0.030		(0.364)	-0.552		(0.453)
Debt/total capital	0.307		(0.326)	0.063		(0.435)	0.212		(0.343)
Sales (+)	0.444	**	(0.214)	0.453	*	(0.245)	0.437	**	(0.214)
Sales (=)	0.846	***	(0.273)	0.835	**	(0.340)	0.843	***	(0.273)
Sales (-)	1.037	***	(0.310)	1.082	**	(0.419)	1.035	***	(0.311)
Sales (--)	1.179	***	(0.423)	1.085	*	(0.605)	1.172	***	(0.423)
Profit (+)	0.891	***	(0.241)	0.750	**	(0.295)	0.888	***	(0.242)
Profit (=)	1.459	***	(0.317)	1.268	***	(0.392)	1.477	***	(0.317)
Profit (-)	2.616	***	(0.328)	2.348	***	(0.401)	2.623	***	(0.329)
Profit (--)	3.148	***	(0.467)	3.079	***	(0.738)	3.140	***	(0.467)
N	2,961			1,690			2,961		
Log likelihood	-2,338			-1,354			-2,328		
Pseudo R^2	0.109			0.087			0.113		
AUROC Equ. (1)	0.724			0.718			0.732		
AUROC Equ. (2)	0.828			0.789			0.830		
Corr. KZ with Equ. (1)	0.756			0.760			0.838		
Corr. KZ with Equ. (2)	0.869			0.667			0.834		

Coefficient estimates are presented together with asymptotic standard errors, which are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The reference categories for Sales and Profit are the most favorable ones. AUROC refers to the area under the receiver operating characteristic curve. Correlations with the original KZ index are based on the four continuous variables only.

Table 2.4: Predicting financing conditions with KZ indicators (non-linear)

	Non-linear			Non-linear Sales growth > 0			Non-linear Winsorize at 5%		
Equation (1): Adverse vs. neutral									
Cash flow/K	-0.148	*	(0.078)	-0.270	**	(0.117)	-0.201	*	(0.109)
Cash/K (1)	-0.433	***	(0.106)	-0.414	***	(0.145)	-0.482	***	(0.143)
Cash/K (2)	0.144	***	(0.038)	0.148	***	(0.052)	0.198	**	(0.077)
Dividends/K (1)	-0.001	***	(0.000)	-0.297	**	(0.135)	-0.445	***	(0.101)
Dividends/K (2)	-0.288	***	(0.066)	-0.018	**	(0.008)	-0.025	***	(0.006)
Debt/total capital	1.298	***	(0.253)	0.797	**	(0.362)	1.304	***	(0.265)
Sales (+)	0.212		(0.192)	0.175		(0.218)	0.204		(0.192)
Sales (=)	0.249		(0.221)	0.082		(0.268)	0.233		(0.221)
Sales (-)	0.441	*	(0.226)	0.133		(0.284)	0.440	*	(0.226)
Sales (- -)	0.487	*	(0.257)	0.656	*	(0.360)	0.476	*	(0.258)
Profit (+)	0.119		(0.227)	0.217		(0.275)	0.130		(0.227)
Profit (=)	-0.189		(0.265)	0.015		(0.327)	-0.166		(0.265)
Profit (-)	0.877	***	(0.252)	1.306	***	(0.311)	0.876	***	(0.252)
Profit (- -)	1.360	***	(0.285)	1.517	***	(0.394)	1.371	***	(0.285)
Equation (2): Adverse vs. favorable									
Cash flow/K	-0.200	**	(0.099)	-0.440	***	(0.141)	-0.248	*	(0.141)
Cash/K (1)	-0.521	***	(0.135)	-0.510	***	(0.180)	-0.490	**	(0.191)
Cash/K (2)	0.171	***	(0.048)	0.193	***	(0.065)	0.174	*	(0.101)
Dividends/K (1)	-0.001		(0.000)	0.036		(0.163)	-0.145		(0.130)
Dividends/K (2)	-0.121		(0.084)	0.003		(0.010)	-0.007		(0.007)
Debt/total capital	-0.019		(0.355)	-0.349		(0.471)	-0.002		(0.373)
Sales (+)	0.436	**	(0.215)	0.437	*	(0.247)	0.425	**	(0.215)
Sales (=)	0.850	***	(0.274)	0.820	**	(0.341)	0.835	***	(0.274)
Sales (-)	1.047	***	(0.311)	1.055	**	(0.421)	1.040	***	(0.311)
Sales (- -)	1.175	***	(0.423)	1.031	*	(0.605)	1.162	***	(0.423)
Profit (+)	0.884	***	(0.242)	0.744	**	(0.297)	0.891	***	(0.242)
Profit (=)	1.462	***	(0.317)	1.272	***	(0.393)	1.481	***	(0.317)
Profit (-)	2.605	***	(0.328)	2.374	***	(0.403)	2.613	***	(0.328)
Profit (- -)	3.114	***	(0.466)	3.056	***	(0.735)	3.125	***	(0.466)
N	2,961			1,690			2,961		
Log likelihood	-2,316			-1,343			-2,313		
Pseudo R^2	0.117			0.095			0.119		
AUROC Equ. (1)	0.738			0.731			0.739		
AUROC Equ. (2)	0.832			0.793			0.832		
Corr. KZ with Equ. (1)	0.688			0.704			0.805		
Corr. KZ with Equ. (2)	0.702			0.618			0.816		
Delta deviance (linear)	42.913			22.082			29.467		
P-value	0.000			0.005			0.000		

Coefficient estimates are presented together with asymptotic standard errors, which are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The reference categories for Sales and Profit are the most favorable ones. AUROC refers to the area under the receiver operating characteristic curve. Correlations with the original KZ index are based on the four continuous variables only. Deviance statistics are compared to the linear specifications in Table 2.4 by referring to the χ^2 distribution (8 d.o.f.).

improves the overall model fit.[11]

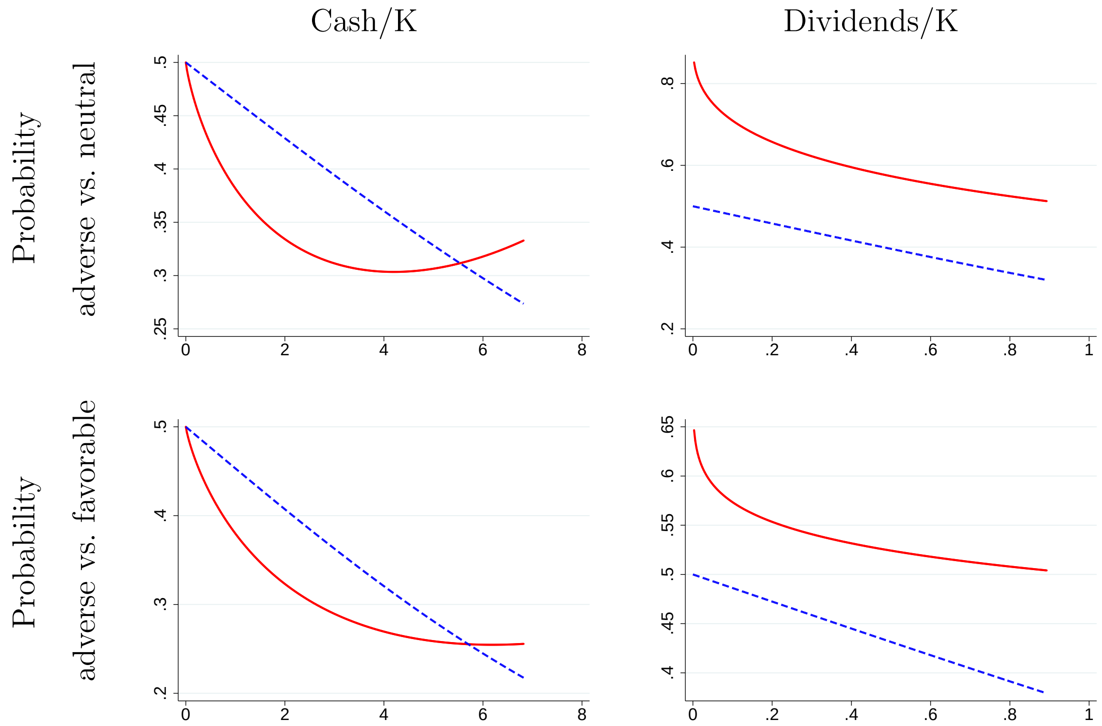
Figure 2.3 shows the shapes of the non-linear transformations that were found to maximize the deviance of the outlier adjusted model reported in column three of Table 2.4. For low cash holdings, the non-linear transformation reveals a particularly strong negative association with a firm's probability to report adverse financing conditions, while the slope becomes flatter for cash holdings that considerably exceed the value of tangible assets. For the extensively high cash holdings, exceeding four times the value of tangible assets, the probability to report adverse rather than neutral financing conditions even tends to increase slightly. This pattern likely reflects the endogeneity of financial constraints and the tendency of firms to accumulate cash out of their cash flows as documented in the literature (see, for instance, Almeida, Campello, and Weisbach 2004). A somewhat similar pattern is found for firms' dividend payments. Here, the negative association between financial constraints and dividend payments is particularly strong for firms paying low or even zero dividends, while the relationship becomes increasingly flat for higher values of the dividend payout ratio.

Comparing coefficient estimates between the linear and the non-linear specifications presented in Tables 2.3 and 2.4, we find that significance levels improve dramatically if deviating from the linearity assumption. Indeed, all of the KZ indicator variables reported in the top panel of Table 2.4 are significant and agree in sign with the original KZ index. However, considering the model separation of the adverse and the favorable financing condition categories (bottom panel), only cash flows and cash holdings are significant throughout, while dividends and leverage are not significantly different across categories. Instead, the separation seems to be rather driven by the economic cycle as indicated by the strongly significant sales and profit assessments.

Moreover, we assess the accuracy of our models to discriminate between financially constrained and unconstrained firm-year observations. As a global performance measure we calculate the area under the receiver operating characteristic (ROC) curve (Bamber 1975). In the context of our analysis, this area can be interpreted as the probability that the logit of a randomly selected constrained firm will be greater than that of a randomly selected unconstrained one. Consequently, values larger than .5 indicate a classification performance that is better than a random classification. For our models, we find the area

[11]The fractional polynomials reported in Table 2.4 read as follows: $Cash/K(1) = X$, $Cash/K(2) = X * log(X)$, with $X = Cash/K + 2.3 * 10^{-10}$ for all three model specifications; $Dividends/K(1) = X^{-.5}$, $Dividends/K(2) = log(X)$, with $X = Dividends/K + .03$ in the first specification; and $Dividends/K(1) = log(X)$, $Dividends/K(2) = log(X)^2$, with $X = Dividends/K + .03$ in the second and the third specification.

Figure 2.3: Estimated probability functions based on linear and non-linear logit-indicator relationships



The graphs compare estimated probability functions based on linear vs. non-linear logit-indicator relationships for the two alternative dichotomous financing conditions indicators. Coefficient estimates are based on the third model of Table 2.3 and Table 2.4 respectively. Dashed lines represent estimated relationships according to models that are linear in the logit. Solid lines show estimated relationships based on non-linear fractional polynomials. Variables are winsorized for the upper tail of the distribution by 5%.

estimates to range from .72 to .83. In addition, although not reported, the area estimates for the KZ variables alone range from .60 to .67. All area estimates are statistically significant at the 1% significance level according to the standard errors suggested by Bamber (1975). Finally, we also correlate the original KZ index with the logits of each model. To ensure comparability, again we employ only the four continuous variables that are similarly comprised in the original KZ index as well as in our sample, excluding Tobin's q. The obtained correlation coefficients range from .62 to .87 and are highly statistically significant throughout.

In summary, our results are in support of the validity of the original KZ index, yet undermining the criticism expressed by Whited and Wu (2006) as well as by Hadlock and Pierce (2010). Whited and Wu's (2006) critique comprises two arguments. First, the KZ index builds on a narrow sample of 49 low dividend firms in the U.S. running from 1970 to 1984. Accordingly, the authors question the external validity of the index coefficients for larger firm samples and different time periods. Second, they employ the argument of Erickson and Whited (2000), who show that Tobin's q contains considerable measurement error. Accordingly, it may not adequately approximate for firms' investment opportunities and induce bias on the coefficient estimates of the remaining KZ indicator variables. Our results refuse this criticism. Employing a broader sample of German manufacturing firms running from 1989 to 2009, we document three results in support of the validity of the KZ index. First, our coefficient estimates of the KZ indicator variables are in line with those of the original index. Second, the original index is highly correlated with the model predictions, we derive on a similar set of indicators. Third, we find the original KZ index to successfully discriminate between financially constrained and unconstrained firms in our sample. Finally, our evidence does not rely on market based measures of average Tobin's q. Instead, we employ firms' own assessments of their current and expected sales as well as of the profitability of potential investment projects.

Furthermore, we provide evidence against the critique raised by Hadlock and Pierce (2010) and shed some light on the potential sources of their contradicting results. In contrast to the original KZ index, Hadlock and Pierce (2010) find no evidence for a significantly negative relationship of dividend payments and financial constraints. In addition, they find a significantly positive relationship for cash holdings, while that variable loads negatively on the KZ index. Exploring non-linearity in the relationship of dividends and cash to the logit of our qualitative financial constraints indicator, we find a convincing explanation for the contradicting results. First, we show that the association of dividend payments and cash holdings with our indicator is strongly negative for small values and becomes almost flat for larger ones. In the case for cash, we even find evidence for a non-monotonic association. This pattern can explain why Lamont, Polk, and Saa-Requejo (2001) report a significantly negative coefficient for dividends in their sample of low dividend paying firms, while Hadlock and Pierce (2010) do not find this association. Moreover, we find dividend and cash variables have a correlation of .4 in our sample. Given this high correlation and the documented non-linear associations, we cannot rule out the possibility that the positive coefficients on cash holdings reported by Hadlock and Pierce (2010) are a result of misspecification.

2.5.2 The WW Index

Whited and Wu (2006) propose an alternative index of financial constraints exploiting an Euler equation approach from a structural model of investment. The WW index also builds on the Compustat universe comprising the 1975 to 2001 period and employs six variables. It loads positively on leverage, in terms of *Long-term debt/assets* and *Industry sales growth*, and negatively on *Cash flow/assets*, a *Dividend dummy*, indicating non-zero dividend payments, firm size in terms of *Log(assets)*, and on firms' individual *Sales growth*. Note that compared to the KZ index, Whited and Wu (2006) define leverage and cash flow slightly differently. Moreover, they report the WW index to be virtually uncorrelated with the KZ index.

We begin the analysis of the WW index by taking a look back at the plots evaluating the bivariate relationships of the explanatory variables and the logits of the qualitative financial constraints assessments as comprised by Figures 2.1 and 2.2. In the bi-variate regressions, we find the slopes to be largely in line with the loadings of the WW index.[12] However, in one case the sign is reversed. Industry sales growth loads positively on the WW index, but it shows a negative correlation with the logit of our financial constraints measures. Whited and Wu (2006) argue that industry sales growth captures firms' investment opportunities more reliably than Tobin's q. Furthermore, they expect financially constrained firms to belong to high-growth industries but to have low sales growth themselves.

Tables 2.5 and 2.6 report the results of the multivariate analysis of the WW index for the linear and the non-linear case respectively.[13] In line with the original index, we find low cash flow, high leverage, and small firm size to be significantly associated with higher probabilities to report financial constraints. Yet, the dividend dummy and individual sales growth are only significant for the regressions discriminating between adverse and favorable financing conditions, but not for separating adverse from neutral ones. Most importantly, however, we find higher values of industry sales growth to be associated with lower probabilities. The respective coefficients are negative and significant throughout. Moreover, unreported results show that both sales growth indicators, the industry-wide as well as the individual one, turn insignificant once we incorporate the sales and profit

[12]The dummy variable indicating positive dividend payments is not comprised in Figures 2.1 and 2.2. Yet, based on a logistic regression model incorporating the dummy and a constant as explanatory variables, a strong negative association is found.

[13]The fractional polynomials reported in Table 2.6 read as follows: $Long-term\ debt/assets$ (1) $= X^{.5}$, $Long-term\ debt/assets$ (2) $= X$, with $X = Long-term\ debt/assets + 9.9 * 10^{-13}$ for model specifications one and three; and $Long-term\ debt/assets$ (1) $= log(X)$, $Long-term\ debt/assets$ (2) $= log(X)^2$, with $X = Long-term\ debt/assets + 9.9 * 10^{-13}$ in the second specification.

Table 2.5: Predicting financing conditions with WW indicators (linear)

	Linear			Linear Sales growth > 0			Linear Winsorize at 5%		
Equation (1): Adverse vs. neutral									
Cash flow/assets	-3.878	***	(0.655)	-3.984	***	(0.910)	-4.420	***	(0.773)
Dividend dummy	0.007		(0.112)	0.034		(0.152)	-0.002		(0.112)
Long-term debt/assets	2.332	***	(0.371)	2.371	***	(0.518)	2.721	***	(0.416)
Log(assets)	-0.147	***	(0.031)	-0.136	***	(0.043)	-0.161	***	(0.034)
Industry sales growth	-1.635	***	(0.544)	-1.932	**	(0.818)	-1.838	***	(0.633)
Sales growth	-0.306		(0.273)	-0.373		(0.457)	-0.447		(0.357)
Equation (2): Adverse vs. favorable									
Cash flow/assets	-4.711	***	(0.869)	-4.506	***	(1.136)	-5.254	***	(1.020)
Dividend dummy	-0.323	**	(0.148)	-0.308		(0.191)	-0.313	**	(0.148)
Long-term debt/assets	0.475		(0.493)	0.382		(0.645)	0.652		(0.558)
Log(assets)	0.016		(0.043)	0.053		(0.057)	0.009		(0.046)
Industry sales growth	-2.168	***	(0.742)	-1.200		(1.043)	-2.498	***	(0.878)
Sales growth	-0.728	**	(0.364)	-0.543		(0.567)	-1.114	**	(0.486)
N	2,961			1,690			2,961		
Log likelihood	-2,526			-1,439			-2,524		
Pseudo R^2	0.037			0.031			0.038		
AUROC Equ. (1)	0.653			0.641			0.655		
AUROC Equ. (2)	0.664			0.622			0.665		
Corr. WW with Equ. (1)	0.592			0.504			0.581		
Corr. WW with Equ. (2)	0.311			0.168			0.327		

Coefficient estimates are presented together with asymptotic standard errors, which are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. AUROC refers to the area under the receiver operating characteristic curve.

indicators that we used to capture investment opportunities in the models evaluating the KZ index. This result casts doubt on the interpretation Whited and Wu (2006) give to this variable as well as on its capability to adequately approximate for firms investment opportunities.

Our findings are in line with those reported by Hadlock and Pierce (2010), who characterize their empirical support of the WW index as mixed. Indeed, employing the six WW index variables individually and together in an ordered logistic regression model predicting qualitatively assessed financial constraints, the authors find the same three variables to have significant coefficients and to agree in sign with the WW index. Given that two of these variables, cash flow and leverage, are essentially the same as in the KZ index, the authors claim that the WW index has probably little advantage over the KZ index. The

Table 2.6: Predicting financing conditions with WW indicators (non-linear)

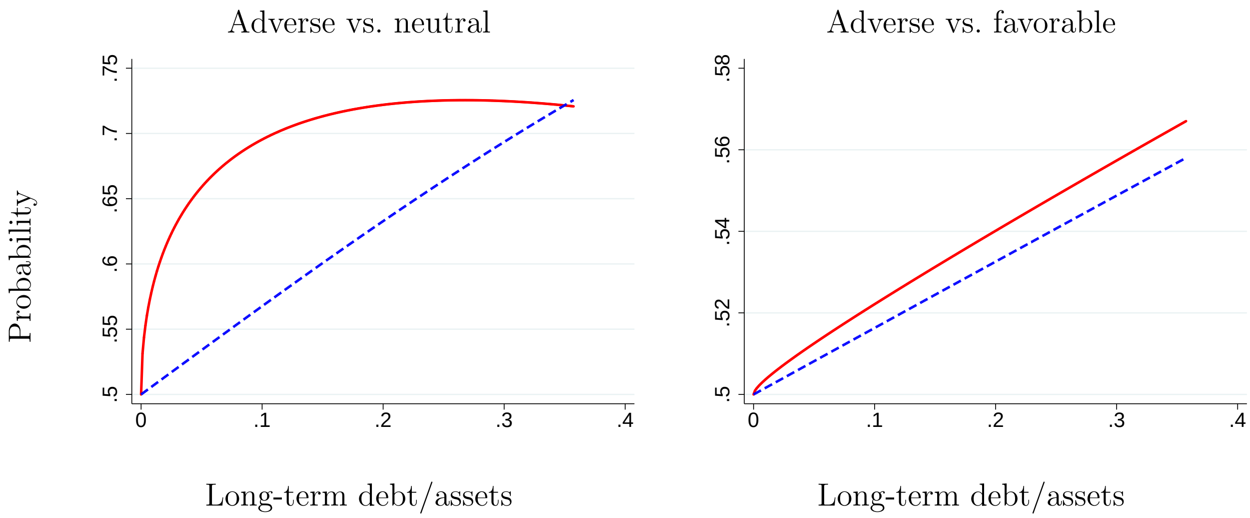
	Linear			Linear Sales growth > 0			Linear Winsorize at 5%		
Equation (1): Adverse vs. neutral									
Cash flow/assets	-3.972	***	(0.663)	-4.092	***	(0.920)	-4.517	***	(0.780)
Dividend dummy	-0.099		(0.114)	-0.073		(0.152)	-0.098		(0.114)
Long-term debt/assets (1)	3.709	***	(0.742)	0.151	***	(0.046)	3.759	***	(0.838)
Long-term debt/assets (2)	-3.446	***	(1.203)	0.004	**	(0.002)	-3.634	**	(1.462)
Log(assets)	-0.157	***	(0.031)	-0.156	***	(0.044)	-0.174	***	(0.034)
Industry sales growth	-1.625	***	(0.548)	-1.903	**	(0.824)	-1.835	***	(0.637)
Sales growth	-0.264		(0.275)	-0.259		(0.460)	-0.400		(0.360)
Equation (2): Adverse vs. favorable									
Cash flow/assets	-4.824	***	(0.878)	-4.614	***	(1.147)	-5.351	***	(1.028)
Dividend dummy	-0.332	**	(0.150)	-0.320	*	(0.191)	-0.312	**	(0.150)
Long-term debt/assets (1)	0.525		(1.013)	0.021		(0.060)	0.088		(1.143)
Long-term debt/assets (2)	-0.224		(1.643)	0.001		(0.002)	0.608		(1.992)
Log(assets)	0.013		(0.043)	0.052		(0.058)	0.008		(0.046)
Industry sales growth	-2.191	***	(0.746)	-1.186		(1.050)	-2.512	***	(0.881)
Sales growth	-0.721	**	(0.367)	-0.530		(0.569)	-1.104	**	(0.489)
N	2,961			1690			2961		
Log likelihood	-2510			-1429			-2510		
Pseudo R^2	0.044			0.037			0.044		
AUROC Equ. (1)	0.665			0.653			0.666		
AUROC Equ. (2)	0.665			0.621			0.665		
Corr. WW with Equ. (1)	0.542			0.447			0.540		
Corr. WW with Equ. (2)	0.312			0.171			0.329		
Delta deviance (linear)	33.022			18.981			18.981		
P-value	0.000			0.001			0.000		

Coefficient estimates are presented together with asymptotic standard errors, which are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Deviance statistics are compared to the linear specifications in Table 2.6 by referring to the χ^2 distribution (4 d.o.f.).

only new variable that they find to offer marginal additional explanatory power is firm size, with smaller firms being more likely to be constrained.

Again, we assess the accuracy of our models based on the WW index variables to discriminate between financially constrained and unconstrained firm-year observations. According to the area under the ROC curve estimates, ranging from .62 to .67, all models still significantly outperform a random classification. However, area estimates as well as the pseudo R^2 statistics are considerably smaller compared to those reported for the KZ index variables. In addition, we also find the correlation of our model predictions with the original WW index to be rather low, ranging from .17 to .59. In particular, the predictions obtained from the regression models discriminating between adverse and favorable financing conditions are found to be poorly approximated by the original index. Although not reported here, we also compute the area under the ROC curve measure employing the original WW index to classify adverse and favorable financing conditions. Indeed, our results reveal that in this case the original WW index is not significantly better than flipping a coin. Given that we found adverse and favorable financing conditions to be adequately separated based on firms self assessed sales and profit expectations, our doubts concerning the adequate approximation of investment opportunities through industry wide and individual sales growth measures is substantiated.

Figure 2.4: Estimated probability functions based on linear and non-linear logit-indicator relationships



The graphs compare estimated probability functions based on linear vs. non-linear logit-indicator relationships for the two alternative dichotomous financing conditions indicators. Coefficient estimates are based on the third model of Table 2.5 and Table 2.6 respectively. Dashed lines represent estimated relationships according to models that are linear in the logit. Solid lines show estimated relationships based on non-linear fractional polynomials. Leverage is winsorized at both tails of the distribution by 5%.

Finally, Figure 2.4 shows the shape of the non-linear transformation of leverage that is found to significantly increase the deviance compared to the linear model specification. For low leverage values, we find a particularly strong positive association with a firm's probability to report adverse financing conditions. Yet, with increasing leverage the slope of the curve becomes flatter. In particular, if a firm's long-term debt to assets ratio exceeds the value of .25, we not longer find an association. This pattern likely reflects the endogenous relationship of financial constraints and leverage as documented in the literature (see, for instance, Acharya, Almeida, and Campello 2007, Hennessy and Whited 2007, and Almeida and Campello 2007). However, this finding only applies to the probability to report adverse rather than neutral financing conditions. For discriminating between adverse and favorable financing conditions, we neither find a linear nor a non-linear variable transformation of leverage to yield significant results.

2.5.3 The SA Index

Based on their critique on the KZ index and their inconclusive results for the WW index, Hadlock and Pierce (2010) advocate for a conservative approach to measure financial constraints. Specifically, they propose an alternative index solely relying on firms' size and age since they claim that these are more exogenous than the surveyed alternatives. Analyzing the relationship of the two indicators with their qualitative financial constraints measure, the authors find strong evidence for non-linearity. In particular, they find slopes to become flatter after exceeding a certain threshold and low order polynomials not to appropriately approximate for the relationship for high values of the explanatory variables. Accordingly, they decide to winsorize both variables at a certain threshold, namely total assets at 4.5 billion year 2004 inflation adjusted dollars, which is approximately the 95% percentile of the distribution, and age at 37 years. In addition, they employ an order two polynomial of total assets by taking its natural logarithm to the power of one as well as to the power two.[14]

In order to assess the ability of the SA index to classify financially constrained and unconstrained firms in our data set, we employ Hadlock & Pierce's (2010) variable transformations accordingly. Minimizing the bias arising from comparisons of firms total assets across jurisdictions, currencies and time, we also winsorize the firms total assets at the 95% percentile of the distribution, which is at 2.7 billion year 2005 inflation adjusted euros. Moreover, Hadlock and Pierce (2010) define the firm age in their sample as the current year minus the first year that the firm has a non-missing stock price on Compustat. In the absence of any comparable information, we define firm age as the observation year minus the year of a firm's incorporation. We admit that this difference possibly drives our results. Given this constraint, we interpret this exercise as a test of the external validity of the SA

[14] According to Hadlock and Pierce (2010), the SA index is calculated as follows: - 0.737 Log(Min(total assets,4.5 billion)) + 0.043 Log(Min(total assets,4.5 billion))^2 - 0.040 Min(Age,37).

Table 2.7: Predicting financing conditions with size and age

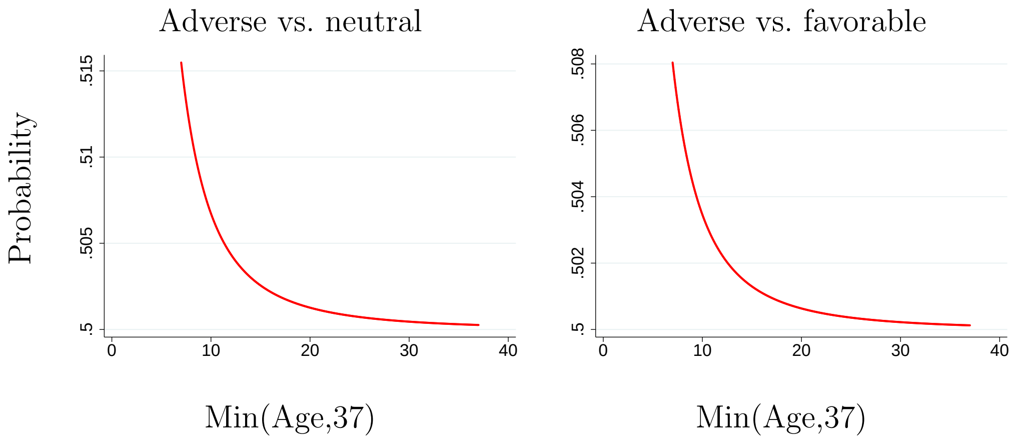
	Original index Hadlock and Pierce transformation			Best non-linear fit Hadlock and Pierce transformation			Best non-linear fit Winsorize at 5%		
Equation (1): Adverse vs. neutral									
Log(assets)	-0.548		(0.616)	-0.173	***	(0.030)	-0.181	***	(0.030)
Log(assets)^2	0.010		(0.017)						
Age37	0.001		(0.006)						
Sqrt(Age37)				4.913	***	(1.604)			
Sqrt(Age37)*log(Age37)				-0.965	***	(0.315)			
Age							0.001		(0.001)
Equation (2): Adverse vs. favorable									
Log(assets)	-0.826		(0.847)	-0.035		(0.041)	-0.037		(0.041)
Log(assets)^2	0.021		(0.023)						
Age37	-0.012		(0.009)						
Sqrt(Age37)				2.621		(2.282)			
Sqrt(Age37)*log(Age37)				-0.537		(0.448)			
Age							-0.001		(0.001)
N	2,961			2,961			2,961		
Log likelihood	-2,601			-2,596			-2,600		
Pseudo R^2	0.009			0.011			0.009		
AUROC Equ. (1)	0.575			0.579			0.574		
AUROC Equ. (2)	0.514			0.517			0.515		
Corr. SA with Equ. (1)	-0.967			-0.873			-0.978		
Corr. SA with Equ. (2)	-0.346			-0.355			-0.721		
AUROC SA Equ. (1)	0.426			0.426			0.426		
AUROC SA Equ. (2)	0.492			0.492			0.492		

Coefficient estimates are presented together with asymptotic standard errors, which are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Age37 is defined as min(Age,37). AUROC refers to the area under the receiver operating characteristic curve. For both regression equations, areas are computed for the presented model estimates as well as for the original SA index.

index on samples of non-listed firms which rely on the real firm age instead of the time period a firm is listed on a stock exchange.

We begin our analysis by re-estimating the SA index on our sample applying the original variable transformations. According to our results, which are reported in the first column of Table 2.7, all coefficients are insignificant and the coefficient of the age variable, winsorized at 37 years, even shows the wrong sign. In addition, the correlation of the model prediction with the original index is highly negative and the model fit is poor. We proceed by searching for the best non-linear model fit applying fractional polynomials. The results, reported in

Figure 2.5:
Estimated probability functions for non-linear logit-indicator relationships



The graphs compare estimated probability functions based on estimates of the non-linear logit-indicator relationship for the two alternative dichotomous financing conditions indicators. Coefficient estimates are based on the second model specification presented in Table 2.7. Age is winsorized at 37 years.

column 2, reveal that the index variables significantly discriminate between adverse and neutral financing conditions reported in our sample, however, the non-linear transformation applied by the original index is refused by the data. Instead, the logit of the probability to report adverse financing conditions appears to be linear in the logarithm of total assets but non-linear in age. More specifically, we find the association of firm age and financial constraints to be significantly stronger for young rather than for more mature firms (see Figure 2.5). Finally, column three presents a robustness check with age not winsorized at 37 years but at both tails of the distribution by 5%. According to the results, firms size in terms of the logarithm of total assets significantly separates firms facing adverse from those facing neutral financing conditions. For age, however, no significant association is found.

In summary, we find little support for the SA index. In particular, the original SA index loadings and variable transformations are refused for our sample. In addition, the original index shows a high negative correlation with our model predictions, and employing the area under the ROC curve as a global classification measure, the original index does not outperform a random classification procedure (see Table 2.7). Although we admit that the SA index performs well on the sample of listed firms studied by Hadlock and Pierce (2010), we caution researchers to apply the index in order to identify financially constrained firms in deviating samples.

2.6 Conclusion

The reliable measurement of financial constraints is key for the analysis of financial-market imperfections as well as for the assessment of their impact on firms' investment decisions. Numerous measures have been proposed by the literature. However, there is considerable debate about their relative merits.

Adding to this debate, we evaluate the extensively used KZ index as well as two more recently proposed alternatives, the WW and the SA index. In the spirit of Kaplan and Zingales (1997), we derive a qualitative financial constraints indicator, building on firms' self-reported assessments of their financing conditions. Accordingly, we are not subject to the critique of Hadlock and Pierce (2010), who claim that the KZ index as well as most of the empirical literature studying financial constraints build heavily on inadequate *ex-ante* sorting criteria. Especially, if they comprise the same information as the explanatory variables employed for the subsequent analysis.

We follow Hadlock and Pierce's (2010) evaluation approach. In particular, we explain firms' qualitative assessments of their financing conditions by the quantitative variables employed by the three indices. In order to infer on their validity to measure financial constraints, we evaluate the signs and the significance levels of the regression coefficients across different model specifications as well as the classification accuracy of the original indices. Finally, we study the sensitivity of our estimates with respect to non-linear variable transformations based on fractional polynomials.

Despite warranted criticism of the KZ index, we find strong evidence in support of its reliability. Our evidence is particularly striking given the substantial differences across samples. Indeed, Kaplan and Zingales (1997) study a narrow sample of 49 listed and low dividend paying U.S. firms operating from 1970 to 1984. Accordingly, the stability of their parameter estimates across samples and over time has been repeatedly questioned (see, for instance, Whited and Wu 2006 and Musso and Schiavo 2008). Employing a large sample of German manufacturing firms running from 1989 to 2009, our results are thus particularly supportive for the index's external validity. In addition, our sample enables implicit inference on the sensitivity of the KZ index to potentially considerable measurement error comprised in Tobin's q (Erickson and Whited 2000). In contrast to the original index, we utilize survey-based assessments of firms' sales expectations as well as of the profitability of the investment projects they face in order to control for firms' investment opportunities. However, we find the coefficients of the remaining indicator variables, comprised in the KZ index, to be in line with our results as well as our model predictions to highly correlate with the original index.

For the recently proposed WW and SA index, however, our results are less supportive. Although we find the WW index, proposed by Whited and Wu (2006), to significantly outperform a random classification scheme, our coefficient estimates for the comprised indicators are not in line with the original index. In particular, for the industry sales growth variable, which loads positively on the index, we find a significant negative association with our survey based financial constraints indicator. Yet, Whited and Wu (2006) employ the variable in order to capture investment opportunities (high industry sales growth) which are supposed to be positively correlated with (binding) financial constraints.

Evaluating the SA index, suggested by Hadlock and Pierce (2010), we reject the hypothesis of external validity. In particular, we find the classification performance of the original SA index to be comparable to flipping a coin. This result is surprising, given that the authors claim the index to be a reasonable choice for measuring financial constraints in many contexts after having extensively studied its robustness and out of sample performance.

Finally, we find individual coefficient estimates, and thus, the inference on the validity of certain indicator variables to be sensitive to the linearity assumption of the indicator-logit relationship commonly made in applied empirical work. In particular, for firms' cash flows, dividend payouts, and leverage ratios the association with financial constraints seems to be particularly pronounced for smaller values. However, for increasing indicator values associations tend to become vague.

Chapter 3

Estimating real effects of restrictive bank lending: bias from firms' current states and future prospects

Based on panel data for the German manufacturing sector observed between 2003 and 2011, we find firms to show considerably lower employment growth one year after becoming subject to bank lending restrictions. Ruling out demand-side factors by matching either on firms' balance sheets or additionally on monthly survey-based assessments of firms' current states and future prospects yields significant results for the former case but insignificant ones for the latter. Indeed, we provide evidence that if matching on balance sheet information only, significant bias remains. Based on our results, we challenge the focus of the related empirical literature on backward-looking balance sheet information in order to infer on the effects of supply-driven changes in bank lending restrictions.

3.1 Introduction

The recent financial crisis is often referred to as an example of the impact of a bank lending supply shock on real economic activity. According to this narrative, banks came under distress and reduced their lending to non-financial firms, which caused reduced investment activity and slowed employment growth (see, for instance, Brunnermeier (2009), Shleifer and Vishny (2010)). However, restrictive bank lending could also have been a reflection of deteriorating firm characteristics and business prospects that actually caused the slowdown in economic activity. Indeed, Kahle and Stulz (2013) find evidence questioning the common view that a credit supply shock was a dominant causal factor for financial and investment policies of U.S. firms during the crisis. In order to disentangle supply-side and demand-side causes of credit contractions, to sufficiently control for firm-side factors is key. Previous literature on this topic controls for firm heterogeneity by primarily using balance sheet variables, which are mostly backward-looking in nature. This ignores that credit supply is also determined by contemporaneous and forward-looking information about a firm, which may not be sufficiently captured by its balance sheet. In this study, we address the question of whether controlling for variables capturing firms' current states and future prospects affects the estimation of real effects of bank lending supply.

Based on a variety of matching estimators, we identify economically and statistically significant effects of restrictive bank lending on firm-level employment growth when matching on balance sheet variables only. However, analyzing the quality of these matching estimators, we find that contemporaneous and forward-looking firm characteristics are insufficiently balanced. Furthermore, controlling in addition for survey-based assessments of firms' current business situations and future expectations significantly lowers the estimated effects, which even turn insignificant. From these results, we draw the conclusion that the empirical literature assessing the real effects of bank lending restrictions should develop sufficient approaches to rule out firm heterogeneity that is not captured by balance sheet data in order to avoid overestimation of credit supply-side effects. Although less stressable, our results also question the importance of a bank lending supply shock for the pronounced economic downturn in Germany during the financial crisis.

Our findings are in line with an increasing literature regarding the consequences of mis-measured fundamentals for the estimation of investment-cash flow sensitivities. While earlier empirical research provides evidence of higher investment-cash flow sensitivities for financially constrained firms, Erickson and Whited (2000) show that most of the stylized facts produced by investment-cash flow regressions are artifacts of measurement error in marginal q. Subsequent research by Cummins, Hassett, and Oliner (2006) sheds light on this measurement error. Employing earnings forecasts from security analysts to control for fundamentals, the authors find cash flow to be uncorrelated with investment for firms that were classified as being liquidity constrained in previous studies relying on Brainard-Tobin average q. This paper shows that analog error in the measurement of firms fundamentals, specifically firms' current states and future prospects, biases the estimates of firms' sensitivity to supply-driven bank lending restrictions.

Our analysis is based on a sample of German manufacturing firms from the "EBDC

Business Expectations Panel" observed between 2003 and 2011. This data set links the monthly Ifo Business survey to balance sheet information. The survey provides panel data on firms' perceptions of bank lending supply, from which we derive a treatment variable indicating that a firm has experienced restrictive bank lending. The survey data also contain year-on-year employment growth rates at the firm-level that are rarely covered by comparable balance sheet data sets. When estimating the effect of restrictive bank lending on employment growth, we rule out firm heterogeneity using a combination of balance sheet data and firms' monthly appraisals of their current business situations and future expectations, which they reveal in the Ifo Business Survey.

Consequently, our main contribution to the existing empirical literature stems from these unique data. First, we use a direct survey-based measure of bank lending restrictions. Second, we provide quantitative estimates of the effect of bank lending restrictions on firm-level employment stocks. Third, we analyze the sensitivity of our estimates with respect to conditioning on rich and timely data on firms' own assessments of their current state and future prospects when the restriction occurs. More specifically, the high frequent panel structure of our data set allows us to identify the timing of a firm's experience of restrictive bank lending on a monthly basis, whereas previous studies are limited to annual data.

This paper is structured as follows: Section 3.2 provides a brief overview of the literature concerning the impact of credit supply-side shocks on real economic activity, specifically focusing on comparable firm-level studies and the control variables they use. The empirical strategy is laid out in Section 3.3, while Section 3.4 describes the data set, defines the treatment, and provides descriptive statistics. Section 3.5 presents results for least squares estimation and for a variety of matching estimators as well as sensitivity analysis with respect to the set of covariates. Section 3.6 lays out robustness exercises and Section 3.7 discusses the role of the financial crisis. Finally, Section 3.8 concludes.

3.2 Literature

Using macroeconomic data, Bernanke (1983) was the first to present empirical evidence for the impact of the collapse of the financial system on borrowers, and therefore the economy as a whole, during the Great Depression from 1930 to 1933. Theoretical models introducing 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 more recently by Gertler and Kiyotaki (2010).

In the empirical literature, credit supply-side effects on real economic activity were estimated based on different approaches – either with a macro or micro focus. Using macro-data, empirical studies by Peek and Rosengren (2000) and Chava and Purnanandam (2011) identify exogenous shocks to banking systems and assess their real economic effects. Kroszner, Laeven, and Klingebiel (2007) and Dell'Ariccia, Detragiache, and Rajan (2008) follow a similar approach to Rajan and Zingales (1998), showing that banking crises adversely affect growth at the sector-level.

Another strand of research that is closer to the approach followed in this paper tries to identify the effects of restricted credit supply at the firm-level. In order to identify bank-side effects, most studies either rely on a set of firm characteristics to rule out firm heterogeneity or use exogenous sources if there is a variation in credit supply.[1] Khwaja and Mian (2008) analyze the impact of liquidity shocks on banks in Pakistan and on the default probability of firms that simultaneously borrow from differently impaired banks. The few firm characteristics covered comprise size, location, and political connectedness. However, they draw their main conclusions from within-firm comparisons, thereby controlling for firm-specific changes in credit demand. For Japan, Gan (2007) finds that a firm's investment depends on the real estate exposure of its top lender during the collapse of the Japanese land market in the early 1990s. He controls for firms' credit demand and creditworthiness as well as for their selection of the lending bank using firms' Tobin's q, cash flow, cash stock, land holdings, recent land purchases, and leverage.

Focusing on the financial crisis, Duchin, Ozbas, and Sensoy (2010) use the crisis as a negative supply-side shock and analyze its effect on firms' investments. They control for firms' Tobin's q, cash flow, cash holdings, and debt. Following a different approach, Almeida, Campello, Laranjeira, and Weisbenner (2012) compare investments of firms with debt maturing just before the outbreak of the financial crisis to investments of firms without maturing debt by using a matching approach. They use the same variables as Duchin, Ozbas, and Sensoy (2010) and further match on industry and rating categories. Chodorow-Reich (2013) constructs a data set of bank relationships for a sample of U.S. firms and finds a significant impact of bank health on employment for small and medium-sized firms, but not for large ones. He controls for firms' current borrowing pattern, firm size, age, and access to public bond markets.

Finally, an increasing body of literature analyzes how bank-side factors affect loan de-

[1] See also an earlier review of existing research strategies with a comprehensive list of classification indicators by Musso and Schiavo (2008)

cisions or loan rates (e.g. Jiménez, Ongena, Peydró, and Saurina (2012), Popov and Udell (2012), Santos (2011), and Ashcraft (2006)). Although they do not consider firms' behavioral responses to credit restrictions, their inference might well be sensitive to the incorporation of indicators of firms' current business situations and future expectations as additional firm-side determinants of loan prices and volumes apart from balance sheet data and credit ratings.

This review of the empirical literature shows its reliance on balance sheet data to control for firm-level heterogeneity. Primarily due to data limitations, complementary indicators of firms' current business situations or future expectations are not considered. However, there is indeed one study that does so and is thus close to this work. Campello, Graham, and Harvey (2010) use survey data to analyze the impact of credit constraints on planned employment cuts and capital expenditures, among other variables. At first, they control for firm heterogeneity using size, ownership, industry, and credit rating categories. In addition, they show that the effects of credit constraints on capital expenditures and employment turn out slightly lower, but still significant, when contemporaneous and forward-looking variables are also included in the matching procedure. More specifically, they consider three dichotomous contemporaneous and forward-looking variables: firm's self-predicted profitability status for 2008, a self-predicted dividend payout status for the same year, and a self-assessment of the firms' long-term growth prospects. However, Campello, Graham, and Harvey (2010) rely on cross-sectional data obtained in the fourth quarter of 2008, right after the bankruptcy of Lehman. Therefore, apart from other differences, their results may not be comparable to those obtained from the panel data utilized in this study for two reasons. First, to rule out endogeneity we match on firm-level current states and future prospects that are observed right before the bank lending restriction initially occurs. Second, we analyze the impact on actually realized employment figures after one year, while Campello, Graham, and Harvey (2010) study the impact on investment and employment plans at the turmoil of the crisis, when uncertainty was at its peak.

3.3 Empirical strategy

To identify the effects of bank lending restrictions on firm-level employment growth, we estimate a treatment effects model using a matching estimator. For every firm in our panel data set, we observe whether a firm i is experiencing restrictive bank lending in month t $(Restricted_{i,t})$ and we measure post-treatment year-on-year employment growth one year later *($\Delta Empl_{i,t+12}$)*. If bank lending restrictions were randomly assigned to firms, observed post-treatment differences in employment growth between restricted and unrestricted firms could be interpreted as effects of bank-side factors. However, the assignment depends on firms' credit demand and creditworthiness, which also affect the observed difference in employment growth. The direction of the resulting bias is ambiguous, however. On the one hand, the effects of bank-side factors could be overstated because firms in dire straits (e.g. due to a product demand shock) are more likely to experience restrictive bank lending and are also likely to have lower employment growth rates. On the other hand, the difference could also understate the effects of bank-side factors if firms with large growth potentials were more likely to experience restrictive bank lending due to risk or asymmetric information (e.g. young, innovative firms or SMEs).

Therefore, the identification of effects of bank-side factors requires controlling for firm heterogeneity. We do so by setting up a quasi-experimental setting using a matching estimator.[2] More precisely, we compare restricted firms to matching firms that are unrestricted, but otherwise similar to the restricted firms in terms of characteristics predicting the experience of restrictive bank lending. The treatment can then be considered randomly assigned conditional on the characteristics on which firms are matched and the average difference in employment growth rates between restricted firms and unrestricted matching firms provides an estimate of the real effects of bank lending supply. Compared to the standard ordinary least squares approach, the semi-parametric matching approach applied in this study relies on less restrictive identifying assumptions. More specifically, we balance the distribution of covariates, $X_{i,t-1}$, between restricted and unrestricted firms and assure a common support. This reduces the risk of misspecification of the functional form of *($E[\Delta Empl_{i,t+12}|X_{i,t-1}]$)*including the control for observably impact heterogeneity and avoids counter factual comparisons based on extrapolations outside the common support of the covariates' distributions.

We combine exact matching and propensity score matching (Rosenbaum and Rubin, 1983) because the large number of firm characteristics, including continuous variables, inhibits the identification of matching firms that are identical with respect to all characteristics as required by the approach based on exact matching. The propensity score is defined as the predicted probability of experiencing restrictive bank lending given a set of pre-treatment firm characteristics, $X_{i,t-1}$:

[2]This approach is also used in other studies (e.g. Campello, Graham, and Harvey (2010), Almeida, Campello, Laranjeira, and Weisbenner (2012)).

$$p(X_{i,t-1}) = Pr(Restricted_{i,t} \mid X_{i,t-1}). \tag{3.1}$$

Assuming that all predictors of $Restricted_{i,t}$ are included in $X_{i,t-1}$, restrictive bank lending can be considered randomly assigned so that it is independent of the outcomes $(\Delta Empl_{0,i,t+12})$ and $(\Delta Empl_{1,i,t+12})$ given pre-treatment firm characteristics $X_{i,t-1}$ and

$$(\Delta Empl_{0,i,t+12}, \Delta Empl_{1,i,t+12}) \perp Restricted_{i,t} \mid p(X_{i,t}) \tag{3.2}$$

holds. Comparing each restricted firm to unrestricted ones with a similar propensity score thus provides an estimated treatment effect that is close to the one derived from an experimental setting (Dehejia and Wahba, 1999) in which restrictive bank lending is randomly assigned. The identifying assumption, however, hinges on the choice of the variables comprised in $X_{i,t-1}$. In this respect, our study provides a major contribution to the existing literature by considering a broader set of variables accounting for balance sheet information, as well as firms' current business situations and future expectations.

However, what drives the exogenous variation in the assignment of bank lending restrictions if firm characteristics are identical? There are two valid arguments for a setting in which firms differ in their availability of bank credit even though they are similar in other characteristics. First, Stiglitz and Weiss (1981) state that one symptom of credit rationing is the possibility that one firm is granted bank credit while the credit application of another similar firm is rejected. In Section 3.4.2 we show that a large number of firms receive the treatment of restrictive bank lending during the financial crisis. During this period, uncertainty about firms' business environment increased, severing asymmetric information between firms and banks, and potentially inducing randomly assigned bank lending restrictions given firm characteristics.

Second, heterogeneity in firms' bank relationships can explain differences in credit supply between otherwise similar firms. This is the case if one firm's relationship bank restricts lending, but the relationship bank of another similar firm does not, inducing a random assignment of restrictive bank lending. There is a body of literature in support of this argument showing that banks with different characteristics, such as size and capitalization, are differently inclined to transmit monetary policy changes to the real sector[3] and that banks differed in their lending behavior during the financial crisis[4]. This is complemented

[3]E.g. Kashyap and Stein (2000); Kishan and Opiela (2000); Gambacorta (2005); Kishan and Opiela (2006); Jiménez, Ongena, Peydró, and Saurina (2012).

[4]E.g. Albertazzi and Marchetti (2010); Ivashina and Scharfstein (2010); Puri, Rocholl, and Steffen

by the fact that if a firm experiences restrictive lending from one bank, simply addressing another one is not necessarily an option. As Sharpe (1990) shows, it can be costly to borrow from outside an existing bank relationship. Empirical evidence lends support to the argument that relationships to banks that were hit by the financial crisis had a negative impact on firms (Almeida, Campello, Laranjeira, and Weisbenner, 2012; Chodorow-Reich, 2013; Santos, 2011). On the contrary, it could of course be argued that a firm's choice of its relationship to a bank is not random, but driven by certain firm characteristics (see, for example, Hainz and Wiegand (2013)). However, the broad set of firm characteristics on which we match firms to randomize the assignment of restrictive bank lending should also help to randomize firms' choices of bank relationships.

3.4 Data

3.4.1 Databases

In the related literature the vast majority of approaches identifying real effects of bank lending restrictions rules out firm heterogeneity primarily by controlling for firm size, age and balance sheet data. Balance sheet variables provide accurate measures of a firm's financial condition, yet they are rather backward-looking and limited to hard information. In the determination of firms' credit demand as well as of banks' assessments of firms' creditworthiness, however, contemporaneous and forward-looking information (e.g. from order books, interim financial statements or business plans) might also be relevant. To approximate such information, we use data from the German "EBDC Business Expectations Panel", which offers panel data on firms' perception of bank lending supply, annual employment growth, and a broad set of firm characteristics including balance sheet information and survey-based appraisals of firms' current business situations and future expectations. The full set of variables used in our analysis is described in Table 3.1.

The "EBDC Business Expectations Panel" links firms' balance sheets from the Bureau van Dyk Amadeus database[5] and the Hoppenstedt database[6] to panel data from the Ifo

(2011).

[5]The Amadeus database contains balance sheet data and other firm-specific information for European firms and covers approximately one million mainly unlisted German firms. Its primary source for Germany is the Creditreform database.

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

Table 3.1: Variable descriptions

Variable	Description	Frequency
Treatment		
Restricted	Change in perception of bank lending from “accommodating” or “normal” to “restrictive”	Varying*
Size and growth		
Empl	Number of employees at company-level	Annual
Δ*Empl*	Year-on-year growth rate in number of employees	Annual
Growth	Positive year-on-year growth rate	Annual
Balance sheet data		
Equity ratio	Equity to total assets	Annual
ROA	Operating profit to total assets	Annual
Fixed assets	Fixed assets to total assets	Annual
Coverage ratio	Operating profit to interest expenses	Annual
Cash	Cash holdings to total assets	Annual
Current situation		
State (+)	Appraisal: Current business situation good	Monthly
State (=)	Appraisal: Current business situation satisfactory	Monthly
State (-)	Appraisal: Current business situation unsatisfactory	Monthly
Orders (+)	Appraisal: Stock of orders relatively high	Monthly
Orders (=)	Appraisal: Stock of orders satisfactory or enough	Monthly
Orders (-)	Appraisal: Stock of orders too small	Monthly
Short-time	Currently working short-time	Quarterly
Export	Firm is exporting	Quarterly
Future expectations		
State exp (+)	Expecting improvement of business over next 6 months	Monthly
State exp (=)	Expecting no change of business over next 6 months	Monthly
State exp (-)	Expecting worsening of business over next 6 months	Monthly
Empl exp (+)	Expecting increasing employment over next 3 months	Monthly
Empl exp (=)	Expecting no change in employment over next 3 months	Monthly
Empl exp (-)	Expecting decreasing employment over next 3 months	Monthly
Short-time exp	Expecting to work short-time during next 3 months	Quarterly
Headcount (+)	Too few employees for demand over next 12 months	Quarterly
Headcount (=)	Enough employees for demand over next 12 months	Quarterly
Headcount (-)	Too many employees for demand over next 12 months	Quarterly

Notes: * As of November 2008, the question regarding bank lending behavior is asked monthly, although only in March and August of each preceding year.

Business Survey. The Ifo Business Survey is a monthly survey which asks 3,600 plants from the German manufacturing sector for an appraisal of their current business situations and expectations for future business. It was launched in 1949 to provide the basis for the Ifo Business Climate Index, a timely measure of economic activity. For this purpose, the Ifo Institute continuously ensures that the panel of firms is representative of the German manufacturing sector.

When linking annual balance sheet and monthly survey data, which is done based on the name and postal address of the firms, we allow for alternative fiscal years. More specifically, fiscal years of some firms in our sample do not coincide with calendar years. Due to the monthly frequency of our data, we can allow for alternative fiscal years by linking the most recent balance sheet to monthly observations. We thereby assume that a firm's balance sheet for the preceding fiscal year is made available to banks immediately at the end of the fiscal year.

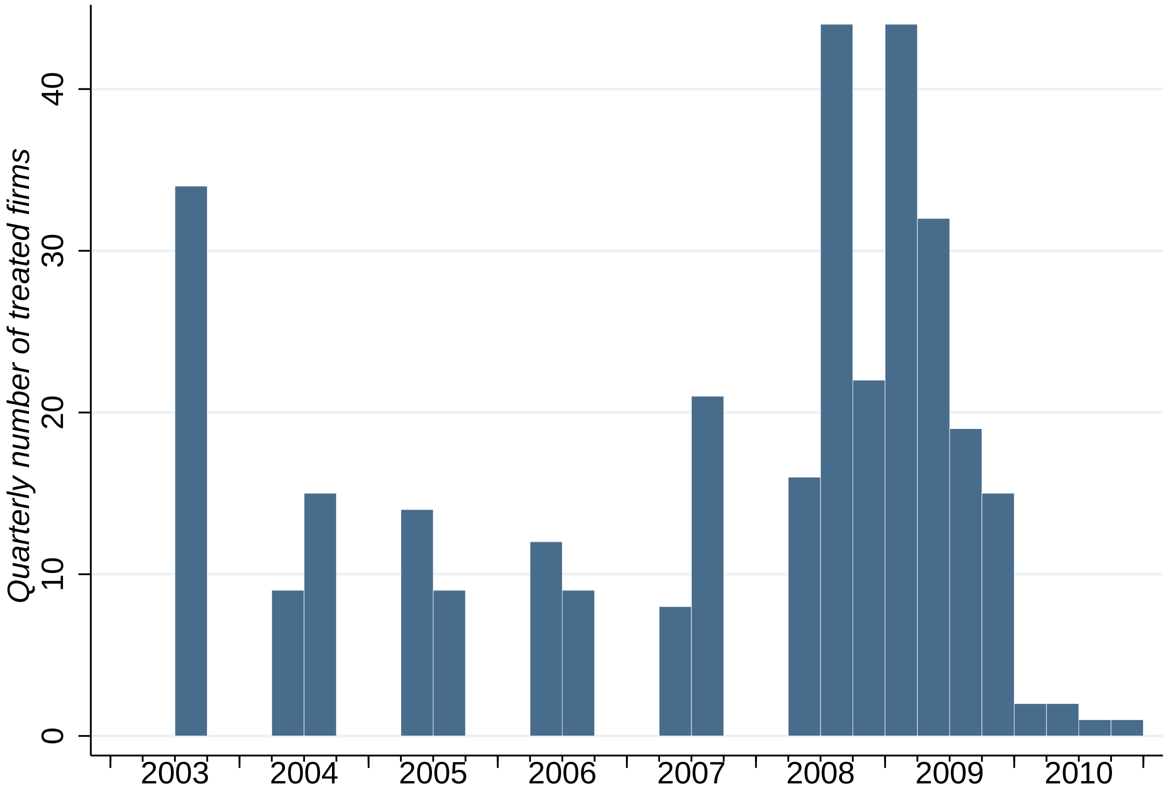
3.4.2 Treatment definition

Based on the panel structure of our data we derive both a firm and time-specific treatment variable indicating that a firm experiences a change in bank lending supply, which is not experienced by a control group of firms. In the Ifo Business Survey firms are asked how they perceive banks' willingness to lend to firms. Possible appraisal categories are "restrictive", "normal", and "accommodating". This enables us to measure a change in credit supply to a firm directly and to analyze the subsequent employment development.

Firms that we refer to as *restricted* in a certain month complete a transition from reporting "normal" or "accommodating" bank lending in one month, *t-1*, to reporting "restrictive" bank lending in the next month, *t*. A firm is defined as *unrestricted* in *t* when it reports "normal" or "accommodating" bank lending in *t-1* and does not switch to reporting "restrictive" bank lending in *t*. Furthermore, we rule out bias from potential previous treatments by requiring that both restricted and unrestricted firms have not reported "restrictive" bank lending in the previous twelve months. This conservative treatment definition is the basis for identifying the effects of restrictive bank lending, but comes at the cost of using only a fraction of the firms in the data set.

After conditioning on the availability of all control variables, $X_{i,t-1}$, our setup comprises a sample of 329 treated and 4,950 untreated potential matching firm-month observations. Figure 3.1 provides the distribution of treated observations over time. The number of treated firms increases sharply during the wake of the financial crisis.

Figure 3.1: Number of treated firms over time



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

For identification, we have to make an assumption about the exact timing of the treatment. As of November 2008, the question is asked on a monthly basis, which allows the exact specification of the treatment month t as the month in which the firm reports restrictive bank lending for the first time. For 2003 to 2008, however, the question is only asked twice a year, in March and August. If, for example, a firm reports normal or accommodating bank lending in March and restrictive bank lending in August, it remains unclear whether the shift occurred in August or at any time 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. This ensures that our control variables, which are drawn from *t-1*, are measured in a month during which the firm is definitely untreated and not already affected by bank lending restrictions. We could alternatively assume that the treatment occurs in the month during which the firm reports "restrictive" bank lending for the first time. Our empirical results, however, are not sensitive to the altering of this assumption.

Another pitfall of our measurement of bank lending restrictions is driven by the wording of the corresponding survey question. Here, a firm is asked for its perception of bank lending supply to firms in general and not to the surveyed firm in particular. Given that all of the other survey questions unambiguously refer to firm specific assessments, we have good reason to assume that firms do not take the question literally, but rather answer it in the context of the other questions, and thus provide firm-specific information. However, if firms are taking the question literally, differences in firms' answers at a given point in time should still reflect difference in firms' information sets. More specifically, we would expect firms to answer the question based on their private information regarding their own availability of bank credit as well as on public information (e.g. from the media or industry-peers). Given that our matching procedure directly matches within time and industry cells, the variation in firms' public information sets at a given point in time should be limited and show little structural bias. Nevertheless, firms might be misclassified. In consequence, the estimated employment effects of restrictive bank lending would be biased downwards. Therefore, our estimates should be considered to be conservative.

Taking account of the potential downward bias in the overall estimates, we focus the analysis on the difference between the estimated effects with and without matching on firms' current business situations and future expectations in addition to size, growth and balance sheet variables. Note that these differences should be unaffected by the potential misclassification. Therefore, potential errors in the identification of treated firms should not affect our main conclusion.

Turning to the consequences for the matching procedure, we reduce bias from potential misclassification by matching firms exactly within quarter and industry cells.[7] It is reasonable to assume that within quarter-industry cells, differences in firms' public information sets are negligible and thus differences in the perception-based treatment variable

[7]Please note that matching on month-industry cells turned out to be impossible due to the lack of a sufficient number of observations in such cells. In an unreported robustness check, we also directly match on months, while conditioning the propensity score on industry dummies. Our results are not sensitive to this change.

($Restricted_{i,t}$) actually reflect differences in the firms' own experience. Accordingly, differences in employment growth between restricted and unrestricted firms can be attributed to firms' own borrowing situation.

Matching firms exactly within quarters and industries further approaches a general problem of potential time and industry-dependence of firms' perceptions of banks' lending behavior. It could be the case that the definition of what firms consider as restrictive bank lending differs over time or across industries. Exact matching on quarter-industry cells leads to an estimator that is largely unaffected by this source of heterogeneity.

3.4.3 Control variables

To test whether matching on firms' current business situations and future expectations reduces selection bias and affects the estimated effects of changes in bank lending supply, we control for firm characteristics from both balance sheets and survey data. Table 3.2 compares summary statistics of pre-treatment control variables, $X_{i,t-1}$, separately for restricted firms and unrestricted potential matching firms (as defined in Section 3.4.2) to indicate that there is a substantial selection bias.

First of all, we control for pre-treatment firm size measured by the natural logarithm of the number of employees ($log(Empl)$).[8] Size is widely used as a predictor of financial constraints because large firms tend to be older and more transparent, which might facilitate access to credit. We also control for pre-treatment year-on-year employment growth rates *($\Delta Empl$)* because post-treatment differences in growth could simply be a follow-up of pre-treatment differences and growth is also considered to ease firm's access to bank credit. Strong growth, however, could also be associated with higher risk or uncertainty originating from firms' business models, which could adversely affect bank lending supply conditions.

In line with the related literature reviewed in Section 3.2, we further rule out heterogeneity in leverage, measured in terms of equity to assets *(Equity ratio)*, profitability as return on assets *(ROA)*, the ratio of fixed assets to total assets *(Fixed Assets)*, the interest coverage ratio in terms of operating profit over interest expenses *(Coverage ratio)*, and cash holdings as a share of total assets *(Cash)*. These balance sheet variables provide valuable information about a firm's financial soundness and are thus widely used in the related literature to control for firm-side factors.

To further approximate and control for a firm's current state, also during the fiscal

[8]Our results are also robust to measuring firm size in terms of the natural logarithm of total assets.

year, we rely on firms' self assessments provided by the monthly Ifo Business Survey. Here, firms assess their current and expected future business conditions qualitatively on a three category scale. More specifically, firms report their overall current business situation to be good, satisfactory, or bad *(State (+), State (=), State (-))*. Due to its open character, this appraisal potentially covers a wide range of complementary information to balance sheet data, while its timeliness is an additional advantage. A second indicator capturing heterogeneity in firms' current states is provided by their assessments of their current stock of orders *(Orders (+), Orders(=), Orders(-))*. In Table 3.2 we only report the positive and the negative categories of these variables for the sake of conciseness.

In addition, we control for the firms' current short-time work status. Short-time work is a partially subsidized labor market instrument that is widely used by German firms to adjust their capacities to business cycle or seasonal demand fluctuations. It was widespread and extensively applied in the German manufacturing sector during the financial crisis. A firm working short-time signals that its workforce is currently too large. Furthermore, we also control for the firms' export status, that is, whether a firm is exporting its products and thus whether it relies on foreign demand, which decreased dramatically during the financial crisis, potentially affecting both a firm's bank lending supply as well as its employment growth.

Moreover, we approximate a firm's future prospects by forward-looking firm characteristics that are likely to predict a firm's bank lending conditions as well as its employment growth. Specifically, we control for a firm's business expectations *(Business expect (+), Business expect (=), Business expect (-))*, its employment expectations *(Empl expect (+), Empl expect (=), Empl expect(-))*, the assessments of its current workforce relative to expected future demand *(Headcount (+), Headcount (=), Headcount (-))*, and its expectation to work short-time *(Short-time exp)*.

Comparing the descriptive statistics obtained in the pre-treatment month, *t-1*, we find that restricted firms are characterized by worse financial conditions than unrestricted firms according to balance sheet information (Table 3.2). With the exception of the equity ratio, differences are not statistically significant due to high standard deviations in the ratios. These are driven by extreme values, which we deal with later in our estimation procedure. Most striking in the context of our analysis, we find significant differences in the fraction of firms reporting negative current business situations *(State (-), Orders (-))*, as well as in negative future expectations *(Business expect (-), Empl expect (-), Headcount (-))*. This suggests that the matching approach might benefit from utilizing these timely

Table 3.2: Descriptive statistics

	Restricted firms (N=329)			Unrestricted firms (N=4950)			$p > t$ $(\bar{X}_R = \bar{X}_U)$
	$\bar{X}_R$	X_R^{med}	*S.D.*	$\bar{X}_U$	X_U^{med}	*S.D.*	
Size and growth							
log(Empl)	5.4	5.3	1.3	5.5	5.4	1.1	0.49
Δ*Empl*	10.3%	0.0%	97.0%	65.6%	0.0%	2459.4%	0.68
Balance sheet data							
Equity ratio	33.0%	30.7%	25.4%	39.1%	38.2%	21.8%	0.000
ROA	-8.4%	2.9%	38.7%	-8.3%	6.0%	47.5%	0.99
Fixed assets	37.3%	36.2%	20.7%	36.5%	36.4%	19.5%	0.45
Coverage ratio	16.9	1.3	4875.8	205.2	4.4	2828.1	0.27
Cash	10.1%	4.2%	12.9%	11.5%	5.8%	13.9%	0.09
Currrent situation							
State (+)	21.6%	0	41.2%	25.5%	0	43.6%	0.12
State (-)	28.6%	0	45.2%	21.5%	0	41.1%	0.003
Orders (+)	10.0%	0	30.1%	13.1%	0	33.7%	0.11
Orders (-)	40.7%	0	49.2%	33.5%	0	47.2%	0.008
Short-time	13.7%	0	34.4%	14.8%	0	35.5%	0.58
Export	88.1%	1	32.4%	88.6%	1	31.8%	0.81
Future expectations							
State exp (+)	16.1%	0	36.8%	19.1%	0	39.3%	0.18
State exp (-)	25.2%	0	43.5%	19.3%	0	39.5%	0.009
Empl exp (+)	6.1%	0	23.9%	6.4%	0	24.5%	0.82
Empl exp (-)	22.2%	0	41.6%	16.4%	0	37.1%	0.007
Short-time exp	19.8%	0	39.9%	20.2%	0	40.2%	0.83
Headcount (+)	5.5%	0	22.8%	6.2%	0	24.1%	0.60
Headcount (-)	26.7%	0	44.3%	20.6%	0	40.4%	0.008

Notes: The table shows means, medians and standard deviations of pre-treatment firm characteristics, $X_{i,t-1}$, separately for treated and untreated firms; the treatment status ($Restricted_{i,t}$) is defined as described in Section 3.4.2; p-values are reported for a two-group mean comparison t-test; no adjustment for multiple testing; the samples only contain observations for which all firm characteristics are available in *t-1*.

contemporaneous and forward-looking indicators of firms' bank lending conditions and growth in order to rule out bias from pre-treatment differences in firm characteristics.

3.5 Results

3.5.1 Least squares

Least squares estimates provide a natural benchmark to assess the effects of changes in bank lending supply on firm-level employment growth with and without controlling for firms' current business situations and future expectations. Therefore, we regress post-treatment year-on-year employment growth rates $\Delta Empl_{i,t+12}$, obtained after twelve months, on the treatment status, $Restricted_{i,t}$, and four different sets of pre-treatment control variables, $X_{i,t-1}$. To rule out the impact of extreme values in employment growth rates and balance sheet variables, we apply the robust regression algorithm described in Hamilton (1991). More specifically, we run an initial ordinary least squares estimation and drop all observations with a Cook's distance larger than one and proceed by using Huber iterations as well as biweight iterations assigning lower weights to influential observations.[9] The results are shown in Table 3.3. All regression models include time and industry dummies. To ensure that the estimations are comparable, we only use observations for which all variables included in the complete setup in Estimation (4) are available. Comparing Estimations (1) to (3), we find that including $log(Empl)_{i,t-1}$, $\Delta Empl_{i,t-1}$, and the set of balance sheet variables virtually does not change the estimated effect of restrictive bank lending supply on firm-level employment growth.

However, including survey-based appraisals of firms' current business situations and future expectations in Estimation (4) lowers the estimated effects of bank-side factors by about one third. We take this as first evidence that for ruling out firm heterogeneity based on firm's size, growth and balance sheet variables only considerable bias remains.

3.5.2 Matching

In contrast to standard regression methods, matching enables further bias reduction by balancing the covariates' distributions between the restricted and the unrestricted firms

[9] In an alternative outlier robust estimation procedure, we winsorize five percent at both tails of the distribution for all non-dichotomous variables and run ordinary least squares estimation. The results presented in Table 3.A.1 in the Appendix are in line with the baseline, although the significance reduction for Estimation (4) is more pronounced.

Table 3.3: Regression results

	(1)	(2)	(3)	(4)
Restricted	-0.0137***	-0.0137***	-0.0137***	-0.0092**
	(0.004)	(0.004)	(0.004)	(0.004)
Size and growth	No	Yes	Yes	Yes
Balance sheet data	No	No	Yes	Yes
Current situation	No	No	No	Yes
Future expectations	No	No	No	Yes
Month dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
R-squared	0.0790	0.1883	0.1930	0.2487
N	5068	5068	5068	5068

Notes: The table shows weighted least squares estimations of $\Delta Empl_{i,t+12}$ on the treatment status, $Restricted_{i,t}$, and different sets of pre-treatment control variables, $X_{i,t-1}$; we use a robust regression algorithm to deal with extreme values by first omitting observations with a Cook's distance greater than one, then by performing weighted least squares estimation based on weights from Huber iterations and biweight iterations as described in Hamilton (1991); "Size and growth", "Balance sheet data", "Current situation", and "Future expectations" are sets of control variables as listed in Table 3.1; the four samples only contain observations for which all control variables of Estimation (4) are available in *t-1*; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

as well as by ensuring a comparison within the covariates' common support. To estimate the effects of supply-driven bank lending restrictions on firm-level employment growth, we combine exact matching on time and industry dummies with propensity score matching on a set of pre-treatment firm characteristics, $X_{i,t-1}$. In accordance with the related literature, we first match on firms' size, growth and balance sheets in *t-1*. In a second specification, we also match on firms' current business situations and future expectations using the survey-based appraisals described in Section 3.4. Based on the comparison of the two specifications, we draw conclusions on the sensitivity of the impact estimates of restrictive lending on firm-level employment growth to the incorporation of timely indicators of firms' current states and future prospects. Turning to the estimation of the propensity score, the following logistic regression model is estimated separately for each quarter:

$$Logit(E[Restricted_{i,t}]) = \alpha + \beta * X_{i,t-1} + \gamma * Industry_i \tag{3.3}$$

where $Restricted_{i,t}$ is a firm's treatment status, $X_{i,t-1}$ is a set of pre-treatment firm characteristics and $Industry_i$ is a set of industry dummies based on the two-digit German standard industry classification (WZ 2008), while α is a constant, β and γ are parameter vectors. Due to the limited number of observations within quarter-industry cells, the propensity score model is estimated separately for each quarter pooling over industries but conditioning on a set of industry-specific indicator variables. The subsequent matching procedure, however, is performed within quarter-industry cells. Also note that in order to account for extreme values in employment growth rates or balance sheet variables, observations are omitted if the square root of their delta deviance influence statistic exceeds a value of three, as suggested by Agresti and Finlay (2008).

To estimate the effects of supply-driven bank lending restrictions, we compare restricted firms to unrestricted ones with a similar propensity score based on three alternative matching estimators: a ten nearest neighbors matching, a radius matching using a caliper of 0.2σ, with σ being the standard deviation of the propensity score in the full sample (Austin, 2011), and an Epanechnikov kernel-based matching with a bandwidth of 0.06. To account for heterogeneity arising from the changing macroeconomic environment and from industry-specific factors, firms are matched within quarter-industry cells. The exact matching on time and industry also reduces potential bias arising from shortcomings in the wording of the survey question on credit supply conditions (see Section 3.4.2). Following the matching procedures, the derived weights are employed to estimate the treatment effects based on weighted least squares.

Table 3.4 reports the average treatment effects on the treated (ATT) for the alternative matching procedures by sets of covariates. According to Panel A, the average treatment effect of $Restricted_{i,t}$ on $\Delta Empl_{i,t+12}$ varies from -1.2 to -2.1 percentage points across matching algorithms if the matching is performed on size, growth and balance sheet variables only. The estimates are statistically and economically significant throughout. Panel B, however, provides matching estimates based on the full set of covariates. Here, estimates range from -0.2 to -1.0 percentage points. Irrespective of the matching algorithm, estimates tend to be smaller in magnitude and statistically insignificant. For two of the three matching algorithms, nearest neighbor and kernel, estimates reported in Panel B are even outside the 90 percent confidence intervals of the those in Panel A. We take this as evidence that the omission of contemporaneous and forward-looking firm characteristics induces a considerable overestimation of the impact of bank lending restrictions on

Table 3.4: Matching estimates for $\Delta Empl_{i,t+12}$ by sets of covariates

Panel A: Matching on size, growth, and balance sheet data only

	NN 10	Radius	Kernel
ATT	-2.08%	-1.24%	-1.64%
S.E.	(0.0048)	(0.0044)	(0.0041)
P-value	0.000	0.005	0.000
Upper bound	-1.29%	-0.52%	-0.96%
Lower bound	-2.87%	-1.96%	-2.32%
# of treated	190	172	183
# of matchings	1156	1342	1694

Panel B: Matching on all variables

	NN 10	Radius	Kernel
ATT	0.16%	-1.00%	-0.16%
S.E.	(0.0059)	(0.0060)	(0.0053)
P-value	0.79	0.10	0.76
# of treated	141	117	127
# of matchings	818	814	1012

Notes: The table reports the average treatment effect on the treated (ATT) of the treatment $Restricted_{i,t}$ on the year-on-year employment growth rate $\Delta Empl_{i,t+12}$ based on weighted least squares estimation; in Panel A weights are derived from matching firms based on size, growth and balance sheet data in *t-1*; upper and lower bounds are reported for 90 percent confidence intervals; in Panel B weights are derived from matching firms based on size, growth, and balance sheet data as well as on their current business situations and future expectations in *t-1*; to account for extreme values, observations with a delta deviance influence statistic larger than three are omitted; p-values are reported for a t-test of significance of the ATT.

employment.[10]

Based on their balancing properties, we assess the quality of the two matching approaches employing alternative sets of covariates. In order to provide an unbiased estimate the approaches need to substantially reduce the differences in pre-treatment firm charac-

[10]This result also holds for alternative outlier adjustments. In an otherwise similar matching procedure, we winsorize five percent at both sides of the distribution of all non-dichotomous variables instead of using the approach suggested by Agresti and Finlay (2008). In this setup, the difference between estimates derived with and without controlling for firms' current business situations and future expectations is even more pronounced, and the differences are statistically significant throughout (see Table 3.A.2 in the Appendix.)

teristics that were documented for the unmatched sample (see Section 3.4.3). According to Table 3.5, however, matching on size, growth, and balance sheet variables does not sufficiently balance the survey-based indicators that were not considered in the propensity score estimation. In particular, statistically significant differences remain for *State (-)*, *Orders (-)*, and *Headcount (-)*. In addition, pre-treatment differences in *Orders (+)*, *Short-time*, and *Empl exp (-)* are close to being statistically significant and show considerable bias. However, additionally considering contemporaneous and forward-looking information seems to reduce the differences substantially (right panel of Table 3.5). Drawing on these results, we conclude that matching on balance sheet variables does not sufficiently balance pre-treatment differences in timely measures of firms' current states and future prospects.[11] Instead, considerable bias is induced.

Turning to the overall significance of the estimated employment effects, our results seem to cast doubt on the relevance of supply-driven effects of bank lending restrictions during the period under consideration. Indeed, if matching on the full set of covariates, incorporating timely information on firms pre-treatment appraisals of their current state and future prospects, treatment effects are found to be small in economic terms and insignificant in statistical terms. Given potential shortcomings in the treatment classification as laid out in Section 3.4.2, we nevertheless cannot rule out downward bias in our estimates and do not stress this result accordingly.

3.6 Robustness

3.6.1 Sample selection

In contrast to the majority of studies in the related empirical literature (see Section 3.2), the sample under consideration comprises mainly non-listed firms in the German manufacturing sector. Accordingly, firms are smaller and more bank dependent compared to studies focusing on listed companies or more market based financial systems (such as in the U.S. or the U.K.).[12] Our results may therefore be more relevant for the empirical research on European bank based economies, particularly if non-listed companies are analyzed and forward-looking measures based on stock price evaluations (such as Tobin's q)

[11]For the sake of conciseness, balancing statistics are presented for the radius matching only. Note, however, that for each of the other matching algorithms significant bias in contemporaneous and forward-looking firm characteristics is found.

[12]See, for example, Allen and Gale (2000) for a comprehensive review of the vast literature on comparative financial systems.

Table 3.5: Balancing properties by sets of covariates

	Balance sheet only ($N_R = 172$; $N_U = 1342$)			All variables ($N_R = 117$; $N_U = 814$)		
	$\bar{X}_R - \bar{X}_U$	$p > t$ ($\bar{X}_R = \bar{X}_U$)	Bias	$\bar{X}_R - \bar{X}_U$	$p > t$ ($\bar{X}_R = \bar{X}_U$)	Bias
Size and growth						
log(Empl)	-12.5%	0.28	-10.20	-2.9%	0.82	-2.39
Δ*Empl*	-1.6%	0.35	-0.09	-0.9%	0.57	-0.05
Balance sheet data						
Equity ratio	-2.5%	0.29	-10.60	-0.5%	0.87	-1.93
ROA	-3.0%	0.52	-6.97	-2.2%	0.66	-5.10
Fixed assets	-0.4%	0.84	-2.16	1.2%	0.62	5.94
Coverage ratio	-13.48	0.76	-0.34	-27.46	0.84	-0.69
Cash	-1.2%	0.39	-8.89	-1.0%	0.55	-7.32
Current situation						
State (+)	-6.1%	0.18	-14.37	-4.2%	0.41	-9.85
State (-)	9.2%	0.06	21.20	1.9%	0.74	4.36
Orders (+)	-5.6%	0.13	-17.38	-2.8%	0.47	-8.70
Orders (-)	9.9%	0.06	20.62	2.3%	0.70	4.84
Short-time	5.8%	0.12	16.73	-2.2%	0.57	-6.40
Export	-0.2%	0.96	-0.54	1.6%	0.62	5.02
Future expectations						
State exp (+)	0.7%	0.87	1.75	-1.1%	0.78	-3.02
State exp (-)	0.7%	0.89	1.57	-2.0%	0.72	-4.83
Empl exp (+)	-0.3%	0.90	-1.20	0.8%	0.72	3.27
Empl exp (-)	7.1%	0.11	18.00	0.2%	0.97	0.40
Short-time exp	1.6%	0.72	4.09	-3.6%	0.47	-9.06
Headcount (+)	-0.1%	0.98	-0.33	0.6%	0.82	2.59
Headcount (-)	9.9%	0.04	23.28	2.9%	0.59	6.92
	$\chi^2(18)$	$p > \chi^2$		$\chi^2(18)$	$p > \chi^2$	
LR-test	16.29	0.70		5.13	1.00	

Notes: The table shows differences in means in pre-treatment covariates, $X_{i,t-1}$, for the radius matching; p-values are reported for two-group mean comparison t-tests and LR-tests for joint significance of all control variables in predicting the treatment status $Restricted_{i,t}$; no adjustment for multiple testing; bias statistics are calculated according to $(\bar{X_R} - \bar{X_U})/\sqrt{\frac{\sigma_R^2+\sigma_U^2}{2}}$.

are unavailable.[13]

Turning to the matching estimator, sample selection is a potential source of bias. Selected sub-samples can differ substantially across matching procedures. Therefore, any comparison of matching estimates should also consider potential bias arising from sample selection, specifically with regard to the sub-samples of treated firms they cover. To rule out differences in the samples due to item non-response, we reduce our whole analysis to firm-month observations for which all variables are non-missing, even if the matching is performed on balance sheet variables only. However, there is additional reason for firms to drop out of the sample. First, and most important, adding the indicators of firms' current states and future prospects more than doubles the number of matching variables. Consequently, the curse of dimensionality is increased. This enables the identification of more appropriate matching firms but also reduces the number of observations effectively employed by the matching algorithms. In particular, the large number of dichotomous explanatory variables considerably increases the probability of perfect predictions of the treatment status within the propensity score estimation via logistic regression. As a standard procedure to ensure numerical stability in this case, explanatory variables that perfectly predict the treatment status along with the relevant observations are excluded from the sample. In addition, the loss in observations is further amplified by restricting the analysis to the common support region as defined by the propensity score as well as by the direct matching within quarter and industry cells. For the radius and the kernel matching, the sample size also decreases according to the caliper and the kernel bandwidth respectively. Altogether, this curse of dimensionality issue causes a considerable deviation of sub-samples employed by the matching on the full set of covariates from the matching on growth, size, and balance sheet information only.

To rule out that sample selection is driving our results, we compare the sub-samples utilized by the matching procedures on the two alternative sets of covariates. We focus on the comparison of the treated firms since their characteristics also determine the selection

[13] Note, however, that although being extensively employed in the applied empirical literature, Tobin's q and related measures might poorly approximate firm's growth opportunities and future returns. In fact, Erickson and Whited (2000) show that for financially constrained firms most of the stylized facts produced by investment-q cash flow regressions are artifacts of measurement error. According to Cummins, Hassett, and Oliner (2006), the manifold potential sources of this measurement error include non-constant returns to scale, market power in product or factor markets, non-convex adjustment costs, putty-clay technologies, and stock market inefficiencies. Given that the financial crisis of 2007/08 simultaneously drives bank lending restrictions in our sample period as well as a pronounced stock market boom-bust cycle, the omission of stock price related variables in this study likely does not alter results considerably.

Table 3.6: Treated firm characteristics in *t-1* by sets of covariates

	Balance sheet	All variables	$p > t$
	$\bar{X}$	$\bar{X}$	
Growth (t+12)			
$\Delta Empl$	-1.1%	-0.4%	0.45
t-1			
Size and growth			
log(Empl)	5.3	5.4	0.53
$\Delta Empl$	2.7%	1.8%	0.56
Balance sheet data			
Equity / Assets	35.0%	35.7%	0.75
ROA	-7.5%	-7.8%	0.95
Fixed Assets / Assets	36.7%	36.1%	0.76
Coverage ratio	43.2	52.1	0.81
Cash / Assets	10.1%	9.9%	0.87
Currrent situation			
State (+)	21.2%	21.0%	0.96
State (-)	29.8%	28.0%	0.72
Orders (+)	10.1%	10.2%	0.98
Orders (-)	43.4%	40.8%	0.61
Short-time	15.7%	9.6%	0.09
Export	88.4%	91.7%	0.30
Expectations			
Business expect (+)	15.7%	15.3%	0.92
Business expect (-)	28.3%	26.8%	0.75
Empl expect (+)	5.1%	3.8%	0.58
Empl expect (-)	24.2%	21.0%	0.47
Short-time expect	22.2%	17.8%	0.31
Hcount (-)	29.8%	26.8%	0.53
Hcount (+)	5.6%	5.1%	0.85

Notes: For the samples selected by the radius matching, the table compares the means of all covariates in *t-1* for the treated firms between the matching on size, growth, and balance sheet variables and the matching on all variables; p-values are provided for a two-group mean comparison test; no adjustment for multiple testing.

of the matching firms. Table 3.6 reports the results for the radius matching.[14] Based on standard t-tests, we find no substantial differences in pre-treatment firm characteristics between the two samples. More specifically, out of twenty mean comparison tests only the one for the short-time work status indicates a statistically significant difference at the ten percent level. However, the expectations to work short-time within the next three months are fairly balanced. The sample comparison suggests that differences in the estimated supply-side effects of restrictive bank lending are rather driven by a reduction in selection bias and less likely caused by sample selection. Furthermore, similar but unreported comparisons of the treated firms in the sub-samples selected by the procedures with the total population of treated firms in the unmatched sample also indicates the representativeness of the selected sub-samples.

3.6.2 Remaining extreme values

We deal with extreme values in $\Delta Empl_{i,t+12}$ and in the balance sheet variables using two approaches in Section 3.5.2. To ensure that the results are not driven by remaining outliers in $\Delta Empl_{i,t+12}$, we also estimate the treatment effect of $Restricted_{i,t}$ on the probability of having a positive employment growth rate, indicated by the dummy variable $Growth_{i,t+12}$, which is unaffected by extreme levels in growth rates. According to Panel A of Table 3.7, the estimated effects of bank-side factors are statistically significant and fairly stable across matching algorithms. If taking into account firms' current business situations and future expectations, however, the effect drops to almost zero and turns insignificant. Again, estimates in Panel B are outside the 90 percent confidence intervals of estimates in Panel A. This confirms our finding of substantial bias in the estimated effects of supply-side-driven bank lending restrictions bank-side factors if firms' current business situations and future expectations are not accounted for, even if we use a measure that is not susceptible to extreme values.

3.7 The role of the financial crisis

In the sample period ranging from 2003 to 2011, the variation in reported bank lending restrictions is largely driven by the financial crisis of 2007/08 (see Figure 3.1). This might affect the extent to which selection bias arises from omitting timely indicators of firms'

[14]Sample comparisons for the nearest neighbors and kernel matching also do not show considerable differences, but are not shown for the sake of conciseness.

Table 3.7: ATT on dichotomous $Growth_{i,t+12}$

Panel A: Matching on size, growth, and balance sheet data only

	NN 10	Radius	Kernel
ATT	-9.05%	-9.19%	-10.22%
S.E.	(0.0255)	(0.0244)	(0.0219)
P-value	0.000	0.000	0.000
Upper bound	-4.86%	-5.17%	-6.62%
Lower bound	-13.24%	-13.21%	-13.83%
# of treated	207	185	197
# of matchings	1260	1427	1789

Panel B: Matching on all variables

	NN 10	Radius	Kernel
ATT	2.48%	-2.00%	0.46%
S.E.	(0.0301)	(0.0313)	(0.0283)
P-value	0.41	0.52	0.87
# of treated	153	124	138
# of matchings	882	845	1048

Notes: The table reports the average treatment effect on the treated (ATT) of the treatment $Restricted_{i,t}$ on the dummy variable $Growth_{i,t+12}$ based on weighted least squares estimation; in Panel A weights are derived from matching firms based on size, growth, and balance sheet data in *t-1*; upper and lower bounds are reported for 90 percent confidence intervals; in Panel B weights are derived from matching firms based on size, growth, balance sheet data, and their current business situations and future expectations in *t-1*; to account for extreme values, observations with a delta deviance influence statistic larger than three are omitted; p-values are reported for a t-test of significance of the ATT.

current states and future prospects for two reasons. First, firms were operating under exceptional conditions and uncertainty about the future macroeconomic environment was high. Therefore, balance sheet data, which are backward looking in nature, became less informative for banks to assess firms' creditworthiness. Under these circumstances we would expect banks to put more weight on contemporaneous and forward-looking firm characteristics. Second, the economic downturn as well as high levels of uncertainty following the financial crisis brought down firms' credit demand considerably. The omission of timely contemporaneous or even forward-looking indicators of firm-specific credit demand is thus particularly likely to induce considerable bias in the time period under consideration.

Splitting our sample in a pre-crises and a crisis sub-sample in order to study structural differences, however, is prohibited by the low number of treatments observed in the pre-crisis period. Defining the second quarter of 2007 as the end of the pre-crisis period would leave us with about 100 treated firm-month observations in the corresponding sub-sample. As a result of the curse of dimensionality issue arising from matching firms within quarter-industry cells, the insufficient number of less than 30 treated firm-month observations can be utilized to draw inference on the pre-crisis sub-sample.[15] In consequence, we have to admit that our results are representative for the crisis but possibly not for the pre-crisis period.

Showing the robustness of our results for the crises sub-sample might be reassuring, however. According to Tables 3.A.3 and 3.A.4 in the appendix, controlling for firms' current business situations and future expectations lowers the estimated treatment effect of restrictive bank lending on employment growth rates as well as the probability of firms growing significantly in the crisis sub-sample. Whether there is a difference compared to non-crisis periods, however, remains an open question to be answered by future research when more data is available.

3.8 Conclusion

The effectiveness and efficiency of policy responses towards recessions and credit slumps crucially depends on the understanding of to what extent credit market outcomes are driven by supply-side or demand-side factors. If under-capitalized banks are a burden on the economy, government intervention may well be justified. However, if on the contrary growth is hampered by firm-side factors (e.g. by subdued expectations or low creditworthiness) and credit volumes go down in response to weak credit demand or higher default risk, government interventions should not necessarily aim at banks.

The empirical analysis of the effects of supply-driven changes in bank lending restrictions on real economic activity is complicated by the need to rule out demand-side factors. On the firm-level, the existing literature primarily does so based on firms' balance sheet information. Balance sheets, however, are published on a low frequency and are backward-looking in nature. In particular, they contain little information on firms' expectations. In

[15]We split the sample according to Ivashina and Scharfstein (2010), who date the peak of the credit boom in the U.S., which was followed by a meltdown of sub-prime mortgages and securitized products, to the second quarter of 2007. In Germany, the first crisis related bank failure occurred at the end of July 2007 when the IKB (Deutsche Industriebank AG) was bailed out.

this study, we observe firms' self-perceived bank lending restrictions on an almost monthly frequency and ask the question of whether controlling for similarly high-frequent survey-based indicators of firms current states and future prospects in addition to balance sheet information impacts the inference on identified treatment effects.

We employ a panel data of German manufacturing firms in which bank lending restrictions are mainly driven by the 2007/08 financial crisis. At the beginning of our analysis, we estimate the effect of restrictive bank lending on firm-level employment growth using a matching estimator based on balance sheet variables only. The results suggest a significant supply-driven effect of bank lending restrictions on firm-level employment growth. However, this effect is not confirmed once we control for survey-based appraisals of firms' current business situations and future expectations. Specifically, treatment effects turn out to be significantly lower while balancing properties improve considerably. In contrast, balancing properties are poor in the case of matching on balance sheet variables only, revealing significant bias from unbalanced contemporaneous and forward-looking firm-specific indicators. Finally, robustness exercises confirm that our results hold irrespective of the matching algorithm or the adjustment of extreme values in employment growth rates and balance sheet variables.

Our findings indicate that estimates of firm-level effects of bank lending restrictions are sensitive to the incorporation of contemporaneous and forward-looking information on firms' credit demand. Indeed, their omission may cause considerable bias. For this reason, our results ask researchers to cautiously infer on the effects of bank lending restrictions if they rely on balance sheet information only.

Acknowledgments

I am indebted to Manuel Wiegand, who is co-author of Chapter 3.

Appendix

Table 3.A.1: Ordinary least squares estimates

	(1)	(2)	(3)	(4)
Restricted	-0.0108*	-0.0121**	-0.0118**	-0.0085
	(0.006)	(0.006)	(0.006)	(0.006)
Size and growth	No	Yes	Yes	Yes
Balance sheet data	No	No	Yes	Yes
Current situation	No	No	No	Yes
Future expectations	No	No	No	Yes
Month dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
R-squared	0.0625	0.0943	0.1016	0.1601
N	5068	5068	5068	5068

Notes: The table shows least squares estimations of $\Delta Empl_{i,t+12}$ on the treatment status, $Restricted_{i,t}$, and different sets of pre-treatment control variables, $X_{i,t-1}$; all non-dichotomous variables are winsorized by five percent at both tails of the distribution; standard errors are clustered at the firm-level; "Size and growth", "Balance sheet data", "Current situation", and "Future expectations" are sets of control variables as listed in Table 3.1; the four samples contain only observations for which all control variables of Estimation (4) are available in *t-1*; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.A.2: ATT on $\Delta Empl_{i,t+12}$ with alternative outlier adjustment

Panel A: Matching on size, growth, and balance sheet data only

	NN 10	Radius	Kernel
ATT	-1.62%	-1.91%	-2.04%
S.E.	(0.0048)	(0.0047)	(0.0041)
P-value	0.001	0.000	0.000
Upper bound	-0.83%	-1.14%	-1.38%
Lower bound	-2.42%	-2.68%	-2.71%
# of treated	198	173	190
# of matchings	1231	1392	1846

Panel B: Matching on all variables

	NN 10	Radius	Kernel
ATT	-0.35%	-0.05%	-0.06%
S.E.	(0.0053)	(0.0056)	(0.0050)
P-value	0.51	0.92	0.89
# of treated	173	143	157
# of matchings	1102	985	1312

Notes: The table reports the average treatment effect on the treated (ATT) of the treatment $Restricted_{i,t}$ on the year-on-year employment growth rate $\Delta Empl_{i,t+12}$ based on weighted least squares estimation; in Panel A weights are derived from matching firms based on size, growth and balance sheet data in *t-1*; upper and lower bounds are reported for the 90 percent confidence interval; in Panel B weights are derived from matching firms based on size, growth, and balance sheet data as well as on their current business situations and future expectations in *t-1*; to account for extreme values, we winsorize five percent of the observations in $\Delta Empl_{i,t+12}$ and all non-dichotomous variables from both sides of the distribution; p-values are reported for a t-test of significance of the ATT.

Table 3.A.3: ATT on $\Delta Empl_{it+12}$ after 2007Q2

Panel A: Matching on size, growth, and balance sheet data only

	NN 10	Radius	Kernel
ATT	-1.50%	-2.33%	-2.13%
S.E.	(0.0057)	(0.0053)	(0.0048)
P-value	0.009	0.000	0.000
Upper bound	-0.55%	-1.45%	-1.34%
Lower bound	-2.44%	-3.20%	-2.91%
# of treated	151	138	148
# of matchings	922	1132	1425

Panel B: Matching on all variables

	NN 10	Radius	Kernel
ATT	-0.34%	-0.81%	-0.91%
S.E.	(0.0052)	(0.0052)	(0.0046)
P-value	0.51	0.12	0.05
# of treated	126	114	121
# of matchings	796	867	1092

Notes: The table reports the average treatment effect on the treated (ATT) of treatment $Restricted_{i,t}$ on year-on-year employment growth rate $\Delta Empl_{i,t+12}$ based on weighted least squares estimation; in Panel A weights are derived from matching firms based on size, growth, and balance sheet data in *t-1*; upper and lower bounds are reported for the 90 percent confidence interval; in Panel B weights are derived from matching firms based on size, growth, balance sheet data, and their current business situations and future expectations in *t-1*; p-values are reported for a t-test of significance of the ATT.

Table 3.A.4: ATT on dichotomous $Growth_{it+12}$ after 2007Q2

Panel A: Matching on size, growth, and balance sheet data only

	NN 10	Radius	Kernel
ATT	-7.70%	-8.69%	-7.96%
S.E.	(0.0288)	(0.0271)	(0.0242)
P-value	0.008	0.001	0.001
Upper bound	-2.96%	-4.22%	-3.99%
Lower bound	-12.44%	-13.14%	-11.93%
# of treated	161	144	156
# of matchings	991	1174	1491

Panel B: Matching on all variables

	NN 10	Radius	Kernel
ATT	-1.27%	-3.59%	-5.05%
S.E.	(0.0309)	(0.0302)	(0.0272)
P-value	0.68	0.24	0.06
# of treated	140	123	132
# of matchings	865	925	1159

Notes: The table reports the average treatment effect on the treated (ATT) of treatment $Restricted_{i,t}$ on on the dummy variable $Growth_{i,t+12}$ based on weighted least squares estimation; in Panel A weights are derived from matching firms based on size, growth, and balance sheet data in *t-1*; upper and lower bounds are reported for the 90 percent confidence interval; in Panel B weights are derived from matching firms based on size, growth, balance sheet data, and their current business situations and future expectations in *t-1*; p-values are reported for a t-test of significance of the ATT.

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Ich versichere hiermit eidesstattlich, dass ich die vorliegende Arbeit selbständig und ohne fremde Hilfe verfasst habe. Die aus fremden Quellen direkt oder indirekt übernommenen Gedanken sowie mir gegebene Anregungen sind als solche kenntlich gemacht. Die Arbeit wurde bisher keiner anderen Prüfungsbehörde vorgelegt und auch noch nicht veröffentlicht.

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