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Carlo Wix

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The Long-Run Real Effects of Banking Crises: Firm-Level Investment Dynamics and the Role of Wage Rigidity

CARLO WIX*

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Abstract

I study the long-run effects of credit market disruptions on real firm outcomes and how these effects depend on nominal wage rigidity at the firm level. Exploiting variation in firms' refinancing needs during the global financial crisis, I trace out firms' investment and growth trajectories in response to a credit supply shock. Financially shocked firms exhibit a temporary investment gap for two years, resulting in a persistent accumulated growth gap six years after the crisis. Shocked firms with rigid wages exhibit a significantly steeper drop in investment and an additional long-run growth gap relative to shocked firms with flexible wages.

Keywords: Financial Crises; Bank Lending; Real Effects; Firm Investment; Wage Rigidity

JEL Classification: E22; E24; E51; G01; G21; G31

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I. Introduction

The 2008-09 global financial crisis was followed by the Great Recession and a subsequent slow recovery in the United States and other advanced economies (Reinhart and Rogoff, 2014). The protracted nature of the recovery and considerable heterogeneity in recoveries across countries has sparked an ongoing debate about the long-run real effects of banking crises, with many important questions remaining unresolved (Romer and Romer, 2017). To what extent does the long-term aftermath of banking crises represent a causal relationship between credit market disruptions and real economic activity? Do banking crises have persistent negative real effects or is there a full recovery in the long run? And what explains heterogeneity in the recovery paths?

Studying these questions with aggregate data is difficult because of several empirical challenges. First, banking crises being followed by a decline in output does not necessarily imply a causal effect of credit market disruptions on real economic activity (Dell'Ariccia, Detragiache, and Rajan, 2008). Adverse economic shocks could also cause financial distress in the banking sector through a reduction in firm investment and credit demand, suggesting reverse causality. Second, assessing economic recoveries requires a concept of "returning to normal," commonly measured by either deviations of GDP from the previous peak or by deviations of GDP from potential output (Fatás and Mihov, 2013). As the former measure ignores the trend growth during recessions, and the latter measure faces great uncertainty in the estimation of potential output, both approaches suffer from the lack of a credible counterfactual of how economic activity would have evolved in the absence of a crisis. Finally, explaining heterogeneity in recoveries across countries faces the challenge that countries differ vastly along many important economic dimensions. This makes it difficult to isolate specific channels (such as labor market frictions) that affect the recovery following a banking crisis.

In this paper, I address these challenges by using loan- and firm-level data to study the long-run effects of the 2008-09 financial crisis on corporate investment and firm growth and how these effects depend on nominal wage rigidity at the firm level. Estimating the effect of credit availability on firm behavior faces the traditional identification challenge of disentangling banks' credit supply from firms' credit demand. Most papers exploit negative shocks to bank health, resulting in reduced bank-specific credit supply, to study the effect on firms that borrow from the affected banks.[1]

[1]Chava and Purnanandam (2011) exploit banks' exposure to Russian sovereign debt during the Russian crisis in 1998; Chodorow-Reich (2014) exploits variation in lender health following the Lehman bankruptcy in 2008; and

Identification relying on bank-firm relationships, however, requires the non-innocuous assumption that bank-firm matching in the loan market is exogenous to firm-specific credit demand (Schwert, 2018). In contrast, the empirical strategy employed in this paper does not rely on bank-firm relationships for identification. I exploit exogenous variation in the refinancing needs of U.S. firms due to maturing term loans and expiring credit lines during the credit crunch in the wake of the 2008-09 financial crisis. Since such long-term debt instruments are a large and important source of corporate funding (Sufi, 2007; Ivashina and Scharfstein, 2010), firms with refinancing needs during this period can be expected to be more adversely affected by the credit crunch than firms that do not have to roll over maturing debt during this same period.[2]

First, I use a local projection difference-in-difference matching estimation approach to trace out the trajectories of investment and firm growth over a period of six years after the 2008-09 financial crisis. I find that firms more adversely affected by the reduction in aggregate credit supply reduce quarterly investment by 2 percentage points over a period of two years compared to similar firms in the control group. These results are consistent with the existing corporate finance literature on the short-run effects of credit market disruptions on firm investment (Almeida et al., 2012; Acharya et al., 2018). I then investigate how affected firms adjust their investment policies in the long run and provide novel evidence on firms' recovery paths after the initial drop in investment. I find that two years after the shock, the investment gap closes and both groups of firms converge to similar investment paths. There is, however, no catch-up effect in the long run. Affected firms do not invest more when access to external finance becomes available again and do not offset the temporary investment gap. Hence, this temporary investment gap due to a transitory credit supply shock results in a persistent accumulated growth gap of 8 percentage points a full six years after the crisis. These findings provide novel firm-level evidence for the significant long-run real effects of banking crises and are consistent with the macroeconomic literature on financial frictions (Ajello, 2016; Romer and Romer, 2017).

Next, I turn to labor market frictions as a potential mechanism underlying the long-run real effects of banking crises. In particular, I investigate how wage rigidity at the firm level affects the recovery paths of firms following a banking crisis. While wage rigidity is a well-documented feature

Acharya et al. (2018) exploit banks' exposure to the European sovereign debt crisis from 2010 to 2012.

[2]Following Almeida et al. (2012), similar identification strategies have previously been employed by De Haas and Van Horen (2012), Garicano and Steinwender (2016), and Duval, Hong, and Timmer (2020).

of the labor market and a central topic in the macroeconomic literature on financial frictions, the existing corporate finance literature has little to say about how wage rigidity affects the response of firms to a financial shock. However, payroll expenses make up a large share of firms' total costs (Klasa, Maxwell, and Ortiz-Molina, 2009; Schoefer, 2021). Thus, the inability or unwillingness of firms to reduce labor costs by lowering wages is likely to affect corporate investment and employment, especially in the case of financially constrained firms. Moreover, the concept of labor hoarding posits that, if feasible, adjusting wages while retaining workers allows firms to avoid the costs of firing, rehiring, and retraining workers, thereby putting them in a better position to expand when the economy recovers (Biddle, 2014).

As an illustrative example, consider the case of Alaska Airlines and Southwest Airlines during and after the 2008-09 financial crisis. The airline industry was hit hard by the crisis, and, as shown in Panel A of Figure 1, both airlines sharply decreased investment during the crisis. However, Alaska Airlines exhibited a much steeper drop in investment and took much longer to recover and reach pre-crisis levels of investment relative to Southwest Airlines. As shown in Panel B, Alaska Airlines also responded to the crisis by laying off employees.[3] Southwest Airlines, in contrast, "alone among companies in the airline industry [...] did not layoff a single employee" and instead, as shown in Panel C, "cut pay rather than jobs".[4] As discussed in a 2009 *Wall Street Journal* article, this "cut pay rather than people strategy" allowed Southwest Airlines to not "incur new-employee hiring and training costs" and was therefore "integral to the process of rebounding from a downturn."[5]

Based on these considerations, I construct a novel firm-level measure of wage rigidity, consistent with the notion of labor hoarding and based on the decomposition of payroll changes into changes in the average wage and changes in employment. To assess how the presence of rigid wages affects firms' response to a credit supply shock, I separately trace out the trajectories of investment and firm growth for financially constrained firms with above- and below-median levels of wage rigidity. First, I document that firms with rigid wages reduce employment by up to 6.6 percentage points more and wages by up to 2.2 percentage points less than firms with flexible wages. I then show that wage rigidity at the firm level significantly exacerbates the negative long-run effects of credit

[3]See also "Alaska Airlines to cut flight, jobs" in the *Seattle Post-Intelligencer* from September 12, 2008.
[4]See "Employment and Pay Implications of the Coronavirus" (Lewin, 2020).
[5]See "Cut Pay, Not People" in the *Wall Street Journal* from February 6, 2009.

market disruptions. Financially constrained firms with rigid wages exhibit a temporary additional decline in investment of up to 2.7 percentage points relative to financially constrained firms with flexible wages. This temporary additional investment gap translates into a long-run additional growth gap of up to 12.6 percentage points several years after the crisis. These results emphasize the role of labor market frictions in the amplification of financial shocks and are consistent with recent findings in the macroeconomic literature on financial frictions (Ajello, 2016).

My findings have implications for the policy response of governments to economic crises. With regard to the 2008-09 financial crisis, the case of Germany may provide additional context. While Germany and the United States experienced a similarly severe recession during the financial crisis, the German economy—unlike the U.S. economy—exhibited virtually no rise in unemployment and recovered faster to pre-crisis levels of output. This "labor market miracle" (Burda and Hunt, 2011) has often been attributed to short-time work ("Kurzarbeit") programs, in which the German government subsidized firms' payroll expenses if firms refrained from layoffs. These policy measures effectively constituted an injection of wage flexibility into the German labor market and likely contributed to the fast recovery of the German economy following the crisis. More recently, the United States implemented similar policy measures in response to the COVID-19 crisis. The Payment Protection Program (PPP) under the Coronavirus Aid, Relief, and Economic Security (CARES) Act offered guaranteed loans to small and medium-sized businesses with the goal to help firms pay their employees and prevent layoffs (Granja, Makridis, Yannelis, and Zwick, 2022). Importantly, loans could be forgiven under the condition that firms maintained employment at close to pre-crisis levels (Autor et al., 2022a). The PPP meaningfully helped to preserve millions of jobs during the early stages of the pandemic (Autor et al., 2022b), thereby possibly contributing to the fast recovery of the U.S. economy in the second quarter of 2020 (Hubbard and Strain, 2020).[6]

Since the 2008-09 financial crisis, a growing literature in both corporate finance and macroeconomics examines the effects of bank lending frictions on the real economy. Despite sharing common themes and insights, these two strands of literature remain largely disconnected regarding two important aspects: the long-run effects of financial shocks and the role of wage rigidity in amplifying these shocks. While these two aspects are central topics in the macroeconomic literature on fi-

[6] However, the PPP has also been criticized for being untargeted and overly expensive (Autor et al., 2022a; Granja et al., 2022). A discussion about the costs and benefits of the PPP is beyond the scope of this paper.

nancial frictions, they have received little attention in the empirical corporate finance literature thus far. In this paper, I bridge the gap between these two strands of literature by studying these questions from a corporate finance perspective.

My paper therefore contributes to several strands of literature. First, my work complements and expands on the existing corporate finance literature on the short-run real effects of credit market disruptions (Chava and Purnanandam, 2011; Almeida et al., 2012; Chodorow-Reich, 2014; Cingano et al., 2016). I study the long-run effects of a financial shock over a time horizon of six years and assess both the short-run effects on firms' corporate policies immediately following the shock, as well as the long-run adjustments made by firms during the recovery. Second, I also contribute to the macroeconomic literature on recoveries from financial crises (Cerra and Saxena, 2008; Fatás and Mihov, 2013; Reinhart and Rogoff, 2009, 2014; Romer and Romer, 2017; Queralto, 2020). While this literature suggests a causal effect of banking crises on long-run real outcomes, most papers acknowledge that this evidence is far from conclusive. As identifying exogenous shocks to credit supply is difficult when using aggregate data, Romer and Romer (2017) suggest that "the most fruitful approach to establishing causation may lie in combining natural experiments with detailed cross-section evidence." My paper contributes to this literature by providing such quasi-experimental micro-level evidence. More generally, my paper also contributes to the macroeconomic literature on financial frictions by providing novel firm-level evidence on the role of wage rigidity in the propagation and amplification of financial shocks (Bernanke, Gertler, and Gilchrist, 1999; Brunnermeier, Eisenbach, and Sannikov, 2012; Jermann and Quadrini, 2012; Ajello, 2016). Finally, my paper is also related to a growing stream of literature on the interaction of financial and labor market frictions (Giroud and Mueller, 2017; Ouimet and Simintzi, 2021; Schoefer, 2021). I contribute to this literature by investigating how wage rigidity affects the short- and long-run response of firms to financial frictions.

II. Empirical Strategy

This section describes the empirical methodology used in the paper. I first discuss the assumptions underlying my identification of a credit supply shock. I then describe the local projection difference-in-differences matching estimation approach used to estimate the long-run effects of the credit supply shock on real firm outcomes. Finally, I describe my novel firm-level measure of wage rigidity used to investigate the role of wage rigidity in the amplification of credit supply shocks.

A. Identification Assumptions

My empirical strategy relies on two identification assumptions. First, the 2008-09 financial crisis constituted a negative shock to aggregate credit supply. Second, the extent to which firms were affected by this reduction in aggregate credit supply is exogenous to firms' counterfactual performance in the years following the shock.

Regarding the first assumption, the 2008-09 financial crisis arguably constituted a period of limited access to many sources of external financing. In this paper, I focus on the credit crunch in the syndicated loan market. Syndicated loans, in the form of term loans and credit lines, are a large and important source of corporate finance (Sufi, 2007). Figure 2 shows the detrended quarterly log volume of newly issued term loans and credit lines to U.S. nonfinancial borrowers over the period from 1995 to 2016. The two dashed horizontal lines denote two standard deviations around the mean volume over the whole period. As indicated by the two dashed vertical lines, between the fourth quarter of 2008 and the first quarter of 2010 credit activity in the syndicated loan market fell significantly below its long-run average.

[Figure 2 about here]

In principle, this observed drop in credit activity might not reflect a credit crunch but could be driven by a reduction in firms' credit demand. However, Ivashina and Scharfstein (2010) show that the reduction in syndicated lending in the wake of the financial crisis cannot be explained by demand effects alone. Likewise, Adrian, Colla, and Shin (2013) argue that the evidence from the 2008-09 financial crisis points overwhelmingly to a shock in the supply of credit by banks and other financial intermediaries.[7] The 2008-09 financial crisis furthermore also negatively affected the corporate bond market (Friewald, Jankowitsch, and Subrahmanyam, 2012; Adrian, Colla, and Shin, 2013), the commercial paper market (Kacperczyk and Schnabl, 2010), and firms' costs of issuing equity (McLean and Zhao, 2014). Firms facing the need to substitute maturing term loans

[7] In a similar vein, Almeida et al. (2012), Becker and Ivashina (2014), Campello, Graham, and Harvey (2010), and Chodorow-Reich (2014) also argue that the observed reduction in lending activity during the crisis can (mainly) be attributed to a reduction in credit supply rather than (purely) be attributed to a reduction in credit demand.

or expiring credit lines with other sources of external financing were therefore likely affected by the financial frictions affecting all modes of corporate funding.[8] Therefore, I define the period from 2008-Q4 to 2010-Q1 as the *treatment period* during which firms were subject to reduced access to external financing.[9]

Regarding the second assumption, the extent to which a firm is affected by this reduction in aggregate credit supply depends on the firm's need for external financing. Financing needs are particularly high for firms with maturing term loans or expiring credit lines. Thus, I define the treatment group to be firms i which had at least one loan facility j maturing during the period from 2008-Q4 to 2010-Q1. Conversely, I define the control group pool to be firms which had neither a term loan maturing nor a credit line expiring during this period. This definition of treatment firms is rather conservative as it contains firms for which the volume of maturing loans constitutes only a small share of the firm's overall corporate financing.[10]

Exploiting firms' refinancing needs for identification requires those needs to be exogenous to firms' performance in the years following the shock. As I show in Section III, the maturity of the median loan facility in my sample is five years. Whether a firm had a term loan maturing or credit line expiring during the 2009 credit crunch is therefore determined by financing decisions made several years in the past. Thus, it is plausible to assume that firms did not schedule their term loans to mature and credit lines to expire in anticipation of the refinancing difficulties they would face several years down the road (Duval, Hong, and Timmer, 2020).[11] Nonetheless, to alleviate concerns that the results are driven by other firm characteristics potentially correlated with firms'

[8]This is consistent with anecdotal evidence during the crisis. For example, Moody's Investor Services reported that "refunding risk for [U.S. investment-grade corporate bond issuers] in 2009 is particularly high given the current credit market conditions" (Moody's, 2009). My identification strategy further does not require the contraction in syndicated lending to be purely supply driven, but only that there was (besides possible demand effects) a significant and sizable reduction in aggregate credit supply that made it harder and costlier for firms to substitute maturing term loans or expiring credit lines with other sources of external finance.

[9]Section VI provides a robustness check using an alternative definition of the credit crunch period in the syndicated loan market.

[10]Section VI provides a robustness check showing that the results become even stronger, once I only include firms in the treatment group for which the share of maturing loans of overall financing is above a certain threshold. Figure A1 in the appendix illustrates the basic idea behind my identification strategy.

[11]Despite the contraction in credit supply in the wake of the financial crisis, it is still possible that firms with refinancing needs during this period were able to roll over maturing term loans or renew expiring credit lines, which would constitute a threat to my identification strategy. Figure A2 in the appendix shows the average quarterly outstanding loan volumes for firms in the treatment group and firms in the control group pool relative to the third quarter of 2008. The decrease in outstanding loan volumes for firms in the treatment group starting in the fourth quarter of 2008 indicates those firms were not able to fully refinance their maturing term loans and expiring credit lines.

refinancing needs, I combine my difference-in-differences approach with an appropriate matching methodology.

B. The Local Projection Difference-in-Differences Matching Estimation Approach

The quasi-experimental variation in how adversely firms were affected by the contraction in credit supply lends itself to a difference-in-differences research design. To alleviate concerns that my results are driven by other firm characteristics that were found to be associated with refinancing risk or firms' corporate policies, I combine the difference-in-differences approach with an appropriate matching methodology. In this paper, I use the bias-corrected Abadie and Imbens (2006) matching estimator, which has recently been used in the corporate finance literature by Almeida et al. (2012), Campello and Giambona (2013), Kahle and Stulz (2013), and Gropp, Mosk, Ongena, and Wix (2019).[12]

To each firm in my treatment group, I match one firm from the pool of control group firms to produce a balanced sample in terms of firm size, the investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage as of 2008-Q3, the quarter immediately before the treatment period. These matching covariates capture potential differences in firms' growth prospects, liquidity, profitability, and capital structure prior to the financial crisis. Additionally, I require an exact match on the 1-digit SIC industry code. The main outcome variables examined are the change in investment, the change in the logarithm of property, plant, and equipment (PPE) assets as a measure of firm growth, the change in the logarithm of employment, and the change in the logarithm of wages.

I estimate the average treatment effect on the treated (ATT) for the change in the outcome variables between 2008-Q3 (the quarter immediately before the treatment period) and each of the 23 post-treatment quarters from 2010-Q2 to 2015-Q4. Thus, I estimate

$$\widehat{\tau}^h_{ATT} = \frac{1}{N_T} \sum_{i:T_i=1} \left[\Delta^h Y_i - \Delta^h \tilde{Y}_i(0) \right] \quad \forall h = 1, \cdots, 23 \tag{1}$$

where N_T is the number of firms in the treatment group, $T_i = 1$ denotes the belonging of firm

[12] See Abadie and Imbens (2002) and Abadie and Imbens (2006) for a more detailed explanation and Abadie, Drukker, Herr, and Imbens (2004) for an implementation of this estimator.

i to the treatment group, $\Delta^h Y_i$ is the observed change in outcome variable Y between the pre-treatment period and the h^{th} quarter of the post-treatment period for treatment firm i, and $\Delta^h \tilde{Y}_i(0)$ the corresponding imputed value of the change in the outcome variable for the matched control firm. Separately estimating the treatment effect for each of the post-treatment quarters, instead of collapsing my sample into a single pre- and post-treatment period, allows me to disentangle short-run effects from long-run effects and trace out a treatment effect curve over time. This methodology is similar in spirit to the local projection method of estimating impulse response functions (Jordà, 2005), which is based on sequential regressions of the endogenous variable shifted forward in time.[13]

C. Measuring Firm-Level Wage Rigidity: The Wage Share of Payroll Adjustment

The role of wage rigidity in shaping firms' response to financial shocks has received little attention in the corporate finance literature so far. However, downward nominal wage rigidity (the lack of nominal wages cuts even during recessions) is a well-documented feature of the labor market (Dickens et al., 2007; Daly et al., 2012). The notion of wage rigidity suggests that financially constrained firms may find it easier to reduce labor costs by laying off workers rather than by lowering wages (Pischke, 2018). On the other hand, the concept of labor hoarding posits that, if feasible, adjusting wages while retaining workers allows firms to avoid the costs of firing, rehiring, and retraining workers, thereby putting them in a better position to expand when the economy recovers (Biddle, 2014). Thus, the inability or unwillingness of firms to reduce their payroll by cutting wages is likely to exacerbate the long-run effects of credit market disruptions on real firm outcomes.

I construct a novel firm-level measure of wage rigidity based on the premise that firms can essentially reduce their payroll expenses along two different margins: They can either lay off workers while keeping wages fixed, or they can lower wages while avoiding layoffs and keeping employment stable.[14] Pischke (2018) shows that employment fluctuations are stronger for occupations with more rigid wages than for occupations with more flexible wages. Thus, firms with a higher degree of wage rigidity can be expected to adjust their payroll via changes in the level of employment

[13]See Favara and Imbs (2015), Jordà, Schularick, and Taylor (2013), Jordà, Richter, Schularick, and Taylor (2021), and Mian, Sufi, and Verner (2017) for recent applications of local projection techniques in Finance.

[14]Due to data constraints, I abstract from other margins of labor cost adjustment, such as bonuses and non-pay benefits.

rather than via changes in the level of the average wage. Since the payroll of a firm i in quarter t is given by the product of its number of employees and its average wage paid, this implies:

$$\Delta Log\left(\text{Payroll}_{i,t}\right) = \Delta Log\left(\text{Employment}_{i,t}\right) + \Delta Log\left(\overline{\text{Wage}_{i,t}}\right) \tag{2}$$

I define my measure of wage rigidity for firm i as the wage share of payroll adjustment θ_i given by

$$\theta_i = \frac{1}{T}\sum_{t=1}^{T} \frac{\Delta Log\left(\overline{\text{Wage}_{i,t}}\right)}{\Delta Log\left(\text{Payroll}_{i,t}\right)} \tag{3}$$

where T is the number of quarters in the pre-treatment period over which the measure is calculated.

I define firms with low values of θ to have a high degree of wage rigidity and firms with high values of θ to have a low degree of wage rigidity. To assess how the presence of rigid wages affects firms' response to a credit supply shock, I split the treatment group of affected firms by the median value of θ into two subsamples: Financially shocked firms with a high degree of wage rigidity, and financially shocked firms with a low degree of wage rigidity:

$$\text{Rigid Wages}_i = \begin{cases} 1 \quad \text{if} \quad \text{Treatment}_i = 1 \quad \& \quad \theta_i < Q_{50}(\theta) \\ 0 \quad \text{if} \quad \text{Treatment}_i = 1 \quad \& \quad \theta_i \geq Q_{50}(\theta) \end{cases} \tag{4}$$

where Treatment_i is the treatment variable as described in in Section II.A, θ_i is the firm-level measure of wage rigidity as defined in Equation (3), and $Q_{50}(\theta)$ is the median value of θ for firms in the treatment group.

III. Data

I combine data from three different sources. I obtain loan-level data on syndicated loans from Refinitiv's Dealscan and LoanConnector database, quarterly data on firms' balance sheets and income statements from Compustat's North America Fundamentals Quarterly database, and data on wages from the Quarterly Workforce Indicators (QWI) data set of the Longitudinal Employer-Household Dynamics (LEHD) program of the U.S. Census Bureau.[15]

[15] The definitions of all variables used in the paper are summarized in Table A1 in the appendix.

A. Loan-Level Data

I obtain data on all term loans and credit lines of U.S. firms from 1985 to 2016 from the Refinitiv's Dealscan and LoanConnector database.[16] I distinguish between loan facilities maturing between 2008-Q4 and 2010-Q1 (treatment facilities), and facilities maturing either before 2008-Q4 or after 2010-Q1 (nontreatment facilities). As shown in Table I, the maturity of the median loan facility in my sample is 60 months—that is, five years. Thus, whether a firm had a term loan maturing or a credit line expiring during the credit crunch period from 2008-Q4 to 2010-Q1 is determined by financing decisions made several years in the past.

[Table I about here]

The unit of observation in the Dealscan database is a loan facility at the time of origination. To merge loan-level data from Dealscan with quarterly balance sheet data from Compustat, I calculate the outstanding amount of term loans and credit lines of firm j in quarter t using the maturity date contained in the database. The resulting dataset contains the volume of outstanding and maturing loan facilities at the firm-quarter level.

B. Firm Balance Sheet Data

I then merge this dataset with the Compustat North America Fundamentals Quarterly database using the Dealscan-Compustat Linking Database from Chava and Roberts (2008). I exclude all financial borrowers (SIC industry codes 6000 - 6999), all not-for-profit and governmental enterprises (SIC codes $>$ 8000), and all firms not incorporated in the U.S. according to Compustat's foreign incorporation code. My outcomes variable from Compustat are the change in investment, the change in the logarithm of PPE assets as a measure of firm growth, the change in the logarithm of employment.[17] I conduct my matching procedure to balance treatment and control firms in terms of pre-treatment values of size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I require an exact match on the 1-digit SIC industry code.

[16] For term loans and credit lines, I follow the definition of Berg, Saunders, and Steffen (2016).

[17] I define investment as the four-quarter moving average of the ratio of quarterly capital expenditures to the fourth lag of quarterly PPE assets. The Compustat item *capxy* represents year-to-date capital expenditures, which I first transform to reflect quarterly values. I then use the moving average of quarterly values to account for seasonality in capital expenditures.

All dependent growth variables are winsorized at the 5 percent level to reduce noise from extreme values, and all matching covariates are winsorized at the 1 percent level.

C. *Wage and Payroll Data*

I obtain wage data at the State × 4-digit NAICS industry × Firm-size level from the QWI dataset of the LEHD program of the U.S. Census Bureau.[18] The QWI dataset is based on unemployment insurance wage records and covers about 92 percent of all private nonfarm employment in the United States (Abowd et al., 2009). I obtain quarterly wage data on the average monthly earnings of employees with a stable job throughout the quarter. Following Kuehn, Simutin, and Wang (2017) and Tuzel and Zhang (2017), I merge the QWI data to firms in Compustat based on their 4-digit NAICS industry code, the state of their headquarters, and the number of employees. I calculate a firm's payroll by multiplying the average wage obtained from the QWI dataset with the firm's number of employees obtained from Compustat.

$$\text{Payroll}_{i,t} = \text{Wage}^{\text{QWI}}_{i,t} \times \text{Employment}^{\text{CS}}_{i,t} \tag{5}$$

I then use this payroll measure to calculate my measure of wage rigidity for each firm as defined in Equation (3) in Section II. One caveat to this analysis is that the QWI data contain information at the business establishment level. Thus, wage data will be subject to measurement error for firms with production facilities outside of their headquarters state. However, Chaney, Sraer, and Thesmar (2012) and Tuzel and Zhang (2017) conclude that headquarters and production facilities tend to cluster in the same state, making headquarters location a reasonable proxy for firm location.[19]

[18] The QWI database classifies firms into five size buckets based on the number of employees: 0-19, 20-49, 50-249, 250-499, and 500+ employees.

[19] While Compustat contains data on firms' staff expenses, this data item (*XLR*) is sparsely populated. To assess the validity of my combined QWI-Compustat payroll measure, I regress this variable against staff expenses from Compustat for firms for which both data sources are available. This regression yields a slope coefficient of 0.93 and an adjusted R^2 of 0.88. Figure A3 in the appendix illustrates the strong positive correlation between the two variables. Figure A4 in the appendix further shows the distribution of the wage share of payroll adjustment as defined in Equation (4). The figure illustrates that wage rigidity does not meaningfully differ between treatment and control firms and that the average wage share of payroll adjustment is below 40 percent, indicating that payroll expenses are largely adjusted via the employment margin.

IV. The Long-Run Real Effects of Banking Crises

A. Matching Quality

I first provide summary statistics of firms in the sample before and after matching. Panel A of Table II compares the mean values of the matching covariates for 736 treated firms and 1,013 nontreated firms in the unmatched sample as of 2008-Q3, the quarter immediately before the treatment period. I provide two different matching quality diagnostics for the balancedness of the sample: the standardized bias defined as the difference of sample means between treated and nontreated firms as a percentage of the square root of the average of sample variances in both groups, and the two-sample t-statistic for differences in means. Treated firms differ from nontreated firms along several important dimensions. The average treated firm is significantly larger than the average nontreated firm and has a lower pre-crisis investment ratio, lower cash holdings, a lower Q, a higher return on assets, and a higher long-term leverage. These differences between treated and nontreated firms emphasize the necessity of employing a matching procedure.

[Table II about here]

To each of the 736 treated firms in my sample I match one firm from the control group pool of 1,013 nontreated firms to produce a balanced sample in terms of the pre-treatment firm characteristics.[20] Additionally, I require an exact match on the 1-digit SIC industry code. Panel B of Table II shows the mean values of the matching covariates for treated firms and matched control firms after applying the matching procedure. The matched sample is balanced in terms of all matching covariates. The standardized bias ranges between 1.54 and 7.78 percent in absolute values, and the differences in means become statistically insignificant. This successful matching procedure alleviates concerns that my results might be driven by differences in firms' growth prospects, liquidity, profitability, and capital structure prior to the financial crises.

[20] Specifically, I match on log total assets, log PPE assets, investment ratios, the logarithm of the ratio of cash to total assets, and the logarithm of long-term leverage. This matching specification provides the best match in terms of all matching variables shown in Table II and perform best in terms of ensuring parallel pre-treatment trends.

B. *Investment and Growth Dynamics*

I examine how financially shocked firms adjust their investment and growth trajectories relative to unshocked firms over a period of six years following the credit supply shock. Identification in a difference-in-differences framework crucially relies on the parallel trends assumption to hold. Panel A of Figure 3 shows the evolution of the mean change in investment ratios relative to 2008-Q3 for both treated firms (solid blue line) and the sample of matched control firms (dashed red line). The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample, respectively. The figure shows that, prior to the financial crisis, there is no significant difference in the investment dynamics between the two groups of firms, as indicated by the overlapping 95 percent confidence intervals. Starting in 2008, there is a decrease in investment of similar magnitude for both treated and control firms. After bottoming out in the first quarter of 2010, however, control firms begin to increase their investment at a steeper rate than financially shocked firms. The figure shows a significant gap in investment ratios up until the first quarter of 2012, when the investment trajectories of the two groups of firms start to converge again.

[Figure 3 about here]

Panel B of Figure 3 shows the evolution of the mean change in the logarithm of PPE assets relative to 2008-Q3 for the two groups of firms. Similarly, there is no significant difference in growth dynamics between the two groups in the period prior to the financial crisis. Starting at the end of the treatment period, and consistent with the investment gap shown in Panel A, firms more adversely affected by the reduction in aggregate credit supply enter a lower growth trajectory following the credit crunch. Then, after the investment gap closes in 2013, treatment and control firms converge to parallel growth paths until the end of the sample period. However, as treated firms do not offset the gap in investment by investing more when credit becomes available again, there remains a persistent growth gap a full six years after the credit supply shock. Panels A and B of Figure 3 illustrate that investment and firm growth paths of treated and matched control firms are on parallel trends before the financial shock but diverge during and after the crisis.

Columns 1 and 2 of Table III report the matching estimation results for the ATT on the change in investment. Starting in the third quarter of 2010, financially shocked firms begin to significantly reduce investment by 1.5 percentage points relative to firms in the matched control group. Over the next two years, the investment gap ranges between 1.5 and 1.7 percentage points per quarter. These coefficients are of similar magnitude as those found in previous studies on the short-run effects of credit market disruptions on investment. Almeida et al. (2012) find a decrease in investment ratios of 2.5 percentage points on a quarterly basis, and Acharya, Eisert, Eufinger, and Hirsch (2018) report a decrease in capital expenditures of 6 percentage points over a period of eight quarters. But how do firms adjust their investment ratios in the long run once credit becomes available again? Beginning in the second quarter of 2012, two years after the end of the credit crunch, the investment gap starts to close and becomes statistically insignificant and close to zero in magnitude. However, there is no catch-up effect, and affected firms do not invest more than firms in the control group in the following years. Therefore, as reported in columns 3 and 4 of Table III, this temporary investment gap translates into a significant and persistent growth gap. Financially shocked firms grow 8.6 percentage points less from 2008-Q3 to 2013-Q1 than firms in the control group. This 9 percentage point growth gap does not close and remains statistically significant until the end of the sample period in 2015-Q4.

[Table III about here]

Figure 4 illustrates the treatment effect curves over time for investment and firm growth. The solid lines represent the matching estimates for the ATT based on the results in columns 1 and 3 of Table III, and the dashed lines represent the corresponding 95 percent confidence intervals. The two figures additionally plot the estimated effects for the pre-treatment period from 2006-Q1 to 2010-Q1 not reported in Table III, providing a formal test of the parallel trends assumption. Panel A illustrates the temporary drop in investment for treated firms over a period of two years following the credit supply shock. Once credit becomes available again, treated firms converge back to similar levels of investment as control firms, but without offsetting the temporary investment gap. As shown in Panel B, this temporary gap in investment translates into a persistent growth gap until the end of the sample period. These findings are consistent, both in terms of timing and magnitude, with the macroeconomic literature on financial frictions and provide novel micro-level evidence for the significant long-run real effects of banking crises.[21]

[21] Using a narrative-based measure of financial distress, Romer and Romer (2017) find that the decline in GDP following a financial crisis is significantly negative and persistent, bottoming out 3.5 years after the shock at 6.0 percent. Figure A5 in the appendix compares the treatment effect curve on firm growth in Panel B of Figure 4 to

[Figure 4 about here]

C. Employment and Wage Dynamics

Next, I investigate how financially shocked firms adjust their long-run employment and wage dynamics. Panels A and B of Figure 5 show the evolution of employment and wage growth, respectively, relative to 2008-Q3 for both treated firms (solid blue line) and the sample of matched control firms (dashed red line). While Panel A shows a small employment gap between treated and control firms following the financial crisis, Panel B illustrates that there were almost no differences in the post-crisis wage dynamics.

[Figure 5 about here]

Columns 1 and 2 of Table III report the matching estimation results for the ATT on employment growth. Financially shocked firms exhibit a decline in employment growth of up to 2.2 percentage points two years after the crisis. Although these employment effects are not statistically significant, their magnitude is comparable to the short-term effects found in previous studies. Studying the employment effects of various credit supply shocks, Cingano, Manaresi, and Sette (2016) report 1.8 percentage points lower employment growth in Italy, Bentolila, Jansen, Jiménez, and Ruano (2015) find employment losses of about 2.8 percentage points in Spain, and Chodorow-Reich (2014) reports a slightly larger 4 percentage point decline in employment in the United States. Columns 3 and 4 of Table III report the matching estimation results for the ATT on wage growth. I find small and statistically insignificant wage effects ranging between 0.1 and 1.2 percentage points in the first three years after the credit crunch, and slightly larger yet still insignificant effects of 1.6 percentage points after six years. These results are consistent with the considerable evidence of downward wage rigidity in the U.S. during and after the Great Recession (Daly, Hobijn, and Lucking, 2012; Fallick, Lettau, and Wascher, 2016).

[Table IV about here]

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the impulse response function from Romer and Romer (2017), showing that my results based on quasi-experimental micro-level evidence are similar in timing and magnitude to the macroeconomic findings in Romer and Romer (2017). While my effects are slightly larger in magnitude (-9.8 versus -6.0 percent), this is consistent with evidence of corporate investment being twice as volatile as output over the business cycle (Jordá, Schularick, and Taylor, 2017).

Figure 6 illustrates the treatment effect curves over time for employment and wage growth. Panel A illustrates the negative, albeit statistically insignificant employment gap starting at the end of the credit crunch period, and Panel B illustrates the weak response of firms' wage policies in the years after the credit crunch. Overall, I find little to no effects of the credit supply shock on employment and wage dynamics in the full sample of firms.

[Figure 6 about here]

V. The Role of Wage Rigidity

I now turn to the question how wage rigidity at the firm level affects firms' investment, growth, employment, and wage dynamics in response to a credit supply shock. While downward nominal wage rigidity is a well-documented feature of the labor market, its consequences have received relatively little attention in the corporate finance literature thus far. In the presence of rigid wages, financially constrained firms may find it easier to reduce labor costs by laying off workers rather than by lowering wages (Pischke, 2018). On the other hand, the concept of labor hoarding posits that, if feasible, adjusting wages while retaining workers allows firms to avoid the costs of firing, rehiring, and retraining workers, thereby putting them in a better position to expand when the economy recovers. Thus, the inability or unwillingness of firms to reduce their payroll expenses by cutting wages is likely to exacerbate the negative long-run effects of credit market disruptions on real firm outcomes.

A. The Role of Wage Rigidity: Methodology

To assess how the presence of rigid wages affects the response of firms to a credit supply shock, I split my treatment group of financially shocked firms into two subsamples as discussed in Section II.C: financially shocked firms with a high degree of wage rigidity, and financially shocked firms with a low degree of wage rigidity.

I then conduct two different matching exercises. First, to compare the treatment effect curves for the full sample with those for the two subsamples, I run the matching estimation described in

Section II.B separately for financially constrained firms with rigid wages

$$T_i^{\text{Rigid}} = \begin{cases} 1 \quad \text{if} \quad \text{Treatment}_i = 1 \quad \& \quad \text{Rigid Wages}_i = 1 \\ 0 \quad \text{if} \quad \text{Treatment}_i = 0 \end{cases} \tag{6}$$

and financially constrained firms with flexible wages

$$T_i^{\text{Flexible}} = \begin{cases} 1 \quad \text{if} \quad \text{Treatment}_i = 1 \quad \& \quad \text{Rigid Wages}_i = 0 \\ 0 \quad \text{if} \quad \text{Treatment}_i = 0 \end{cases} \tag{7}$$

and trace out the treatment effect curves on investment, growth, employment, and wages for each of the two groups.

This analysis, however, is subject to the caveat that wage rigidity at the firm level could be correlated with other firm characteristics that might explain the heterogeneity in firms' responses to a credit supply shock. The main concern in this regard is firm size. Previous literature has found that small firms tend to exhibit a higher degree of wage rigidity than large firms.[22] Large firms, however, are also likely to be less affected by a reduction in bank credit supply, as they tend to have better access to alternative sources of external finance in both bond and equity markets. To alleviate these concerns, I conduct a *within treatment group* matching exercise. I match affected firms with rigid wages to affected firms with flexible wages to produce a sample of treatment group firms that is balanced in terms of the relevant firm characteristics:

$$T_i^{\text{Rigid vs. Flexible}} = \begin{cases} 1 \quad \text{if} \quad \text{Treatment}_i = 1 \quad \& \quad \text{Rigid Wages}_i = 1 \\ 0 \quad \text{if} \quad \text{Treatment}_i = 1 \quad \& \quad \text{Rigid Wages}_i = 0 \end{cases} \tag{8}$$

I then run the matching estimation described in Section II.B for this sample and trace out the treatment effect curves on investment, growth, employment, and wages for financially shocked firms with rigid wages relative to financially shocked with flexible wages.

[22] Du Caju, Fuss, and Wintr (2007) find that wage rigidity is much higher for small firms, as large firms usually have firm-level collective wage agreements that enhance wage flexibility. Similarly, Avouyi-Dovi, Fougére, and Gautier (2013) report that negotiating wages is more costly for small firms, resulting in a lower frequency of wage changes.

B. *Affected Firms with Rigid and Flexible Wages versus Unaffected Firms*

This section presents the estimation results of the first matching exercise from Equations (6) and (7) and compares the treatment effect curves of financially shocked firms with rigid wages and with flexible wages to the matched control group of financially unshocked firms.

Column 1 of Table V reports the matching estimation results for the change in investment for financially shocked firms with rigid wages. I find a large and significant decrease in investment of up to 2.5 percentage points relative to unaffected firms in the matched control group. This decline in investment is larger in magnitude than the investment drop for the full sample of financially shocked firms reported in Table III. Beginning in the second quarter of 2012, the investment gap for affected firms with rigid wages starts to close and becomes statistically significant and close to zero in magnitude. As shown in Column 2, there is no significant drop in investment for affected firms with flexible wages. In line with Schoefer (2021), these findings suggest that wage flexibility at the firm level can mitigate the negative effects of restricted access to external finance. Columns 3 and 4 report the corresponding matching estimation results on firm growth. Consistent with the significant investment gap for affected firms with rigid wages, Column 3 shows a large and persistent growth gap of up to 12.2 percentage points relative to firms in the matched control group. This growth gap remains large and statistically significant until almost the end of the sample period. Conversely, and consistent with the respective investment results, the growth gap for affected firms with flexible wages is small in magnitude and statistically insignificant, as shown in Column 4.

[Table V about here]

Columns 1 and 2 of Table VI report the matching estimation results for employment growth for affected firms with rigid and flexible wages, respectively. While largely insignificant, the effect of the credit supply shock on employment growth is negative and large in magnitude for firms with rigid wages. Those firms exhibit an additional decline in employment growth of up to 3.6 percentage points compared to financially unshocked firms in the matched control group. On the other hand, employment effects are insignificant and small in magnitude for financially shocked firms with flexible wages. Columns 3 and 4 report the matching estimation results on wage growth. While there are no significant effects on the wage policies of affected firms with rigid wages, Column 4 shows that financially shocked firms with flexible wages significantly reduce wages in response to the credit supply shock.

[Table VI about here]

Figure 7 traces out the treatment effect curves over time for financially shocked firms with rigid wages based on the matching estimates in Columns 1 and 2 of Tables V and VI. Panel A illustrates the significant drop in investment and Panel B the resulting large growth gap for financially shocked firms with rigid wags. Panels C and D illustrate how those firms adjust their labor costs following the crisis by reducing employment rather than by lowering wages.

[Figure 7 about here]

Analogously, Figure 8 traces out the treatment effect curves for financially shocked firms with flexible wages based on the matching estimates in Columns 3 and 4 of Tables V and VI. Panel A shows that affected firms with flexible wages do not significantly reduce investment in response to the credit supply shock and thus, as shown in Panel B, do not exhibit a significant growth gap in the years following the crisis. Panels C and D illustrate how affected firms with flexible wages reduce labor costs by cutting wages while keeping employment stable. Thus, Figure 8 illustrates how wage flexibility at the firm level can mitigate the negative effects of a credit supply shock on investment and firm growth.

[Figure 8 about here]

Finally, Figure 9 compares the treatment effect curves for the full sample with the treatment effect curves for affected firms with rigid wages and affected firms with flexible wages, respectively. The solid black lines represent the matching estimates for the full sample estimated in Section IV; the dashed blue lines represent the corresponding estimates for affected firms with rigid wages; and the dotted red lines represent the corresponding estimates for affected firms with flexible wages. Confidence intervals are omitted for clarity. Panels A and B show that financially shocked firms with rigid wages exhibit a steeper drop in investment, resulting in a larger accumulated growth gap relative to financially shocked firms with flexible wages. Panels C and D illustrate the stark difference in the wage and employment responses. Firms with rigid wages reduce labor costs by reducing employment, while firms with flexible wages cut wages and keep employment stable. These findings are consistent, both in terms of timing and magnitude, with findings in the macroeconomic literature on financial frictions that wage rigidity is a necessary feature to create amplification of financial shocks (Ajello, 2016).[23]

[Figure 9 about here]

C. *Affected Firms with Rigid Wages versus Affected Firms with Flexible Wages*

To alleviate concerns that my results in the previous section are driven by other firm characteristics correlated with wage rigidity, especially firm size, I next conduct the within treatment group matching exercise from Equation (8). I first provide summary statistics for financially shocked firms with rigid and flexible wages. Panel A of Table VII compares the 334 affected firms with rigid wages and the 325 affected firms with flexible wages in the unmatched sample. In line with the empirical literature on wage rigidity, I find that firms with rigid wages are significantly smaller than firms with flexible wages. Moreover, financially shocked firms with rigid wages have a higher pre-crisis investment ratio, lower cash flow, and higher long-term leverage relative to financially shocked firms with flexible wages.

[Table VII about here]

To each of the 334 financially shocked firms with rigid wages, I match one firm from the control group pool of financially shocked firms with flexible wages to produce a balanced sample in terms of the relevant firm characteristics. Panel B of Table VII shows the mean values of the matching covariates for the two groups of firms after matching. While statistically significant differences remain in terms of the return on assets and the long-term leverage, the matched sample is balanced in terms of all other matching covariates, most importantly firm size.

Table VIII reports the matching estimation results for investment, firm, employment, and wage growth. Column 1 shows that financially shocked firms with rigid wages exhibit an additional decline in investment of up to 2.7 percentage points relative to financially shocked firms with flexible wages. There are significant differences in investment ratios between the two groups of

[23] Ajello (2016) calibrates a dynamic general equilibrium model with financial frictions and estimates the response of, inter alia, investment to a negative financial shock. The corresponding impulse response function in the model with rigid wages shows a temporary investment gap of about 2 percent that closes after 12 quarters. Furthermore, the decline in investment is less steep and the recovery much quicker in the model with flexible wages. Figure A6 in the appendix compares the treatment effect curves on investment in Panel A of Figure 9 to the impulse response function from Ajello (2016), showing that I obtain very similar results for financial shocked firms with rigid and flexible wages, both in terms of timing and magnitude.

firms about two years after the shock. As shown in Column 2, this temporary investment gap translates into a growth gap of up to 13 percentage points three years after the crisis. While the growth gap becomes statistically insignificant at the end of the sample period, its magnitude remains large at about 11 percentage points. Columns 3 and 4 show the differential adjustment of employment and wage trajectories in response to the credit supply shock. Financially shocked firms with rigid wages reduce employment by up to 6.6 percentage points more and wages by up to 2.2 percentage points less than financially shocked firms with flexible wages.

[Table VIII about here]

Figure 10 illustrates the treatment effect curves for financially shocked firms with rigid wages relative to financially shocked firms with flexible wages based on the matching estimates in Table VIII. Panel A illustrates how financially shocked firms with rigid wages reduce investment compared to financially shocked firms with flexible wages. As shown in Panel B, this translates into a growth gap that remains large in magnitude until the end of the sample period. Panels C and D illustrate the relative reduction in employment and the relative increase in wages for financially shocked firms with rigid wages compared to firms with financially shocked flexible wages.

[Figure 10 about here]

D. Policy Implications: A Tale of Two Countries and Two Crises

Taken together, these findings provide novel firm-level evidence on the role of wage rigidity for the amplification of financial shocks. Moreover, the results are consistent with the notion of labor hoarding, which posits that, if feasible, adjusting wages while retaining workers might put firms in a better position to expand when the economy recovers. Wage flexibility might thus mitigate the negative long-run effects of banking crises. This section discusses the policy response in Germany during the financial crisis and in the United States during the COVID-19 crisis in the context of my empirical findings.

Like the United States, Germany experienced a credit crunch (Puri, Rocholl, and Steffen, 2011) and a similarly severe recession during the 2008-09 financial crisis. However, unlike in the United States, there was virtually no rise in unemployment in Germany during the crisis, an economic development that has been dubbed the "German labor market miracle" (Burda and Hunt, 2011).[24] German GDP also recovered slightly faster to pre-crisis levels than U.S. GDP and significantly faster than the GDP in other major economies (CEA, 2014). The resilience of the German labor market and the fast recovery of the German economy has often been attributed to short-time work ("Kurzarbeit") programs subsidized by the government.[25] Under these short-time work programs, firms refrain from layoffs but instead reduce workers' hours. Workers are paid the wage for the actual hours worked plus a compensation ("Kurzarbeitergeld") between 60 and 67 percent of the net pay for the hours not demanded. Firms are later reimbursed for these expenses by the German Federal Employment Agency. During the financial crisis, the German government expanded the short-time work scheme by prolonging the duration of firm subsidies and by reducing the required minimum number of affected workers (Burda and Hunt, 2011). These policy measures effectively constituted an injection of wage flexibility into the German labor market, as firms were able to reduce labor costs by lowering their wage bill without laying off workers. The fast recovery of the German economy following the crisis is consistent with my results that more flexible wages might mitigate the long-run real effects of banking crises.

More recently, the United States implemented a similar policy measure in response to the COVID-19 crisis. The PPP was established through the CARES Act, which was passed on March 27, 2020.[26] The goal of the PPP was to provide forgivable loans to small and medium-sized firms to help them pay their employees and therefore prevent layoffs and preserve employment relationships (Hubbard and Strain, 2020; Granja et al., 2022). U.S. Congress disbursed \$349 billion through the PPP in the first two weeks of April and, following the exhaustion of the funds, allocated an additional \$320 billion by the end of April. Importantly, loans could be forgiven under, inter alia, the conditions that firms maintained employment at close to pre-crisis levels and spent at least 60 percent of the loan amount on payroll expenses (Autor et al., 2022a). Ultimately, 96 percent of PPP loans were forgiven, effectively rendering the PPP a subsidy for firms' payroll expenses (Autor et al., 2022b). The PPP meaningfully helped to preserve an additional 3.6 million jobs during the

[24] See Giroud and Mueller (2017) for a similar discussion.

[25] For a discussion of labor hoarding and short-time work in Germany during the financial crisis, see Burda and Hunt (2011), Cahuc and Carcillo (2011), Brenke, Rinne, and Zimmermann (2013), Balleer, Gehrke, Lechthaler, and Merkl (2016), and Giroud and Mueller (2017).

[26] For a detailed discussion about the PPP, its design, and its effects, see Hubbard and Strain (2020), Doniger and Kay (2021), Autor et al. (2022a), Autor et al. (2022b), and Granja et al. (2022).

early stages of the pandemic (Autor et al., 2022b), possibly contributing to the fast recovery of the U.S. economy in the second quarter of 2020 (Hubbard and Strain, 2020). Thus, similar to the German "Kurzarbeit", the PPP allowed firms to reduce payroll expenses without laying off employees, thereby constituting an injection of wage flexibility into the labor market.

There are, however, also caveats regarding these policy measures. First, while incentives for firms to engage in labor hoarding might be optimal during (temporary) downturns, they might also prevent the efficient reallocation of labor across firms in the long run (Brenke, Rinne, and Zimmermann, 2013; Giroud and Mueller, 2017). Second, as means testing whether a firm is truly in "unavoidable financial difficulties" (Burda and Hunt, 2011) is challenging, subsidies might go to firms that would have possibly refrained from layoffs even in the absence of government intervention. Payroll subsidies might therefore be an effective—but inefficient and expensive—policy measure (Autor et al., 2022b; Granja et al., 2022).

VI. Robustness Checks

A. Alternative Definition of the Treatment Period

In my baseline specification, I define the period from 2008-Q4 to 2010-Q1 as the credit crunch period in the syndicated loan market. During these six quarters, credit activity in the syndicated loan market was significantly below its long-run average. In this section, I perform a robustness check using a narrower definition of the treatment period, from 2009-Q1 to 2009-Q4. As the credit availability in the syndicated loan market was at its lowest in 2009 (Becker and Ivashina, 2014), I expect the results to be stronger using this narrower definition of the treatment period. Table IX presents the results of this robustness check. Column 1 shows that firms more adversely affected by the reduction in aggregate credit supply from 2009-Q1 to 2009-Q4 reduce investment by up to 2.0 percentage points relative to firms in the matched control group over a period of two years following the crisis. As shown in Column 2, this temporary investment gap results in a persistent growth gap of 12 percentage points. As expected, both the temporary drop in investment as well as the persistent growth gap are larger in magnitude than for the baseline results in Table III.

[Table IX about here]

B. Alternative Definitions of the Treatment Group

In my baseline specification, I define the treatment group to be firms that had at least one term loan or credit line maturing during the period from 2008-Q4 to 2010-Q1, and I define the control group pool to be firms that had neither a term loan nor a credit line maturing during this period. This definition of the treatment group is rather conservative and might include firms for which the volume of maturing loans constitutes only a small share of its overall corporate financing. As such firms have lower refinancing risk, they should be only marginally affected by the reduction in aggregate credit supply. To address this concern, I perform a robustness check and define the treatment group to be firms for which the volume of maturing loans during the credit crunch period exceeds 10 percent of the firm's total assets. The control group pool remains unchanged and consists of firms that had neither a term loan nor a credit line maturing during this period. As firms with a larger volume of maturing loans are expected to be more adversely affected by the reduction in credit supply, I expect the results to be stronger using this stricter definition of the treatment group. Table X presents the results of this robustness check. Column 1 shows that firms for which the volume of maturing loans during the credit crunch period exceeds 10 percent of total assets reduce investment by up to 2.4 percentage points relative to firms in the matched control group in the years after the crisis. As shown in Column 3, this temporary investment gap results in a persistent growth gap of 12.2 percentage points, respectively. Again, as expected, both the temporary drop in investment as well as the persistent growth gap are larger in magnitude than for the baseline results in Table III.

[Table X about here]

VII. Conclusion

This paper investigates the long-run effects of a banking crisis on real firm outcomes and how these effects depend on nominal wage rigidity at the firm level. Financially shocked firms reduce investment over a period of two years following a credit supply shock before returning to normal levels of investment. As these firms do not offset the temporary investment gap, the credit supply shock results in a persistent and significant accumulated growth gap a full six years after the shock. Wage rigidity at the firm level significantly exacerbates the negative long-run effects of banking crises. Financially shocked firms with higher levels of wage rigidity exhibit a steeper drop in investment and grow more slowly than financially shocked firms with flexible wages.

Since the 2008-09 financial crisis, an extensive literature in both corporate finance and macroeconomics examines the effects of financial frictions on the real outcomes. To date, these two strands of literature remain largely disconnected regarding two important aspects: the long-run effects of financial shocks, and the role of wage rigidity in amplifying these shocks. While these two aspects are central topics in the macroeconomic literature on financial frictions, they have received little attention in the empirical corporate finance literature thus far. Studying these questions with aggregate data has several drawbacks. First, identifying exogenous shocks to credit supply is difficult when using aggregate data. Second, "measuring recovery" requires a credible counterfactual of how economic activity would have evolved in the absence of such a shock. In light of these empirical challenges, Romer and Romer (2017) suggest that "the most fruitful approach to establishing causation may lie in combining natural experiments with detailed cross-section evidence." By studying the long-run effects of credit market disruptions on real firm outcomes, my paper provides such quasi-experimental micro-level evidence.

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(A) Investment **(B)** Employment **(C)** Wages

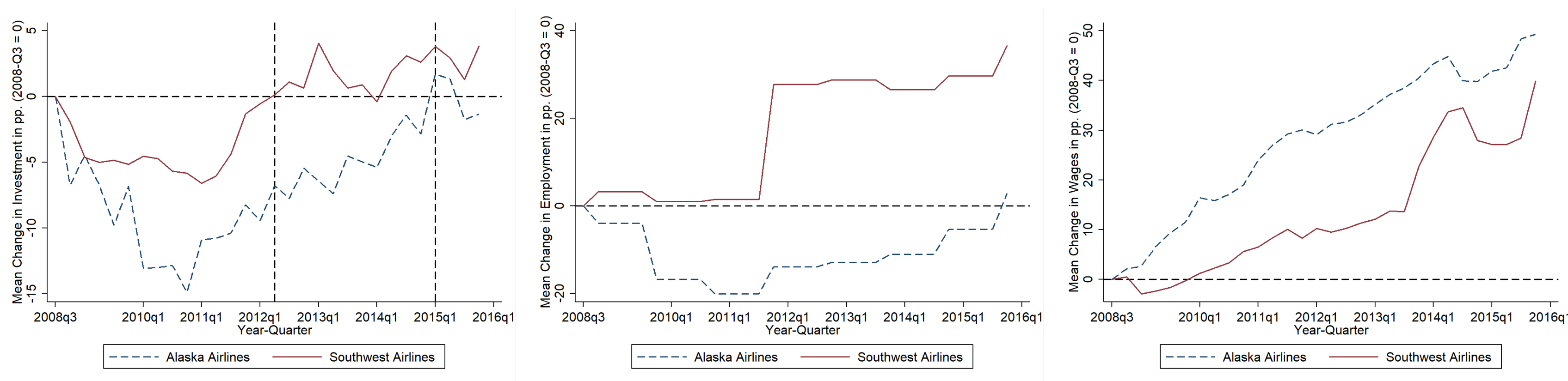


Figure 1. Illustrative Example: Alaska Airlines versus Southwest Airlines. This figure illustrates the change in investment ratios (Panel A), employees (Panel B), and wages (Panel C) relative to 2008-Q3 for Southwest Airlines (solid red line) and Alaska Airlines (dashed blue line). Investment and employment dynamics are calculated from the Compustat North America Fundamentals Quarterly database, and wage dynamics are approximated based on the Quarterly Workforce Indicators (QWI) dataset. All data sources are described in detail in Section III.

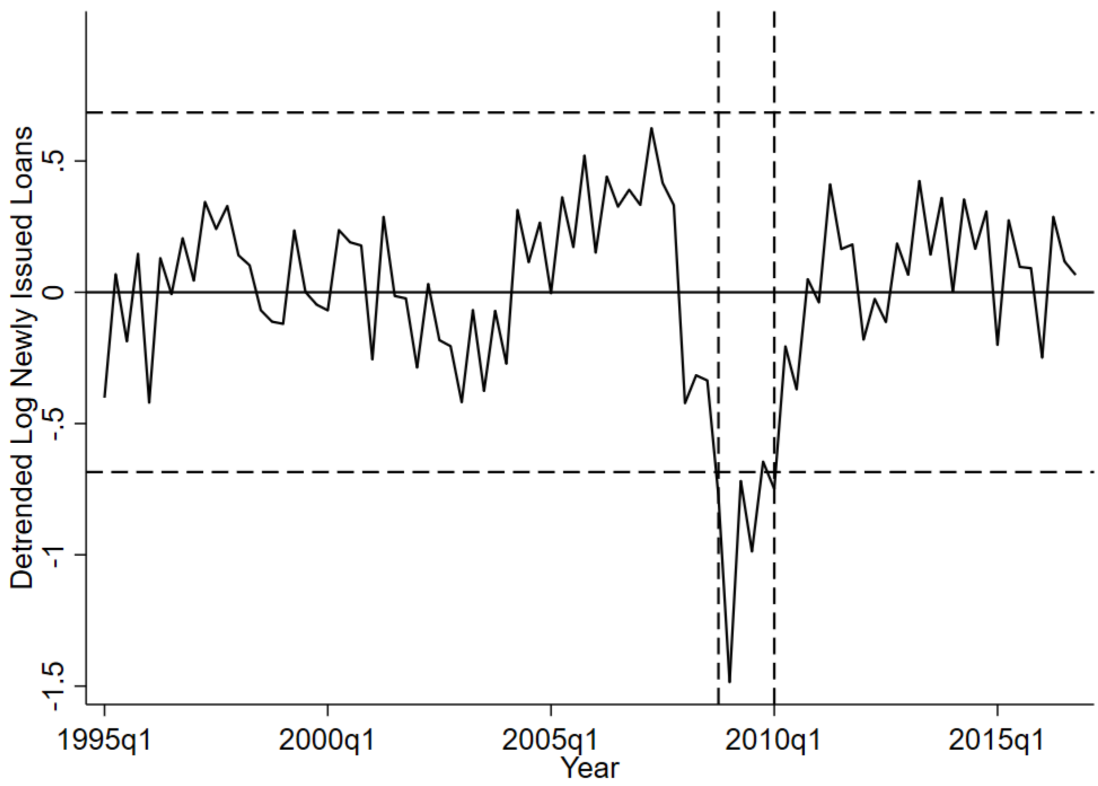
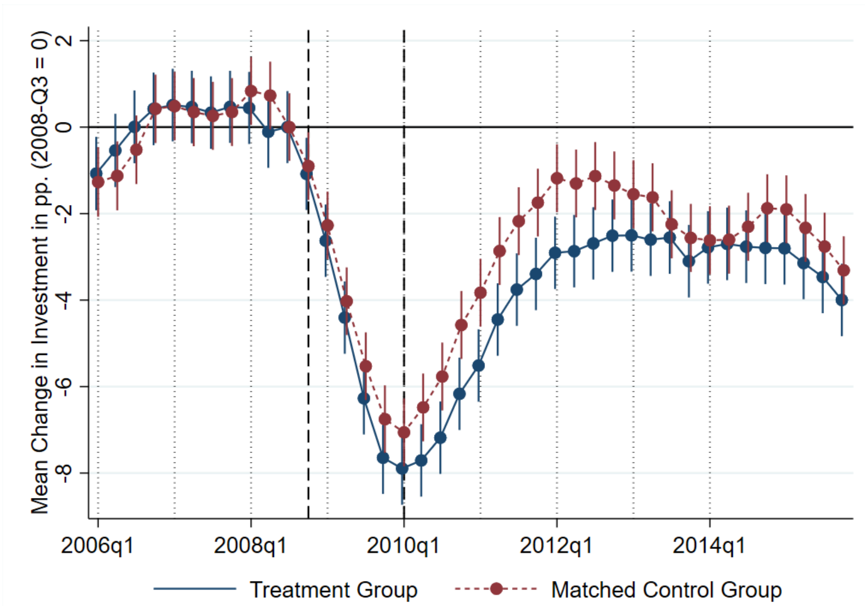


Figure 2. Newly Issued Loans by Quarter. This figure shows the detrended quarterly total log volume of newly issued term loans and credit lines to U.S. nonfinancial borrowers over the period from 1995 to 2016. The two dashed horizontal lines denote two standard deviations around the mean volume of newly originated loans over the whole period. The two vertical lines mark the quarters 2008-Q4 and 2010-Q1, the period during which credit activity fell significantly below the long-run average of loan originations. This time window defines the credit crunch period in the syndicated loan market, which I use as the treatment period in this paper.

(A) Investment Ratios



(B) Firm Growth

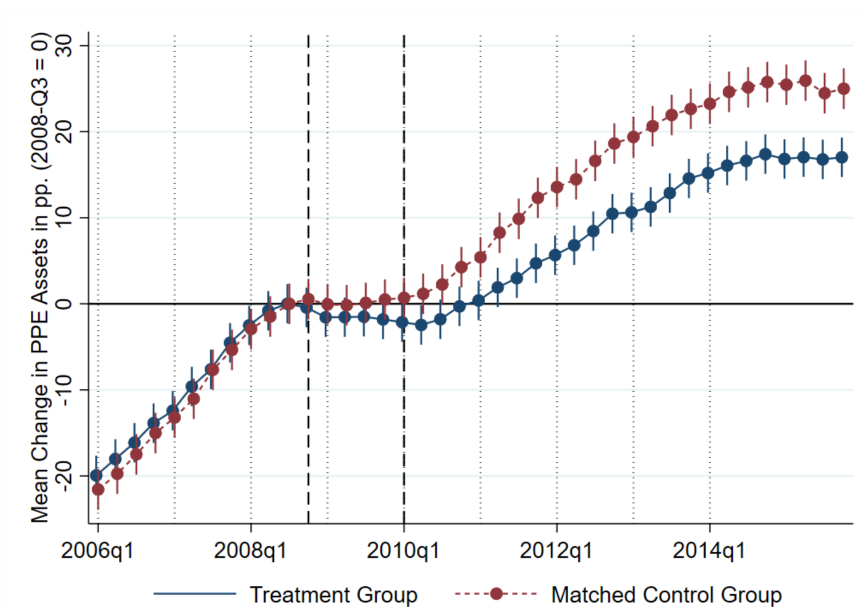
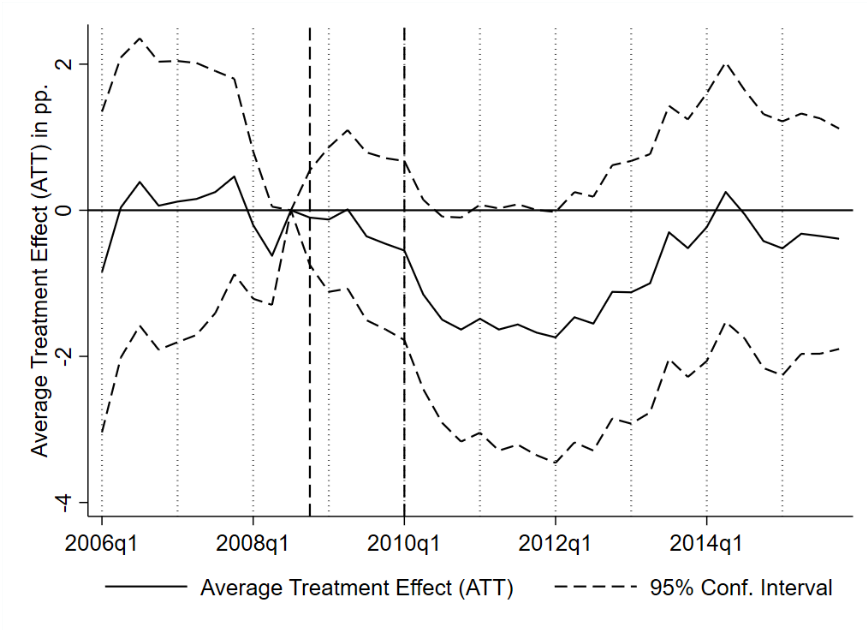


Figure 3. Mean Change in Investment Ratios and Firm Growth Over Time. This figure shows the evolution of the mean change in investment ratios (Panel A) and the mean change in the logarithm of property, plant, and equipment (PPE) assets (Panel B) relative to 2008-Q3 for both treated firms (solid blue line) and matched control firms (dashed red line). Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms in the matched control group have neither a term loan maturing nor credit line expiring during this period. The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample. The whiskers mark the 95% confidence intervals.

(A) Investment Ratios



(B) Firm Growth

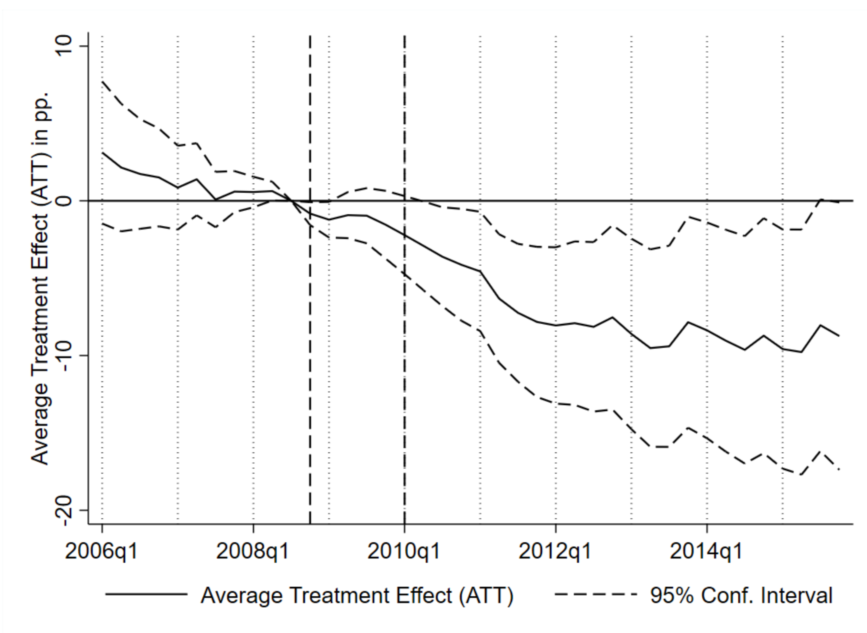
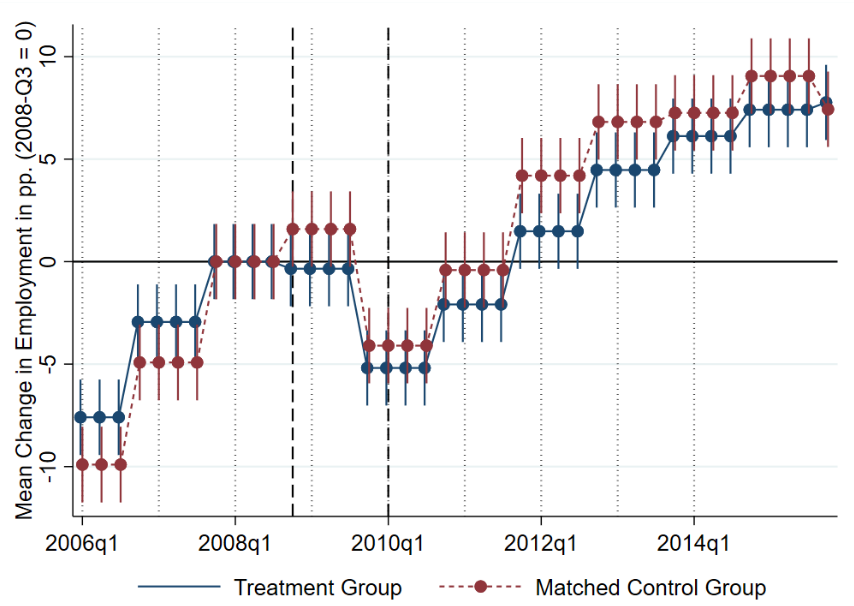


Figure 4. Treatment Effect Curve Over Time: Investment and Firm Growth. This figure shows the average treatment effect on the treated (ATT) on the change in investment (Panel A) and the change in the logarithm of property, plant, and equipment (PPE) assets (Panel B) between 2008-Q3 and multiple pre- and post periods from 2006-Q1 to 2015-Q4. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms in the matched control group have neither a term loan maturing nor credit line expiring during this period. The solid line represents the matching estimates for the ATT based on the results in Table III. The two dashed lines represent the 95% confidence intervals. The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

(A) Employment



(B) Wages

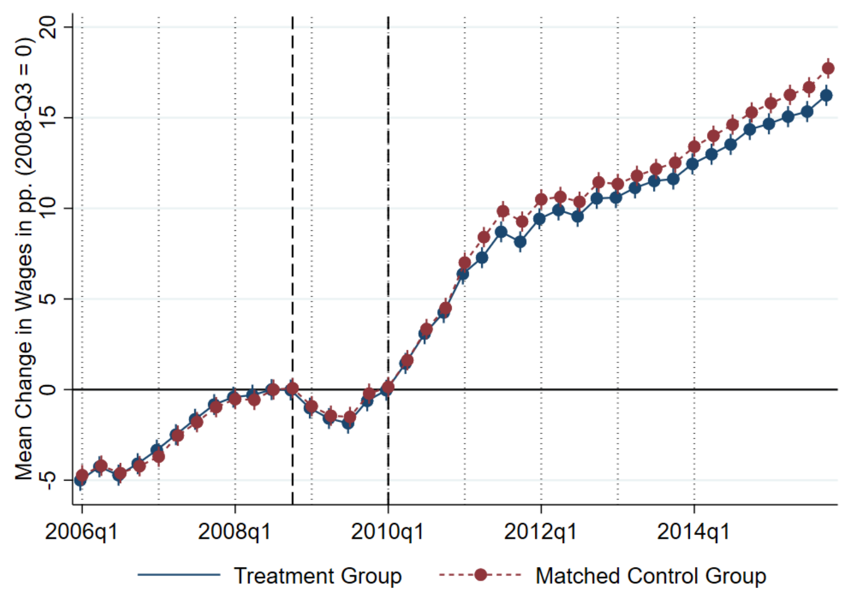
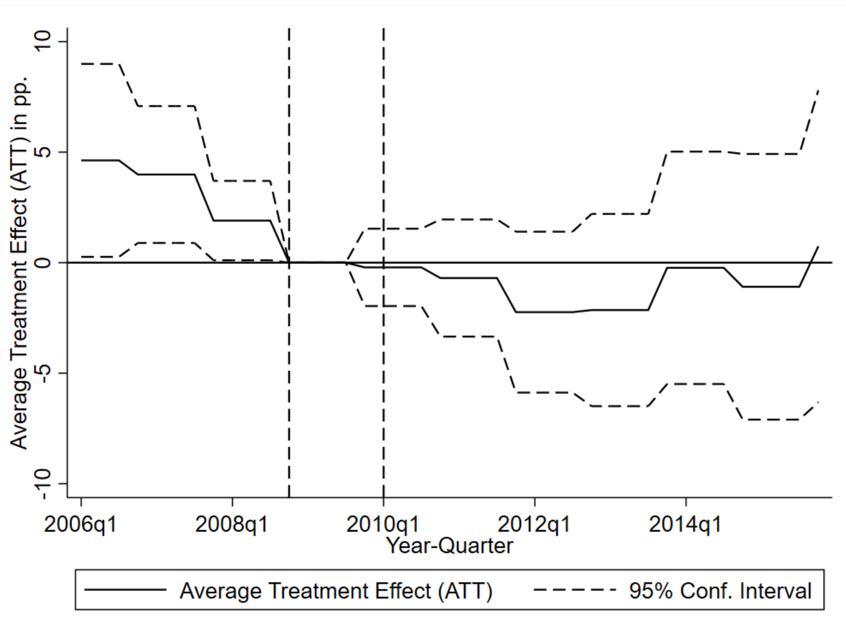


Figure 5. Mean Change in Employment and Wages Over Time. This figure shows the evolution of the mean change in the logarithm of the number of employees (Panel A) and the mean change in the logarithm of wages (Panel B) relative to 2008-Q3 for both treated firms (solid blue line) and matched control firms (dashed red line). Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms in the matched control group have neither a term loan maturing nor credit line expiring during this period. The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample. The whiskers mark the 95% confidence intervals.

(A) Employment



(B) Wages

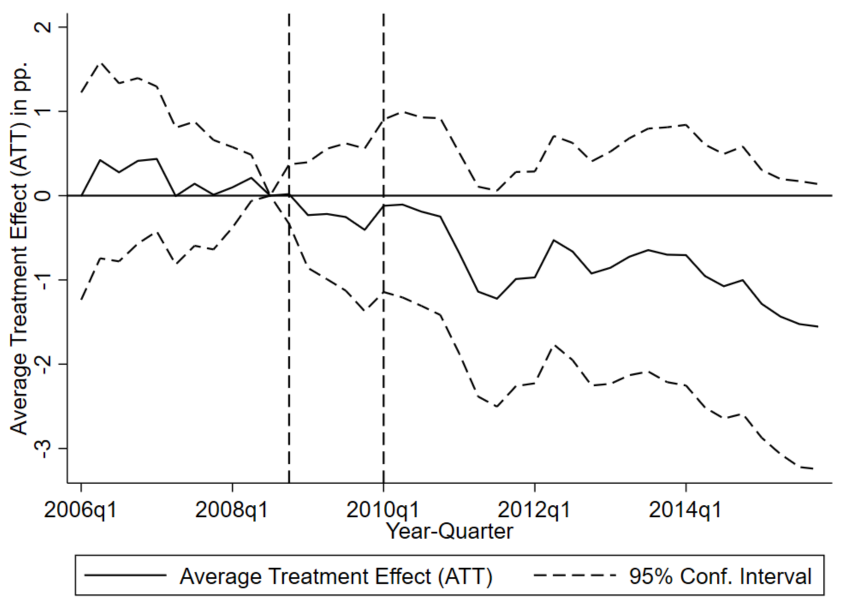


Figure 6. Treatment Effect Curve Over Time: Employment and Wage Growth. This figure shows the average treatment effect on the treated (ATT) on the change in the logarithm of employees (Panel A) and the change in the logarithm of wages (Panel B) between 2008-Q3 and multiple pre- and post periods from 2006-Q1 to 2015-Q4. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms in the matched control group have neither a term loan maturing nor credit line expiring during this period. The solid line represents the matching estimates for the ATT based on the results in Table III. The two dashed lines represent the 95% confidence intervals. The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

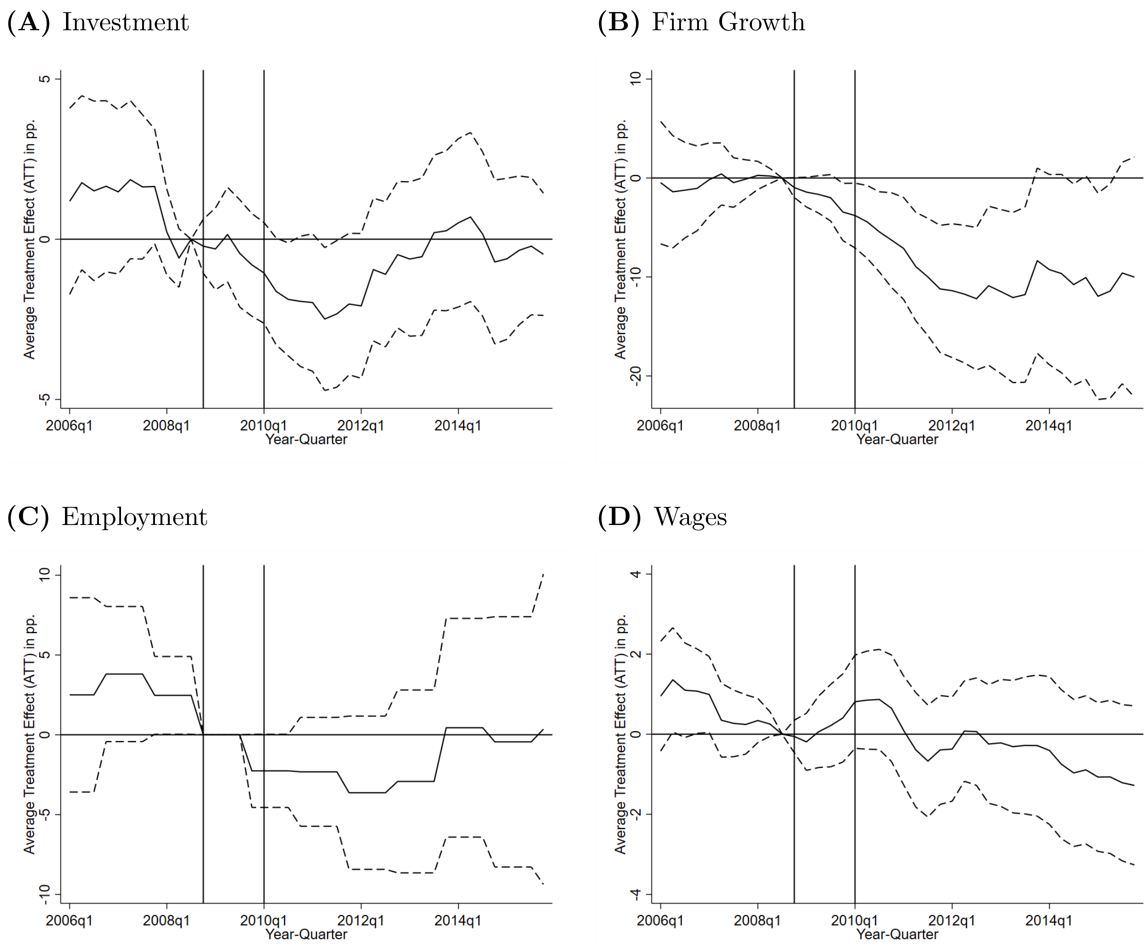
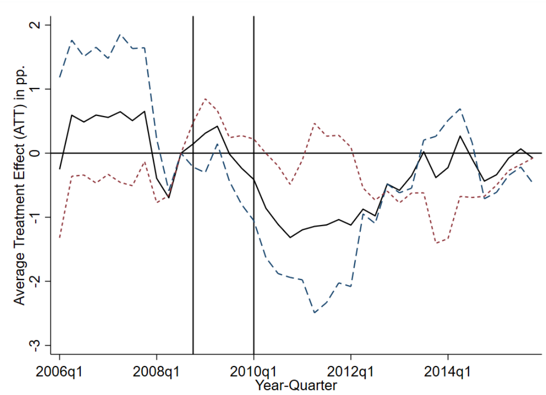


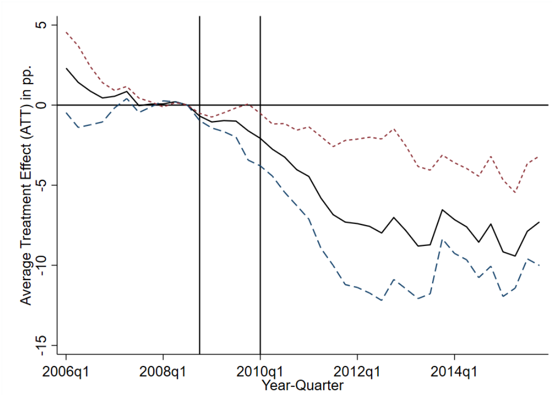
Figure 7. Financially Shocked Firms with Rigid Wages: Treatment Effect Curves Over Time. This figure shows the average treatment effect on the treated (ATT) on the change in investment (Panel A), the change in the logarithm of property, plant, and equipment (PPE) assets (Panel B), the change in the logarithm of employment (Panel C), and the change in the logarithm of wages (Panel D) between 2008-Q3 (the last quarter before the credit supply shock) and multiple pre- and post periods from 2006-Q1 to 2015-Q4. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have a below-median wage share of payroll adjustment, while nontreated firms in the matched control group have neither a term loan maturing nor credit line expiring during this period. The solid lines in each panel represents the matching estimates for the ATT based on the corresponding results in Table V and Table VI. The two dashed lines in each panel represent the 95% confidence intervals. The two vertical lines in each panel mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

Figure 8. Affected Firms with Flexible Wages: Treatment Effect Curves Over Time. This figure shows the average treatment effect on the treated (ATT) on the change in investment (Panel A), the change in the logarithm of property, plant, and equipment (PPE) assets (Panel B), the change in the logarithm of employment (Panel C), and the change in the logarithm of wages (Panel D) between 2008-Q3 (the last quarter before the credit supply shock) and multiple pre- and post periods from 2006-Q1 to 2015-Q4. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have an above-median wage share of payroll adjustment, while nontreated firms in the matched control group have neither a term loan maturing nor credit line expiring during this period. The solid line in each panel represents the matching estimates for the ATT based on the corresponding results in Table V and Table VI. The two dashed lines in each panel represent the 95% confidence intervals. The two vertical lines in each panel mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

(A) Investment



(B) Firm Growth



(C) Employment

(D) Wages

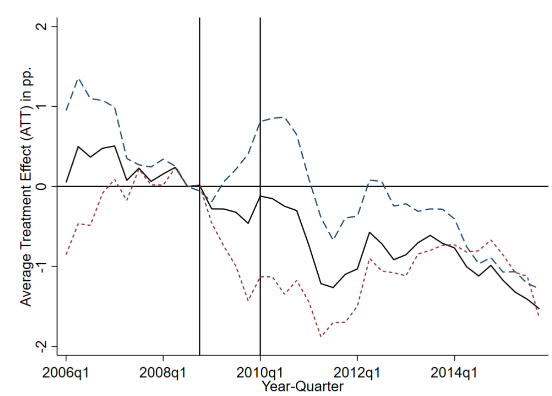


Figure 9. Full Sample, Affected Firms with Rigid Wages, and Affected Firms with Flexible Wages: Comparing Treatment Effect Curves Over Time. This figure compares the average treatment effects on the treated (ATT) on the change in investment (Panel A), the change in the logarithm of property, plant, and equipment (PPE) assets (Panel B), the change in the logarithm of employment (Panel C), and the change in the logarithm of wages (Panel D) between 2008-Q3 (the last quarter before the credit supply shock) and multiple pre- and post periods from 2006-Q1 to 2015-Q4. The solid black line in each panel represents the matching estimates based on the full sample (Figures 4A, 4B, 6A, and 6B), the dashed blue line in each panel represents the matching estimates based on treatment firms with rigid wages (Figure 7), and the dotted red line in each panel represents the matching estimates based on treatment firms with flexible wages (Figure 8). Confidence bands are omitted for clarity. The two vertical lines in each panel mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

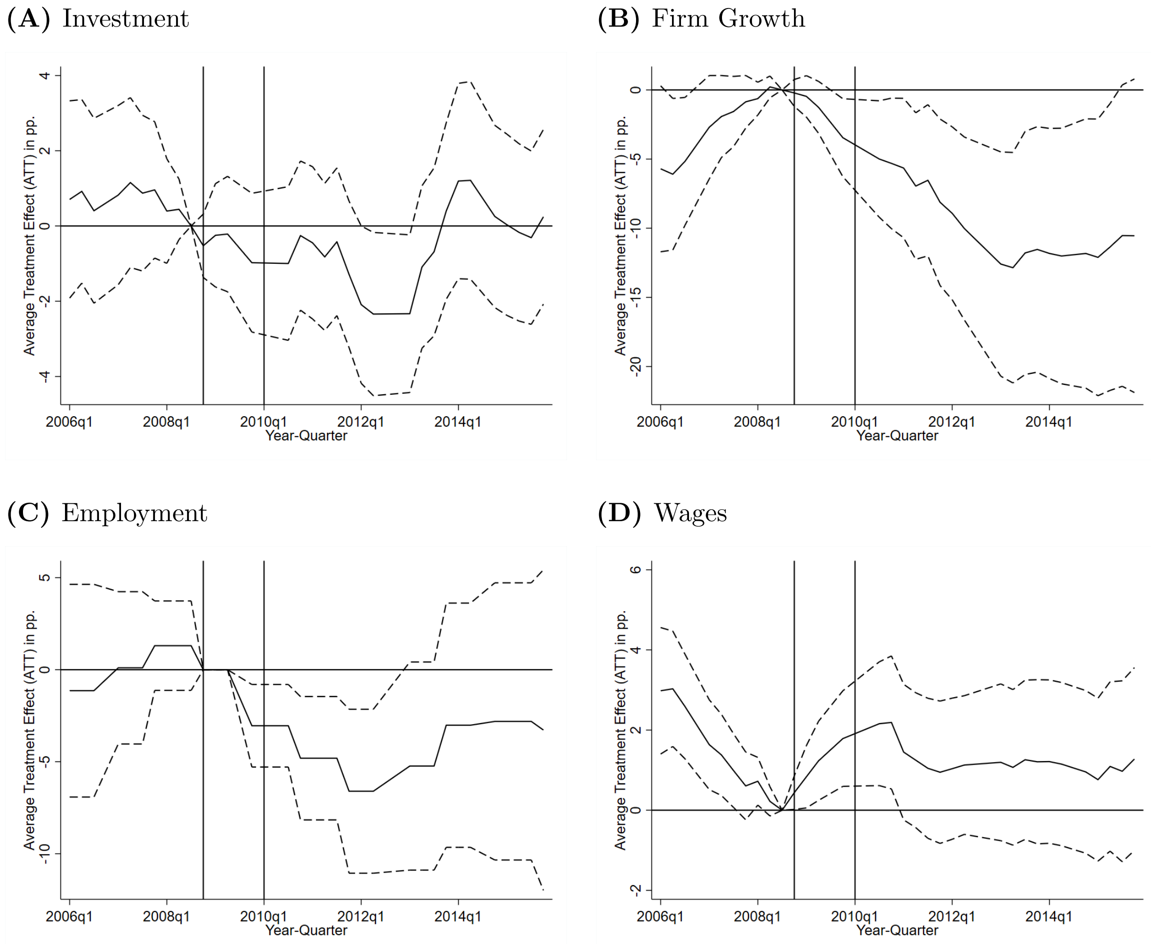


Figure 10. Affected Firms with Rigid Wages versus Affected Firms with Flexible Wages: Treatment Effect Curves Over Time. This figure shows the average treatment effect on the treated (ATT) on the change in investment (Panel A), the change in the logarithm of property, plant, and equipment (PPE) assets (Panel B), the change in the logarithm of employment (Panel C), and the change in the logarithm of wages (Panel D) between 2008-Q3 (the last quarter before the credit supply shock) and multiple pre- and post periods from 2006-Q1 to 2015-Q4. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have a below-median wage share of payroll adjustment. Nontreated firms in the matched control group are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have an above-median wage share of payroll adjustment. The solid line in each panel represents the matching estimates for the ATT based on the results in Table VIII. The two dashed lines in each panel represent the 95% confidence intervals. The two vertical lines in each panel mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

Table I
Summary Statistics: Syndicated Loans

This table reports the summary statistics of 1,281 treatment facilities and 6,018 nontreatment facilities at the individual loan level. A *treatment facility* is a term loan maturing or credit line expiring between 2008-Q4 and 2010-Q1, and a *nontreatment facility* is a term loan maturing or credit line expiring either before 2008-Q4 or after 2010-Q1. The table reports the means, standard deviations (SD), and the 10^{th}, 50^{th}, and 90^{th} percentiles of the distributions of the respective sample of loan facilities. The variable *Term Loan Indicator* takes the value of 1 if the facility is a term loan and 0 if the facility is a credit line. The variable *Credit Line Indicator* vice versa takes the value of 1 if the facility is a credit line and 0 if the facility is a term loan. For definitions of term loans and credit lines, I follow Berg, Saunders, and Steffen (2016).

	Treatment Facilities						Nontreatment Facilities					
	#	Mean	SD	10th	50th	90th	#	Mean	SD	10th	50th	90th
Facility Volume ($M)	1281	481	1173	25	200	1500	6018	581	1019	40	291	1468
Maturity (Months)	1281	50	24	12	60	70	6018	59	23	36	60	83
Term Loan Indicator		0.35	0.48					0.34	0.47			
Credit Line Indicator		0.65	0.48					0.66	0.47			

Table II
Matching Quality

This table provides pre-treatment summary statistics on treated firms, nontreated firms, and matched control firms. Panel A compares the mean values of firm characteristics of 736 treated and 1013 nontreated firms in the unmatched sample. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms have neither a term loan maturing nor credit line expiring during this period. The table provides two different measures for the balancedness of the sample: %Bias is the difference of the sample means between treated and nontreated firms as a percentage of the square root of the average of sample variances in both groups; t-stat is the test statistic of the two-sample t-test for differences in means. Panel B compares the mean values of firm characteristics of treated firms and the sample of matched control firms based on the Abadie and Imbens (2006) matching estimator. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The definitions of all matching covariates are presented in Table A1.

Panel A: Treated and Nontreated Firms (Unmatched Sample)

Matching Covariate	Treated	Nontreated	%Bias	t-Stat
Size	21.62	20.11	81.83	16.69***
Investment	5.79	7.27	−27.40	−5.48***
Cash Holdings	7.88	15.09	−50.07	−9.96***
Q	1.53	1.70	−17.41	−3.37***
Cash Flow	8.04	7.94	0.18	0.04
Return on Assets	3.39	2.52	22.58	4.38***
Long-Term Leverage	26.64	19.27	34.31	7.01***
Number of Firms	736	1013		

Panel B: Treated and Control Firms (Matched Sample)

Matching Covariate	Treated	Control	%Bias	t-Stat
Size	21.62	21.64	−1.54	−0.28
Investment	5.79	5.49	7.78	1.43
Cash Holdings	7.88	7.72	1.67	0.31
Q	1.53	1.48	7.55	1.33
Cash Flow	8.04	9.18	−2.74	−0.49
Return on Assets	3.39	3.49	−3.96	−0.72
Long-Term Leverage	26.64	25.68	5.15	−0.72
Number of Firms	736	736		

Table III
Matching Results: Investment and Firm Growth

This table reports the difference-in-differences matching estimation results for the average treatment effect on the treated (ATT) on investment and firm growth based on the bias-corrected Abadie and Imbens (2006) matching estimator. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms have neither a term loan maturing nor credit line expiring during this period. To each treated firm, I match one control firm from the control group pool of nontreated firms to produce a balanced sample in terms of firm size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I match on the 1-digit SIC industry code. Each row contains the change between the period before the credit supply shock (2008-Q3) and the respective post-period. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.	Δ Investment		Δ Log PPE Assets	
	(1)	(2)	(3)	(4)
Post Period	ATT	SE	ATT	SE
2010-Q2	−1.15*	0.66	−2.91**	1.46
2010-Q3	−1.50**	0.72	−3.61**	1.63
2010-Q4	−1.63**	0.78	−4.13**	1.84
2011-Q1	−1.49*	0.80	−4.56**	2.97
2011-Q2	−1.63*	0.85	−6.32***	2.12
2011-Q3	−1.56*	0.84	−7.24***	2.27
2011-Q4	−1.67**	0.86	−7.83***	2.48
2012-Q1	−1.74**	0.88	−8.06***	2.58
2012-Q2	−1.46*	0.87	−7.91***	2.69
2012-Q3	−1.55*	0.89	−8.15***	2.79
2012-Q4	−1.12	0.88	−7.53**	3.04
2013-Q1	−1.12	0.92	−8.61***	3.14
2013-Q2	−1.00	0.90	−9.52***	3.25
2013-Q3	−0.30	0.88	−9.40***	3.31
2013-Q4	−0.52	0.90	−7.85**	3.48
2014-Q1	−0.23	0.93	−8.37**	3.55
2014-Q2	0.25	0.91	−9.04**	3.66
2014-Q3	−0.05	0.87	−9.63**	3.75
2014-Q4	−0.42	0.89	−8.72**	3.87
2015-Q1	−0.52	0.89	−9.58**	3.94
2015-Q2	−0.32	0.84	−9.78**	4.04
2015-Q3	−0.35	0.82	−8.03*	4.14
2015-Q4	−0.39	0.77	−8.74**	4.41
Observations	736		736	

Table IV
Matching Results: Employment and Wage Growth

This table reports the difference-in-differences matching estimation results for the average treatment effect on the treated (ATT) on employment and wage growth based on the bias-corrected Abadie and Imbens (2006) matching estimator. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms have neither a term loan maturing nor credit line expiring during this period. To each treated firm, I match one control firm from the control group pool of nontreated firms to produce a balanced sample in terms of firm size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I match on the 1-digit SIC industry code. Each row contains the change between the period before the credit supply shock (2008-Q3) and the respective post-period. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.	Δ Log Employment		Δ Log Wages	
	(1)	(2)	(3)	(4)
Post Period	ATT	SE	ATT	SE
2010-Q2	−0.21	0.89	−0.11	0.56
2010-Q3	−0.21	0.89	−0.19	0.57
2010-Q4	−0.70	1.35	−0.25	0.60
2011-Q1	−0.70	1.35	−0.68	0.61
2011-Q2	−0.70	1.35	−1.14*	0.63
2011-Q3	−0.70	1.35	−1.22*	0.65
2011-Q4	−2.24	1.86	−0.99	0.65
2012-Q1	−2.24	1.86	−0.97	0.64
2012-Q2	−2.24	1.86	−0.53	0.63
2012-Q3	−2.24	1.86	−0.66	0.66
2012-Q4	−2.14	2.22	−0.92	0.68
2013-Q1	−2.14	2.22	−0.86	0.70
2013-Q2	−2.14	2.22	−0.72	0.72
2013-Q3	−2.14	2.22	−0.65	0.73
2013-Q4	−0.23	2.68	−0.70	0.77
2014-Q1	−0.23	2.68	−0.71	0.79
2014-Q2	−0.23	2.68	−0.95	0.79
2014-Q3	−0.23	2.68	−1.08	0.80
2014-Q4	−1.09	3.06	−1.00	0.81
2015-Q1	−1.09	3.06	−1.28	0.81
2015-Q2	−1.09	3.06	−1.44*	0.83
2015-Q3	−1.09	3.06	−1.52*	0.86
2015-Q4	0.74	3.60	−1.55*	0.86
Observations	736		736	

Table V
Sample Split Matching Results: Investment and Firm Growth

This table reports the difference-in-differences matching estimation results for the average treatment effect on the treated (ATT) on investment and firm growth separately for treated firms with rigid wages and treated firms with flexible wages. Treated firms with rigid (flexible) wages are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have a below (above) median wage share of payroll adjustment. Nontreated firms in the control group pool have neither a term loan maturing nor credit line expiring from 2008-Q4 to 2010-Q1. To each treated firm, I match one control firm from the control group pool of nontreated firms to produce a balanced sample in terms of firm size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I match on the 1-digit SIC industry code. Each row contains the change between the period before the credit supply shock (2008-Q3) and the respective post-period. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.	Δ Investment				Δ Log PPE Assets			
	Δ Rigid		Δ Flexible		Δ Rigid		Δ Flexible	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Period	ATT	SE	ATT	SE	ATT	SE	ATT	SE
2010-Q2	−1.63*	(0.85)	0.00	(0.77)	−4.43**	(1.88)	−1.18	(1.84)
2010-Q3	−1.88**	(0.90)	−0.20	(0.82)	−5.44***	(2.06)	−1.15	(2.03)
2010-Q4	−1.94*	(1.04)	−0.49	(0.85)	−6.27**	(2.44)	−1.57	(2.29)
2011-Q1	−2.98*	(1.09)	−0.11	(0.87)	−7.11***	(2.62)	−1.36	(2.48)
2011-Q2	−2.49**	(1.14)	0.47	(0.86)	−8.94***	(2.79)	−1.96	(2.71)
2011-Q3	−2.33**	(1.17)	0.27	(0.85)	−10.00***	(3.02)	−2.59	(2.84)
2011-Q4	−2.02*	(1.13)	0.28	(0.89)	−11.21***	(3.28)	−2.20	(3.09)
2012-Q1	−2.08*	(1.15)	0.09	(0.96)	−11.38***	(3.44)	−2.12	(3.19)
2012-Q2	−0.95	(1.14)	−0.54	(0.97)	−11.73***	(3.54)	−2.00	(3.33)
2012-Q3	−1.10	(1.15)	−0.73	(1.05)	−12.20***	(3.67)	−2.11	(3.51)
2012-Q4	−0.48	(1.16)	−0.59	(1.04)	−10.88***	(4.10)	−1.48	(3.83)
2013-Q1	−0.62	(1.23)	−0.78	(1.08)	−11.47***	(4.23)	−2.56	(4.01)
2013-Q2	−0.55	(1.25)	−0.62	(1.00)	−12.08***	(4.38)	−3.83	(4.15)
2013-Q3	0.20	(1.23)	−0.62	(1.00)	−11.78***	(4.51)	−4.06	(4.25)
2013-Q4	0.26	(1.27)	−1.41	(1.03)	−8.35*	(4.77)	−3.12	(4.46)
2014-Q1	0.51	(1.34)	−1.33	(1.03)	−9.26*	(4.90)	−3.60	(4.53)
2014-Q2	0.69	(1.34)	−0.67	(0.94)	−9.65*	(5.11)	−3.96	(4.61)
2014-Q3	0.16	(1.31)	−0.69	(0.87)	−10.77**	(5.18)	−4.44	(4.74)
2014-Q4	−0.71	(1.30)	−0.67	(0.90)	−10.05*	(5.25)	−3.20	(4.89)
2015-Q1	−0.61	(1.28)	−0.50	(0.90)	−11.95**	(5.31)	−4.67	(4.92)
2015-Q2	−0.35	(1.18)	−0.27	(0.92)	−11.43**	(5.52)	−5.46	(4.99)
2015-Q3	−0.22	(1.09)	−0.18	(0.92)	−9.60*	(5.70)	−3.65	(5.13)
2015-Q4	−0.47	(0.97)	−0.08	(0.89)	−10.01	(6.19)	−3.18	(5.42)
Observations	334		334		334		334	

Table VI
Sample Split Matching Results: Employment and Wage Growth

This table reports the difference-in-differences matching estimation results for the average treatment effect on the treated (ATT) on employment and wage growth separately for treated firms with rigid wages and treated firms with flexible wages. Treated firms with rigid (flexible) wages are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have a below (above) median wage share of payroll adjustment. Nontreated firms in the control group pool have neither a term loan maturing nor credit line expiring from 2008-Q4 to 2010-Q1. To each treated firm, I match one control firm from the control group pool of nontreated firms to produce a balanced sample in terms of firm size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I match on the 1-digit SIC industry code. Each row contains the change between the period before the credit supply shock (2008-Q3) and the respective post-period. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.	Δ Log Employment				Δ Log Wages			
	Δ Rigid		Δ Flexible		Δ Rigid		Δ Flexible	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Period	ATT	SE	ATT	SE	ATT	SE	ATT	SE
2010-Q2	−2.26*	(1.17)	1.72	(1.08)	0.85*	(0.62)	−1.13	(0.76)
2010-Q3	−2.26*	(1.17)	1.72	(1.08)	0.87*	(0.64)	−1.35*	(0.77)
2010-Q4	−2.32	(1.74)	1.82	(1.63)	0.65	(0.68)	−1.17	(0.80)
2011-Q1	−2.32	(1.74)	1.82	(1.63)	0.10	(0.69)	−1.44*	(0.82)
2011-Q2	−2.32	(1.74)	1.82	(1.63)	−0.39	(0.73)	−1.88**	(0.86)
2011-Q3	−2.32	(1.74)	1.82	(1.63)	−0.67	(0.71)	−1.70*	(0.91)
2011-Q4	−3.63	(2.45)	1.21	(2.26)	−0.39	(0.69)	−1.70*	(0.90)
2012-Q1	−3.63	(2.45)	1.21	(2.26)	−0.37	(0.66)	−1.50*	(0.89)
2012-Q2	−3.63	(2.45)	1.21	(2.26)	0.08	(0.64)	−0.90	(0.87)
2012-Q3	−3.63	(2.45)	1.21	(2.26)	0.07	(0.68)	−1.06	(0.90)
2012-Q4	−2.92	(2.92)	0.94	(2.68)	−0.24	(0.75)	−1.08	(0.93)
2013-Q1	−2.92	(2.92)	0.94	(2.68)	−0.22	(0.81)	−1.12	(0.94)
2013-Q2	−2.92	(2.92)	0.94	(2.68)	−0.31	(0.84)	−0.84	(0.95)
2013-Q3	−2.92	(2.92)	0.94	(2.68)	−0.28	(0.87)	−0.80	(0.97)
2013-Q4	0.44	(3.49)	3.11	(3.42)	−0.28	(0.90)	−0.74	(1.01)
2014-Q1	0.44	(3.49)	3.11	(3.42)	−0.40	(0.94)	−0.73	(1.01)
2014-Q2	0.44	(3.49)	3.11	(3.42)	−0.75	(0.95)	−0.82	(1.01)
2014-Q3	0.44	(3.49)	3.11	(3.42)	−0.97	(0.94)	−0.80	(1.02)
2014-Q4	−0.44	(4.00)	2.39	(3.88)	−0.89	(0.94)	−0.67	(1.03)
2015-Q1	−0.44	(4.00)	2.39	(3.88)	−1.07	(0.95)	−0.85	(1.02)
2015-Q2	−0.44	(4.00)	2.39	(3.88)	−1.07	(0.97)	−1.08	(1.06)
2015-Q3	−0.44	(4.00)	2.39	(3.88)	−1.21	(0.99)	−1.12	(1.11)
2015-Q4	0.35	(4.96)	3.59	(4.29)	−1.28	(1.01)	−1.65	(1.11)
Observations	334		334		334		334	

Table VII
Matching Quality: Affected Firms with Rigid and Flexible Wages

This table provides pre-treatment summary statistics on treated firms with rigid wages, treated firms with flexible wages, and treated firms with flexible wages in the matched control group. Panel A compares the mean values of firm characteristics of 334 treated firms with rigid wages and 334 treated firms with flexible wages in the unmatched sample. Treated firms with rigid (flexible) wages are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have a below (above) median wage share of payroll adjustment. The table provides two different measures for the balancedness of the sample: %Bias is the difference of the sample means between treated and nontreated firms as a percentage of the square root of the average of sample variances in both groups; Δ Means tests for differences in means using a two-sample t-test. Panel B compares the mean values of firm characteristics of treated firms with rigid wages and the sample of matched control treated firms with flexible wages based on the Abadie and Imbens (2006) matching estimator. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The definitions of all matching covariates are presented in Table A1 in Appendix C.

Panel A: Affected Firms with Rigid and Flexible Wages (Unmatched Sample)

Matching Covariate	Rigid	Flexible	%Bias	Δ Means
Size	21.39	21.99	−37.95	−0.60***
Investment	6.27	5.20	25.49	1.08***
Cash Holdings	8.28	7.21	10.85	1.07
Q	1.52	1.55	−4.72	−0.04
Cash Flow	−2.17	10.31	−24.37	−12.49**
Return on Assets	3.21	3.58	−13.57	−0.38*
Long-Term Leverage	28.35	25.31	15.84	3.23**
Number of Firms	334	334		

Panel B: Affected Firms with Rigid and Flexible Wages (Matched Sample)

Matching Covariate	Rigid	Control	%Bias	Δ Means
Size	21.39	21.54	−9.46	−0.15
Investment	6.27	5.68	14.38	0.60*
Cash Holdings	8.28	8.32	−0.46	−0.05
Q	1.52	1.56	−5.06	−0.04
Cash Flow	−2.17	8.68	−15.26	−10.85*
Return on Assets	3.21	4.10	−31.05	−0.89***
Long-Term Leverage	28.35	26.34	18.72	2.01**
Number of Firms	334	334		

Table VIII
Matching Results: Affected Firms with Rigid and Flexible Wages

This table reports the difference-in-differences matching estimation results for the average treatment effect on the treated (ATT) on investment, firm growth, employment growth, and wage growth based on the Abadie and Imbens (2006) matching estimator. Treated firms with rigid (flexible) wages are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and that have a below (above) median wage share of payroll adjustment. To each treated firm with rigid wages, I match one firm from the control group pool of treated firms with flexible wages to produce a balanced sample in terms of firm size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I match on the 1-digit SIC industry code. Each row contains the change between the period before the credit supply shock (2008-Q3) and the respective post-period. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.	Δ Investment		Δ Log PPE Assets		Δ Log Employment		Δ Log Wages	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Period	ATT	SE	ATT	SE	ATT	SE	ATT	SE
2010-Q2	−1.06	(1.01)	−3.78*	(1.94)	−3.04***	(1.14)	2.22***	(0.75)
2010-Q3	−1.00	(1.04)	−5.00**	(2.15)	−3.04***	(1.14)	2.16***	(0.79)
2010-Q4	−0.25	(1.01)	−5.31**	(2.41)	−4.81***	(1.71)	2.19**	(0.85)
2011-Q1	−0.45	(1.03)	−5.65**	(2.57)	−4.81***	(1.71)	1.45*	(0.86)
2011-Q2	−0.82	(1.00)	−6.95**	(2.70)	−4.81***	(1.71)	1.25	(0.86)
2011-Q3	−0.42	(1.00)	−6.53**	(2.79)	−4.81***	(1.71)	1.05	(0.89)
2011-Q4	−1.29	(0.99)	−8.12***	(3.07)	−6.60***	(2.27)	0.95	(0.91)
2012-Q1	−2.09*	(1.07)	−8.92***	(3.19)	−6.60***	(2.27)	1.04	(0.90)
2012-Q2	−2.34**	(1.11)	−10.02***	(3.37)	−6.60***	(2.27)	1.13	(0.88)
2012-Q3	−2.74**	(1.15)	−10.61***	(3.52)	−6.60***	(2.27)	0.88	(0.92)
2012-Q4	−2.28**	(1.10)	−12.53***	(3.91)	−5.23*	(2.88)	0.83	(0.95)
2013-Q1	−2.33**	(1.07)	−12.59***	(4.13)	−5.23*	(2.88)	1.20	(1.00)
2013-Q2	−1.09	(1.10)	−12.86***	(4.25)	−5.23*	(2.88)	1.07	(0.99)
2013-Q3	−0.69	(1.14)	−11.79***	(4.48)	−5.23*	(2.88)	1.26	(1.01)
2013-Q4	0.40	(1.20)	−11.53**	(4.52)	−3.01	(3.38)	1.21	(1.04)
2014-Q1	1.19	(1.32)	−11.82**	(4.61)	−3.01	(3.38)	1.21	(1.04)
2014-Q2	1.21	(1.34)	−12.01**	(4.71)	−3.01	(3.38)	1.15	(1.04)
2014-Q3	0.71	(1.24)	−11.73**	(4.76)	−3.01	(3.38)	1.01	(1.06)
2014-Q4	0.25	(1.23)	−11.83**	(4.96)	−2.81	(3.84)	0.95	(1.03)
2015-Q1	0.03	(1.23)	−12.11**	(5.10)	−2.81	(3.84)	0.76	(1.04)
2015-Q2	−0.17	(1.20)	−11.35**	(5.29)	−2.81	(3.84)	1.09	(1.08)
2015-Q3	−0.31	(1.17)	−10.53*	(5.55)	−2.81	(3.84)	0.97	(1.15)
2015-Q4	0.24	(1.18)	−10.54*	(5.78)	−3.28	(4.44)	1.28	(1.16)
Observations	334		334		334		334	

Table IX
Robustness Check: Alternative Definition of the Treatment Period

This table reports the difference-in-differences matching estimation results for the average treatment effect on the treated (ATT) on investment, firm, employment, and wage growth using an alternative definition for the treatment period. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2009-Q1 to 2009-Q4. Nontreated firms in the matched control have neither a term loan maturing nor credit line expiring during this period. To each treated firm, I match one control firm from the control group pool of nontreated firms to produce a balanced sample in terms of firm size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I match on the 1-digit SIC industry code. Each row contains the change between the period before the credit supply shock (2008-Q3) and the respective post-period. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.	Δ Investment		Δ Log PPE Assets		Δ Log Employment		Δ Log Wages	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Period	ATT	SE	ATT	SE	ATT	SE	ATT	SE
2010-Q2	−0.92	(0.65)	−3.42**	(1.49)	−0.27	(0.91)	−0.41	(0.56)
2010-Q3	−1.45**	(0.72)	−4.08**	(1.66)	−0.27	(0.91)	−0.44	(0.57)
2010-Q4	−1.70**	(0.76)	−5.08***	(1.88)	−0.73	(1.39)	−0.55	(0.59)
2011-Q1	−1.73**	(0.76)	−5.13**	(2.02)	−0.73	(1.39)	−0.93	(0.60)
2011-Q2	−2.01**	(0.82)	−6.82***	(2.17)	−0.73	(1.39)	−1.31**	(0.62)
2011-Q3	−2.02**	(0.81)	−7.48***	(2.32)	−0.73	(1.39)	−1.35**	(0.64)
2011-Q4	−1.90**	(0.83)	−8.91***	(2.54)	−2.73	(1.91)	−0.98	(0.65)
2012-Q1	−1.90**	(0.85)	−8.86***	(2.63)	−2.73	(1.91)	−1.10*	(0.64)
2012-Q2	−1.41*	(0.83)	−8.87***	(2.72)	−2.73	(1.91)	−0.75	(0.64)
2012-Q3	−1.23	(0.84)	−9.03***	(2.83)	−2.73	(1.91)	−0.79	(0.68)
2012-Q4	−0.61	(0.85)	−8.99***	(3.02)	−3.00	(2.31)	−1.00	(0.70)
2013-Q1	−0.66	(0.87)	−9.77***	(3.12)	−3.00	(2.31)	−0.74	(0.73)
2013-Q2	−0.58	(0.84)	−10.99***	(3.20)	−3.00	(2.31)	−0.49	(0.74)
2013-Q3	−0.54	(0.84)	−11.05***	(3.28)	−3.00	(2.31)	−0.33	(0.76)
2013-Q4	−0.72	(0.88)	−9.06***	(3.47)	−1.04	(2.75)	−0.22	(0.80)
2014-Q1	−0.65	(0.94)	−9.67***	(3.52)	−1.04	(2.75)	−0.28	(0.81)
2014-Q2	−0.42	(0.94)	−9.97***	(3.61)	−1.04	(2.75)	−0.59	(0.82)
2014-Q3	−0.48	(0.90)	−11.27***	(3.70)	−1.04	(2.75)	−0.73	(0.82)
2014-Q4	−0.91	(0.90)	−10.51***	(3.82)	−1.31	(3.15)	−0.84	(0.82)
2015-Q1	−0.99	(0.89)	−12.29***	(3.85)	−1.31	(3.15)	−0.91	(0.82)
2015-Q2	−0.44	(0.81)	−12.27***	(3.97)	−1.31	(3.15)	−1.07	(0.85)
2015-Q3	−0.17	(0.79)	−11.48***	(4.04)	−1.31	(3.15)	−1.25	(0.88)
2015-Q4	−0.14	(0.74)	−11.54***	(4.26)	−0.49	(3.65)	−1.38	(0.87)
Observations	581		581		581		581	

Table X
Robustness Check: Alternative Definition of the Treatment Group

This table reports the difference-in-differences matching estimation results for the average treatment effect on the treated (ATT) on investment, firm, employment, and wage growth using an alternative definition for the treatment group. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1 and for which the volume of the maturing term loans or expiring credit lines is more than 10 percent of total assets. Nontreated firms in the matched control group have neither a term loan maturing nor credit line expiring during this period. To each treated firm, I match one control firm from the control group pool of nontreated firms to produce a balanced sample in terms of firm size, investment ratio, cash holdings, Q, cash flow, return on assets, and long-term leverage. Additionally, I match on the 1-digit SIC industry code. Each row contains the change between the period before the credit supply shock (2008-Q3) and the respective post-period. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.	Δ Investment		Δ Log PPE Assets		Δ Log Employment		Δ Log Wages	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Period	ATT	SE	ATT	SE	ATT	SE	ATT	SE
2010-Q2	−1.71**	(0.82)	−3.62**	(1.66)	−1.20	(1.07)	0.68	(0.68)
2010-Q3	−2.03**	(0.89)	−4.46**	(1.87)	−1.20	(1.07)	0.55	(0.70)
2010-Q4	−2.11**	(0.96)	−5.44**	(2.12)	−2.26	(1.67)	0.28	(0.74)
2011-Q1	−1.62*	(0.93)	−5.09**	(2.25)	−2.26	(1.67)	−0.16	(0.73)
2011-Q2	−2.15**	(0.96)	−6.41***	(2.44)	−2.26	(1.67)	−0.59	(0.76)
2011-Q3	−2.13**	(0.95)	−7.87***	(2.64)	−2.26	(1.67)	−0.67	(0.78)
2011-Q4	−2.09**	(0.98)	−8.90***	(2.86)	−3.59	(2.23)	−0.28	(0.75)
2012-Q1	−2.35**	(1.04)	−9.02***	(2.97)	−3.59	(2.23)	−0.46	(0.72)
2012-Q2	−1.79*	(1.03)	−9.43***	(3.08)	−3.59	(2.23)	0.08	(0.69)
2012-Q3	−1.80*	(1.09)	−10.26***	(3.23)	−3.59	(2.23)	0.02	(0.72)
2012-Q4	−1.12	(1.12)	−10.30***	(3.45)	−4.86*	(2.65)	−0.33	(0.78)
2013-Q1	−1.33	(1.19)	−11.80***	(3.57)	−4.86*	(2.65)	−0.27	(0.82)
2013-Q2	−0.84	(1.13)	−12.39***	(3.69)	−4.86*	(2.65)	−0.26	(0.84)
2013-Q3	−0.10	(1.11)	−12.12***	(3.81)	−4.86*	(2.65)	−0.30	(0.86)
2013-Q4	−0.22	(1.15)	−8.83**	(4.08)	−4.42	(3.10)	−0.32	(0.88)
2014-Q1	0.33	(1.21)	−9.47**	(4.20)	−4.42	(3.10)	−0.33	(0.89)
2014-Q2	0.97	(1.17)	−10.71**	(4.36)	−4.42	(3.10)	−0.59	(0.90)
2014-Q3	0.85	(1.12)	−11.80***	(4.47)	−4.42	(3.10)	−0.59	(0.90)
2014-Q4	0.10	(1.14)	−10.71**	(4.59)	−5.58	(3.47)	−0.49	(0.91)
2015-Q1	−0.12	(1.13)	−12.15***	(4.62)	−5.58	(3.47)	−0.84	(0.92)
2015-Q2	0.36	(1.05)	−12.23**	(4.76)	−5.58	(3.47)	−1.01	(0.95)
2015-Q3	0.18	(1.05)	−11.24**	(4.94)	−5.58	(3.47)	−1.42	(0.99)
2015-Q4	−0.12	(0.98)	−11.95**	(5.21)	−5.52	(4.11)	−2.01**	(1.01)
Observations	430		430		430		430	

Appendix A. Identification Strategy

I define the treatment group to be firms i that had at least one loan facility j maturing during the period from 2008-Q4 to 2010-Q1. Conversely, I define the control group pool to be firms that had neither a term loan maturing nor a credit line expiring during this period:

$$\text{Treatment}_i = \begin{cases} 1 \quad \text{if} \quad \exists \text{Facility}_{i,j} : \text{Maturity Date}(\text{Facility}_{i,j}) \in [\text{2008-Q4}, \text{2010-Q1}] \\ 0 \quad \text{if} \quad \nexists \text{Facility}_{i,j} : \text{Maturity Date}(\text{Facility}_{i,j}) \in [\text{2008-Q4}, \text{2010-Q1}] \end{cases} \tag{A1}$$

Figure A1 illustrates the basic idea behind my identification strategy.

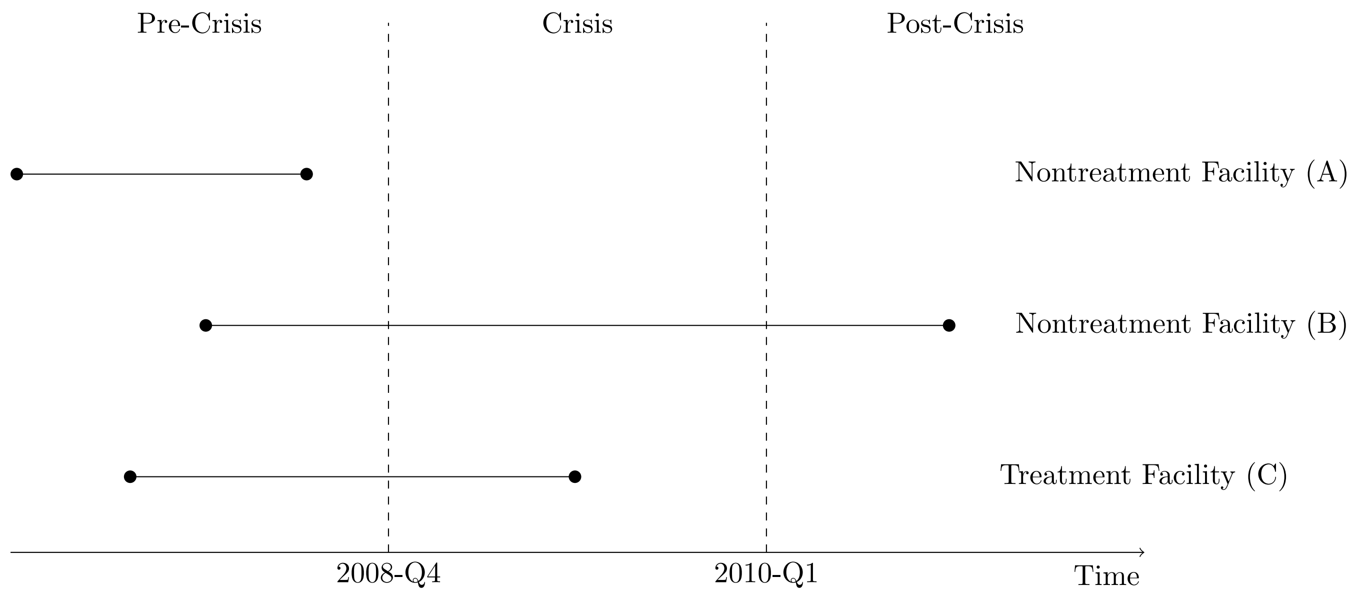


Figure A1. Basic Identification Idea. This figure illustrates the basic idea behind my identification strategy. I split the loan facilities in my sample into *Nontreatment Facilities* maturing either before (Facilty A) or after (Facility B) the credit crunch period, and *Treatment Facilities* maturing during the credit crunch period between 2008-Q4 and 2010-Q1 (Facilty C). I define my treatment group to be firms with at least one treatment facility maturing during the credit crunch period.

Appendix B. Additional Figures

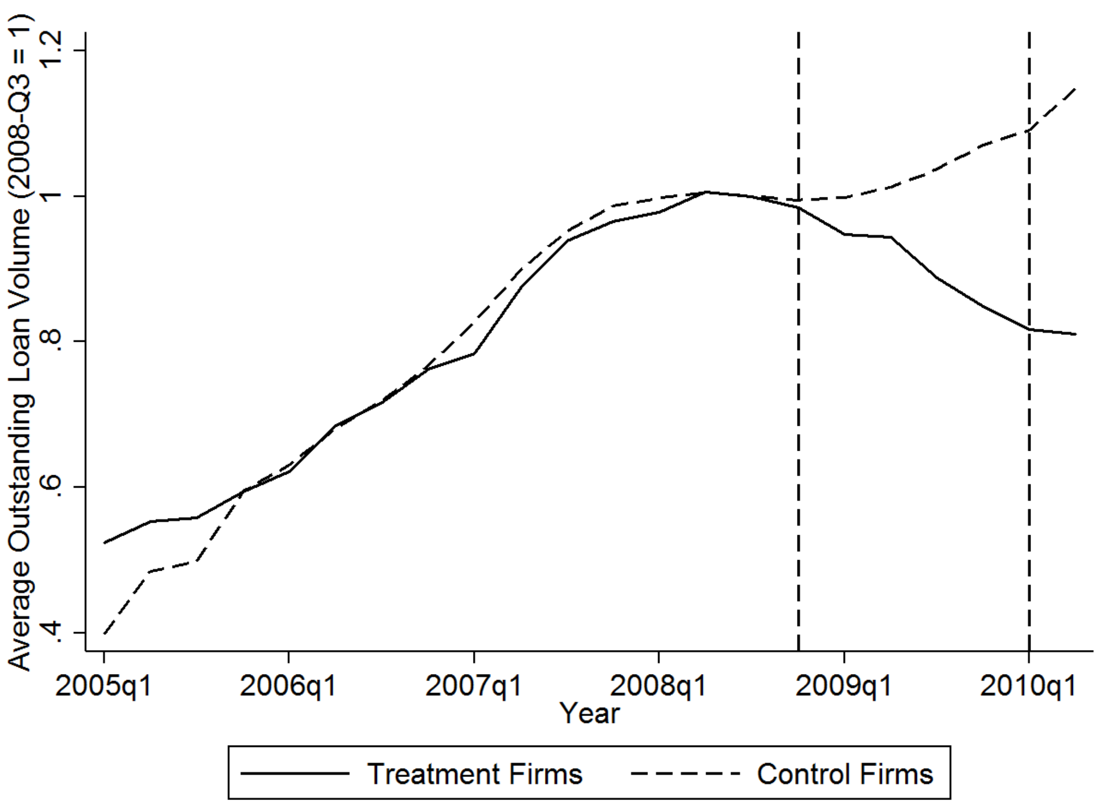


Figure A2. Mean Outstanding Syndicated Loan Volumes by Quarter. This figure shows the evolution of mean outstanding syndicated loan volumes for both treated firms (solid line) and nontreated firms (dashed line), relative to 2008-Q3. Treated firms are firms that have at least one term loan maturing or credit line expiring during the period from 2008-Q4 to 2010-Q1, while nontreated firms have neither a term loan maturing nor credit line expiring during this period. The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

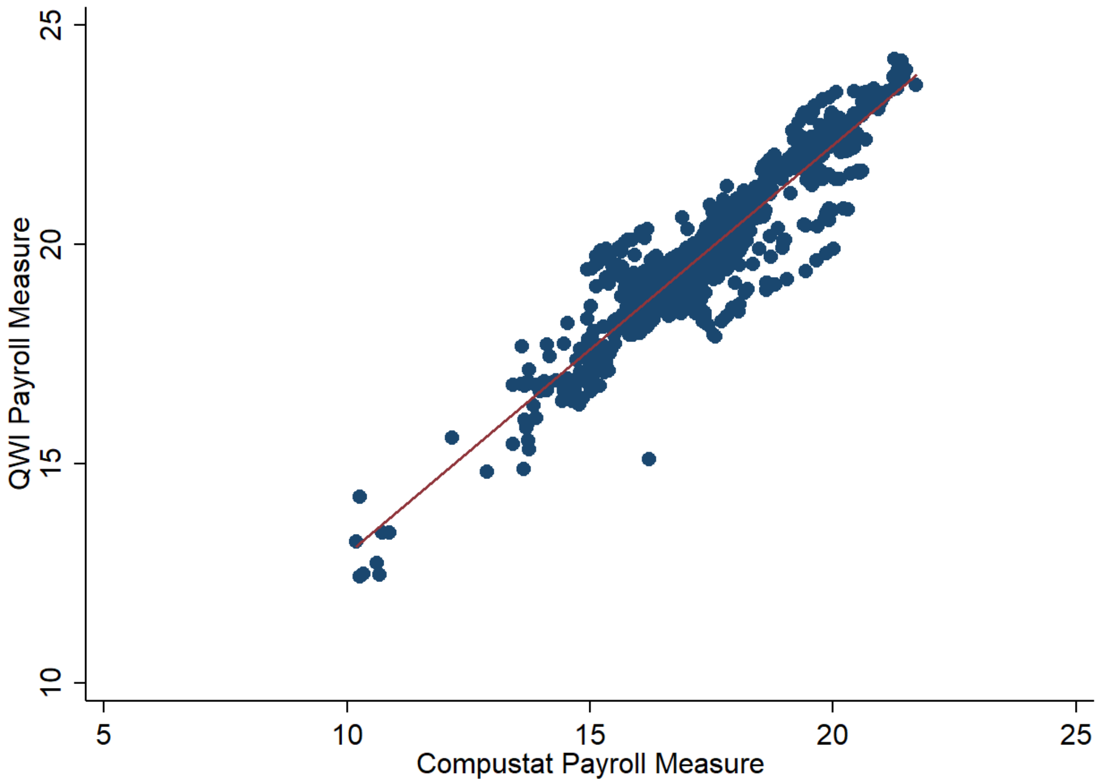


Figure A3. Combined QWI-Compustat Payroll Measure versus Compustat Staff Expenses. This figure plots the logarithm of the combined QWI-Compustat payroll measure as defined in Section III against the logarithm of the Compustat staff expense item *XLR* for firms for which both data sources are available. The slope coefficient of the associated regression is 0.93 and the adjusted R^2 is 0.88, indicating that the combined QWI-Compustat payroll measure provides a reasonable approximation for firms' actual payroll expenses.

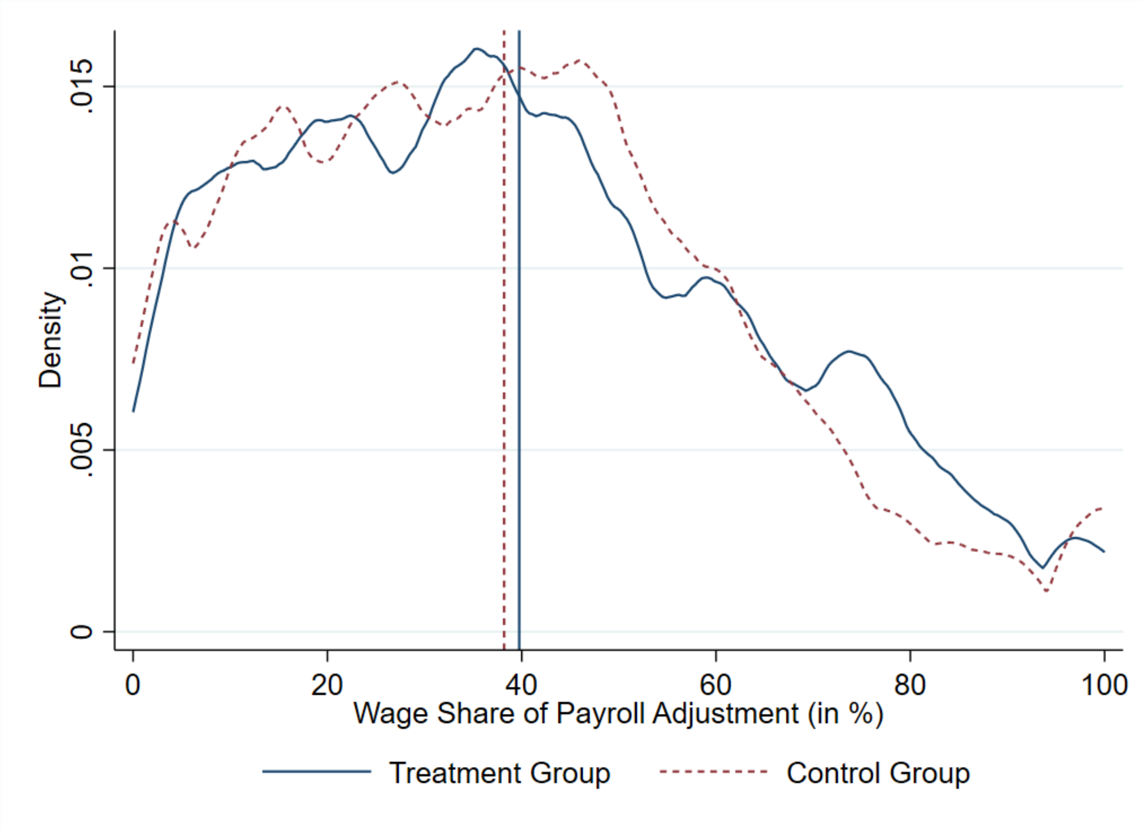
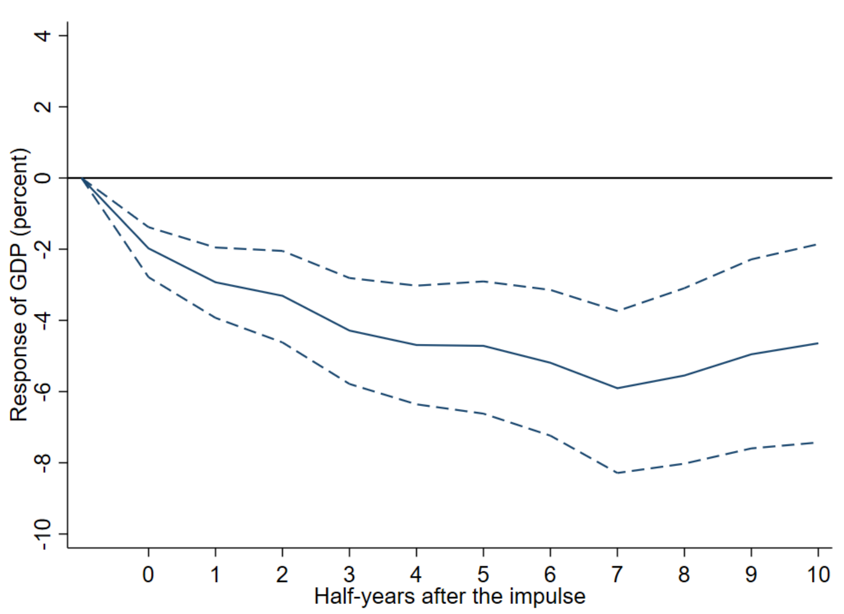


Figure A4. Wage Share of Payroll Adjustment. This figure illustrates the distribution of the wage share of payroll adjustment for 668 firms in the treatment group (solid blue line) and 668 firms in the treatment group 796 firms in the control group (dashed red line). The vertical lines mark the mean values of the wage share of payroll adjustment for treatment firms and control firms, respectively.

(A) Romer and Romer (2017)



(B) Wix (2023)

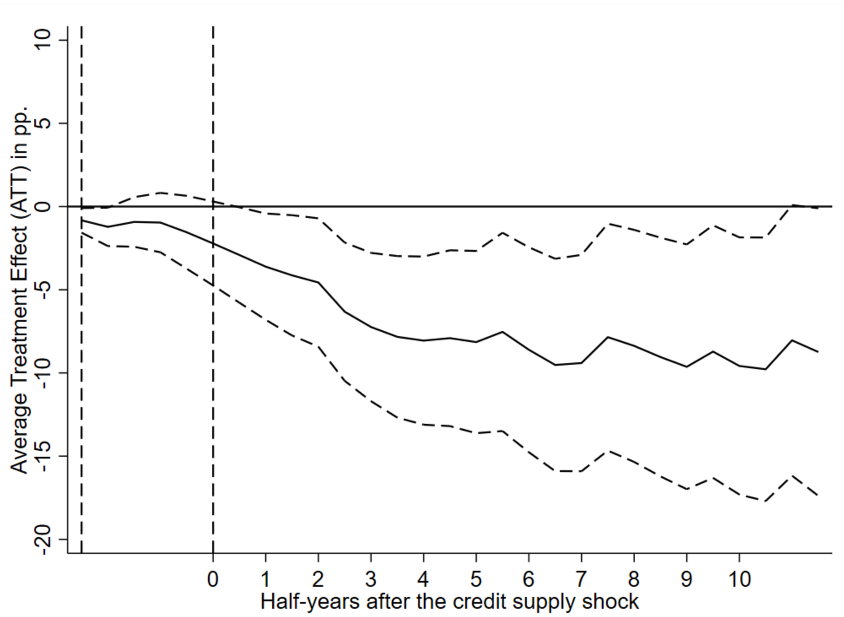
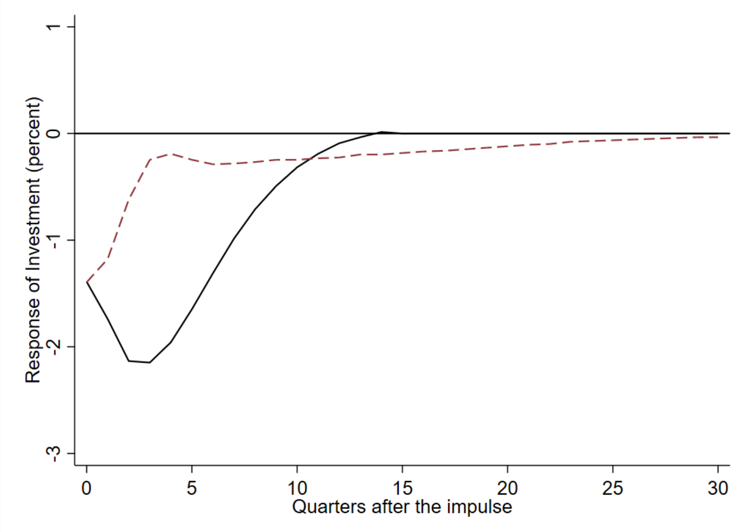


Figure A5. The Long-Run Real Effects of Financial Crises: Romer and Romer (2017) versus Wix (2023). Panel A illustrates the impulse response function for real GDP to an impulse of financial distress from Panel A of Figure 5 in Romer and Romer (2017). The dashed lines show the two-standard-error confidence bands. Panel B replicates the treatment effect curve on firm growth from Panel B in Figure 4. For ease of comparison, the x-axis in Panel B is now scaled in half-years after the end of the credit crunch period in 2010q1 (=0). The solid line represents the matching estimates for the ATT based on the results in Table III. The two dashed lines represent the 95% confidence intervals. The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

(A) Ajello (2016)



(B) Wix (2023)

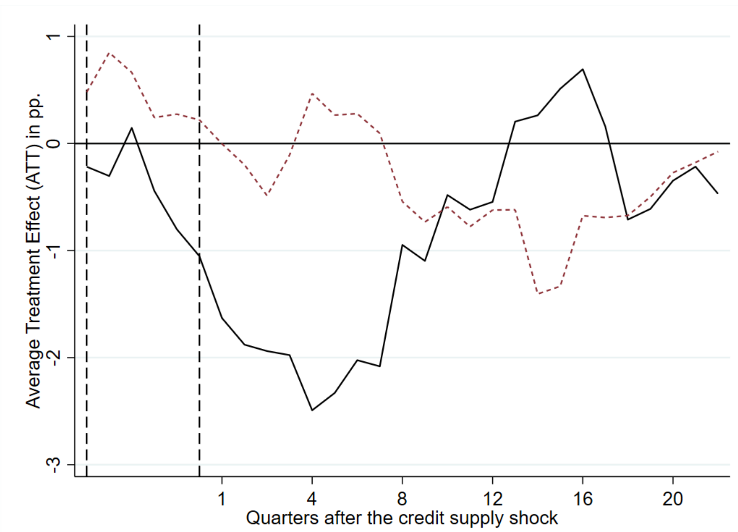


Figure A6. The Role of Wage Rigidity in the Amplification of Financial Shocks: Ajello (2016) versus Wix (2023). Panel A illustrates the impulse response functions of investment to a negative financial shock from Figure 6 in Ajello (2016). The solid black line illustrates the model results with rigid wages, and the dashed-dotted red line illustrates the model results with flexible wages. Panel B replicates the treatment effect curve on investment from Panel A in Figure 9. For ease of comparison, the black line now illustrates the treatment effect curve for financially shocked firms with rigid wages, and the dotted red line illustrates the treatment effect curve for financially shocked firms with flexible wages. The x-axis in Panel B is now scaled in quarters after the end of the credit crunch period in 2010q1 (=0). The two vertical lines mark 2008-Q4 and 2010-Q1, the beginning and end of the credit crunch period in my sample.

Appendix C. Additional Tables

Table A1
Variable Definitions

This table shows the definitions of all dependent variables, matching variables, and wage rigidity variables used in the paper. The definitions provide the items of Compustat's North America Fundamentals Quarterly database or the items of the Quarterly Workforce Indicators (QWI) database used to construct the variables.

Variable	Definition	Data Source
Dependent Variables		
ΔInvestment	Investment$_{t+k}$ - Investment$_t$	Compustat
Δ Log PPE Assets	Log($ppentq_{t+k}$) - Log($ppentq_t$)	Compustat
Δ Log Employment	Log(emp_{t+k}) - Log(emp_t)	Compustat
Δ Log Wages	Log(Wage$_{t+k}$) - Log(Wage$_t$)	QWI
Log(Wage)	$\frac{(Log(earns_t)+Log(earns_{t-1})+Log(earns_{t-2})+Log(earns_{t-3}))}{4}$	QWI
Matching Variables		
Size	Log(atq_t)	Compustat
Investment	$\frac{\left(capxyq_t+capxyq_{t-1}+capxyq_{t-2}+capxyq_{t-3}\right)}{ppentq_{t-4}}$	Compustat
Cash Holdings	$\frac{cheq_t}{atq_t}$	Compustat
Q	$\frac{atq_t+prccq_t \times cshoq_t - ceqq_t}{atq_t}$	Compustat
Cash Flow	$\frac{ibq_t+dpq_t}{ppentq_{t-1}}$	Compustat
Return on Assets	$\frac{oibdpq_t}{atq_{t-1}}$	Compustat
Long-Term Leverage	$\frac{dlttq_t}{atq_t}$	Compustat
SIC Industry Code	*sic*	Compustat
Wage Rigidity Variables		
Payroll	$emp_t \times earns_t$	Compustat/QWI
Wage Rigidity θ_t	$\frac{1}{T}\sum_{t=1}^{T}\frac{\Delta Log(earns_t)}{\Delta Log(\text{Payroll}_t)}$	Compustat/QWI