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The Persistent Effect of a Credit Crunch on Output and Productivity: Technical or Allocative Efficiency?*

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Central Bank of
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Abstract

This paper estimates the impact of a credit crunch on output and productivity growth by disentangling two complementary channels: technical and allocative efficiency. Starting from the census of Chilean firms and exploiting a large-scale natural experiment during the 2008-09 crisis, I show that a sharp contraction in credit supply had persistent real effects, as the output of more affected firms did not catch up with that of less affected firms. A negative but moderate effect on firm-level productivity helps to explain this persistence. Also, the dispersion of the marginal productivity of capital increases within more affected firms. In the aggregate, productivity growth explains around a third of the impact of the credit crunch on output growth in the medium run. In turn, allocative efficiency explains twice as much as technical efficiency of productivity growth.

Resumen

Este trabajo estima el impacto de una contracción del crédito en el crecimiento del producto y la productividad, considerando dos canales complementarios: cambios en la tecnología y en la reasignación de recursos. Un experimento natural a gran escala durante la crisis de 2008-09 muestra que una contracción abrupta del crédito bancario puede tener efectos reales persistentes, ya que el producto de aquéllas firmas más afectadas por la falta de crédito nunca alcanza al de aquéllas menos afectadas. Un efecto negativo pero moderado en la productividad de estas firmas ayuda a explicar este efecto persistente. Además, la dispersión de las productividades marginales del capital aumenta para estas mismas firmas. A nivel agregado, en el mediano plazo, el crecimiento de la productividad explica alrededor de un tercio del impacto de la contracción del crédito en el crecimiento del producto. A su vez, los cambios en la reasignación de recursos explican el doble que los cambios en la tecnología del crecimiento en productividad.

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1 Introduction

Whether credit supply contractions affect total factor productivity (TFP), and if they do, why, remains an open question. Several studies on the aftermath of financial crises use country-level data to show that in many episodes output does not recover its pre-crisis trend after a credit crunch, which suggests that potential output—and therefore productivity—may be damaged by large and sharp contractions in the supply of credit. However, most existing studies are unable to establish a causal relationship.

This paper isolates and quantifies the effect of a sharp contraction in credit supply in the context of a recession on output and productivity growth, distinguishing between two complementary channels. On the one hand, it assesses its impact on technical efficiency, that is, on the ability to generate more output holding inputs constant. On the other hand, it assesses the impact of the credit crunch on allocative efficiency, that is, on the ability to move resources towards their more productive use.

Firstly, starting from the census of Chilean firms and using a large-scale natural experiment in a difference-in-differences setting, I show that a credit crunch during the 2008-09 global financial crisis had a persistent negative effect on factor accumulation and output growth at the firm-level. The protracted recovery pattern of firms more affected by the credit supply shock is similar to that of countries hit by a financial crisis, such as the US after the Great Recession. I then assess the impact of the credit crunch on firm-level technical efficiency in the medium term, and adopt the framework in Hsieh and Klenow (2009) to explore its effects on aggregated output and TFP through both allocative and technical efficiency channels.

In order to obtain firm-level estimates of the causal effect of a negative credit supply shock during a recession, I take advantage of a major policy intervention by the Chilean government in 2009, which aimed to support firms' access to credit during the downturn: the capitalization of the third largest and only public bank in Chile. I show that fresh funds allowed the capitalized bank to increase commercial lending while all other banks were contracting their credit supply sharply. Following the literature on the real effect of banking shocks, I construct two groups of firms with similar ex-ante distributions of observable characteristics: those that had a lending relationship with the bank before the announcement of the intervention—treated firms henceforth—and those that did not—control firms henceforth.[1] Due to the size of the capitalized bank and its well-diversified portfolio, the sample includes firms of different sizes, ages and industries. I

[1]Several studies take advantage of the fact that bank lending relationships are sticky to provide evidence of the real effects of banking shocks at the firm-level. See, for example, Huber (2018), Chodorow-Reich (2014), Khwaja and Mian (2008), Bentolila et al. (2017), Cingano et al. (2016), Gan (2007), Amiti and Weinstein (2011), Paravisini et al. (2015), Iyer et al. (2013) or Schnabl (2012).

show that while all firms experienced the negative aggregate demand shock in 2009, only those connected to the bank were able to circumvent the sharp contraction of credit during the recession.

I address the main identification challenges stemming from non-random firm-bank matching in several ways. Firstly, I use an omitted variables test proposed by Jiménez et al. (2011), to show that the broad set of observable firm characteristics in the data are able to capture unobserved credit demand shocks. Then, in order to reinforce the identification strategy and obtain a control group of firms similar to those in the treatment group, I use a simple matching algorithm. Both groups of firms have similar distributions of variables such as size, age, output, capital and employment before the shock, which allows me to compare firm-level averages, group aggregates and measures of dispersion within groups over time. Also, I provide additional evidence and robustness checks supporting the parallel trends identification assumption needed to obtain causal firm-level estimates. In particular, using financial and employment data at the monthly frequency, I show that treated and control firms react similarly to the aggregate demand shock that preceded the announcement of the policy intervention.

My results show that in the short run, lending, output, capital and employment fell more for firms that were not related to the capitalized bank, which confirms the results from studies analyzing similar settings in different countries.[2] During 2009, total bank lending fell 15% more for firms in the control group, on average. The fall in credit exacerbated the effect of the adverse aggregate demand shock among these firms: output and employment growth fell around 3% and 2.6% more, while capital accumulation fell by 4% more, on average.

In the medium run, once the adverse demand shock had passed and the policy intervention was over, firms in the control group had lower levels of bank debt and recovered more slowly, leading to a persistent effect on output growth, which on average was 4.6% lower between 2007 and 2012, four years after the shock. Consistently, the effect on factor accumulation was also persistent, as employment and capital grew about 5% and 9% less, respectively, relative to treated firms during the same period. Again, these firm-level estimates are similar in magnitude to those obtained separately for employment, output and capital by other studies, which supports the external validity, beyond the Chilean economy, of the natural experiment analyzed here.

An economy's measure of technical efficiency corresponds to the aggregation of firms' physical productivities; therefore, my next step is to assess how these were affected by the credit crunch. In particular, I first show to what extent damaged firm-level

[2]See, for example, Chodorow-Reich (2014) for an estimation of the effects of the credit supply contraction on employment during the Great Recession in the US; and Cingano et al. (2016) for an estimation of the effects of the global financial crisis on investment and output in Italy.

productivity growth explains persistent lower output growth. To do so, I depart from purely reduced-form estimates and assume a constant-returns-to-scale Cobb-Douglas production function, which I estimate for the sample in the period before the recession. In addition, because I do not observe firms' prices, I take advantage of the natural experiment and follow De Loecker and Warzynski (2012) to obtain a value for the average markup charged by firms in the sample, which is critical for the distinction between revenue productivity and physical productivity in the presence of distortions affecting the allocation of resources. An upper bound for the effect of the credit crunch on firm-level productivity growth four years after the shock is 1.5%—roughly a fifth of the effect on physical output growth—which suggests the absence of a strong effect on TFP through technical efficiency.

I then analyze the effect of the credit crunch on TFP through allocative efficiency. To this end, I adopt the framework in Hsieh and Klenow (2009), which allows me to obtain a measure of aggregate productivity and its technical and allocative efficiency components for each group of firms separately. I show that before the crisis aggregates of output and TFP follow similar trends but they diverge afterwards, leading to larger productivity growth for treated firms in the medium term. Consistently, the dispersion of marginal productivities of capital within firms affected by the credit crunch rises during the downturn and afterwards. Furthermore, I show that four years after the shock around a third of the differences in output growth between groups are explained by differential productivity growth, and from these, 65% are accounted by differences in allocative efficiency due to the credit crunch.

This paper is related to a growing body of empirical studies that analyze the aftermath of banking crises using cross-country data; see Cerra and Saxena (2008), Reinhart and Rogoff (2009), Krishnamurthy and Muir (2017), Schularick and Taylor (2012). Although these studies are unable to establish a causal relationship, as emphasized by Romer and Romer (2017), they show that financial crises lead to deeper and more protracted recessions. In particular, Cerra and Saxena (2008) and Reinhart and Rogoff (2009) argue that output does not recover its pre-crisis trend, which suggests that credit crunches damage TFP.[3] This view is supported by studies that use granular data to establish a causal link between credit and firm-level productivity through the technology channel. In line with Huber (2018) and Manaresi and Pierri (2018), Levine and Warusawitharana (2014), and de Ridder (2018) argue that less innovation due to the lack of credit is the main reason explaining protracted recoveries after financial crises. For the Great Recession in the U.S., Redmond and Van Zandweghe (2016) argue that

[3] That is, the level of aggregate productivity and therefore potential output, would be persistently lower than the implied by the pre-crisis trend.

innovation rather than resource allocation was the main driver of lower TFP growth afterwards.

Other papers use firm or plant-level data to study the dynamics of misallocation during crises episodes. Even though these studies do not identify the role of financial frictions, they provide measures of the overall impact of misallocation on TFP. Oberfield (2013) uses the Chilean survey of manufacturing establishments to study the evolution of TFP during the Chilean banking crisis of 1982 by adopting the framework of Hsieh and Klenow (2009) as well. He shows that changes in allocative efficiency, driven by between-industry differences, explain about one-third of the drop in TFP. In a related paper, Sandleris and Wright (2014) estimate that a decline in allocative efficiency accounts for the 10% of the fall in TFP during the Argentinian crisis of 2001.

Few studies analyze the effect of large unexpected adverse credit supply shocks on misallocation, usually relying on structural models. Midrigan and Xu (2014) use Korean data to calibrate a two-sector model with borrowing constraints and simulate the effects of a credit crunch of the magnitude of the 1997 financial crisis. They find that the credit crunch can generate sizable TFP losses due to increased capital misallocation and firm exit. In a related paper, Buera and Moll (2015) use a model with financial frictions to show that credit crunches may appear as an efficiency wedge in settings with heterogeneous firm productivity.

Empirical studies on the relationship between finance and TFP in the medium term provide mixed evidence. On the one hand, Larrain and Stumpner (2017) estimate significant positive effects on industry-level productivity after financial liberalization in Eastern European countries. Also, Caggese (2019) shows that financial frictions affect firms' productivity growth by reducing investment in innovation due to lower entry and competition. On the other hand, Gilchrist et al. (2013) find evidence consistent with Midrigan and Xu (2014) and Moll (2014), who argue that financial frictions do not account for much of the observed misallocation in the long run, as entrepreneurs can save-out of the financial constraint. Finally, Gopinath et al. (2017) also use the framework in Hsieh and Klenow (2009) to illustrate how capital inflows increased misallocation in Southern European countries. They find that lower interest rates decreased productivity, as capital was allocated towards firms with high collateral but less productive.

This paper contributes to this literature by showing empirically how the lack of credit during recessions can have persistent effects on output. In particular, it shows that the contraction in credit supply affects TFP by reducing firm-level productivity growth, and more importantly, by impairing allocative efficiency in the economy. These results help in the understanding of protracted recoveries in the aftermath of a financial crisis.

The rest of the paper is structured as follows. Section 2, I provide a description of the institutional framework and the natural experiment. In Section 3, I present the empirical strategy and address the identification challenges for firm-level results. Section 4 shows and discusses these results. Section 5 assesses the impact of the credit crunch on productivity. Finally, Section 6 concludes.

2 Institutional framework and natural experiment

2.1 The 2008-09 crisis in Chile

Chile has an open economy, and as such, following the decline in world trade starting in 2008, it was hit by the global crisis. Exports, which at that moment represented around 40% of Chilean GDP, fell by more than 30% in the last quarter of 2008. During 2009, GDP fell by 1.5% and unemployment rose from 7% to almost 11%, as shown in Figure 1.[4]

Alegría et al. (2018) show how Chilean banks experienced significant difficulties in accesing foreing sources of funding during the crisis. Thus, lending conditions deteriorated quickly after the fall of Lehman Brothers and bank commercial loans ground to a halt after October 2008.[5] By June 2009, the flow of new commercial loans had fallen by more than 30% on an annual basis, as shown in Figure 2. According to the Central Bank of Chile, the fall in bank lending was largely explained by a supply effect as banks hoarded liquidity and significantly tightened lending conditions, fearing a sudden stop in external financing.[6]

Even though the initial impact of the global crisis in Chile was significant, it was short-lived, and the economy started its recovery during the second quarter of 2010. The financial system weathered the crisis well, and bank lending recovered its pre-crisis levels by the end of 2011. To a great extent, the transitory nature of the shock was due to the policies implemented by the government and the central bank, which reacted fast after Lehman Brothers. The collapse of the banking system in 1982 and the financial crisis in 1998 had shown the disastrous consequences that the lack of credit can have in the economy, so both institutions coordinated their efforts to support the

[4]A detailed account of the impact of the global crisis in Chile can be found in De Gregorio (2009).

[5]Chile's banking system is large relative to the corporate debt market and most firms depend on it for external funding. See Sirtaine (2006) for details. In 2008 total private bank lending was around 80% of GDP while domestic outstanding private debt in the form of corporate bonds or commercial papers was approximately 15% of GDP. For comparison the average for OECD countries in 2007 was around 90% for bank lending and around 70% for private corporate debt.

[6]See Cowan and Marfan (2009) and the OECD's review of the Chilean financial system (2011).

financial system. The central bank focused on liquidity provision for banks, while the Chilean Government announced a package aimed to boost lending in the economy.[7] The government's idea was to reach firms directly. Among these measures, the most important in terms of scale and scope was the capitalization of the only state-owned and third largest bank in Chile, Banco Estado (BECH). The bank's mandate was to lend out the fresh funds in the shortest time possible while maintaining its credit standards.[8]

2.2 Policy change

The capitalization was announced in December 2008 and was implemented in two steps, in March and September 2009. It was large enough so as to allow BECH to increase lending while other banks were doing the opposite.[9] The government injected 500 million dollars of fresh funds, equivalent to a 36% increase in the bank's equity. Considering that the bank could leverage approximately up to ten times the amount of the capitalization, it implied a potential injection of five billion dollars in new loans, around 3.6% of the stock of total bank debt in the economy. By the end of 2009, BECH had lent 4.3 billion dollars in new debt, approximately a 22% increase in its portfolio. BECH's commercial lending increased by 24%, while it decreased by 13% on average at other banks. As a result, BECH's market share went up from 10% to 14% in one year. Figure 3 shows the impact of the policy in the bank's commercial lending. The dramatic difference between BECH and other banks indicates that the bank fulfilled its mandate and used the new funds to act counter-cyclically, increasing loans by a significant amount in a short period.

BECH conducted a nationwide effort in order to lend out the new capital without increasing the delinquency rate of its portfolio. Credit offers were made at very competitive terms to firms with good credit history across all industries and regions in the

[7] The government also implemented a complementary package to stimulate aggregate demand equivalent to 2.8% of GDP.

[8] BECH is the only public bank in Chile and although the board of directors is appointed by the government, its management is non-partisan. It provides a full range of financial services to both consumer and companies, operates in all regions of the country and has a well-diversified loan portfolio across all industries and business segments. As pointed out in Lagos and Tapia (2014), it is not a development bank and aims to be competitive and profitable. The above average—compared to private banks—annual returns of around 16% between 2002 and 2007 confirm this idea. As a commercial bank, it is subject to the same regulation as its competitors, except for an additional corporate tax of 40%. Also, its credit policies do not differ significantly from other banks. BECH was created in 1953, and its public role translates into an emphasis on the unbanked and the mortgage market. Initiatives like Cuenta RUT—a universal current account—and Caja Vecina—a banking outlet available in rural areas—are good examples of this public role. It is the bank with the largest mortgage portfolio, focusing on low and medium income households.

[9] Importantly, BECH was not in distress at any time, with a Basel ratio of 10.8 before the capitalization, well above the minimum required by law.

country. Although small firms were also considered, medium-sized and large companies were given priority because of their relevance in the trade credit and production network. Later in the paper, I show that this can be seen in the data.

The magnitude and scope of the policy intervention make the natural experiment analyzed in this paper unique. The crisis and the capitalization affected all types of firms; therefore, the sample of firms used in this paper is reasonably representative of the economy as a whole. Many firms in the sample are entirely bank-dependent, in contrast with other studies in the literature, which usually focus on relatively large firms that have access to capital markets.

3 Empirical strategy

3.1 Identification

In light of the studies that show that bank relationships are sticky and matter for greater availability of credit, we might expect BECH to prioritize its clients when lending out the funds from the capital injection.[10] Thus, the bank's lending shock in 2009 should translate into a positive credit supply shock during the crisis for BECH's clients relative to other firms. In Section 3.3, I show that this is true and quantify the magnitude of the credit shock.

Following this idea, I divide firms into two groups: those that had a banking relationship with BECH prior to the capitalization and those that did not. A firm is considered to have a banking relationship with BECH if it had at least one outstanding commercial loan with the bank between January 2005 and December 2008. Firms with a banking relationship with BECH—the treated—will be characterized by a dummy variable ($BeRel$) equal to 1.[11] The idea behind this definition is to capture whether the bank *knew* a firm before the capitalization. The way a banking relationship is measured in the literature varies across studies, sometimes focusing on the intensity of the relationship by using a continuous measure, as in Khwaja and Mian (2008) or Chodorow-Reich (2014). In this case, because the policy change affected only one bank, it is natural to use a binary variable, which also makes the estimates of both financial and real effects easier to interpret.

Then, I compare the average firm-level growth rate of different measures of banking debt, output, capital and labor for the two groups of firms over time. In the absence

[10] Borrowers and lenders form relationships to overcome inefficiencies caused by asymmetric information. Through a relationship, the firm provides information about its business, which reduces the expected cost of providing capital to the lender. See Chodorow-Reich (2014); Khwaja and Mian (2008); Petersen and Rajan (1994).

[11] I only observe loans from January 2005, which explains the time span in the definition.

of unobserved demand shocks affecting both groups differently, we could attribute any differences in the growth rates to the—causal—effects of a credit supply shock. Since I am comparing growth rates over time, the identification assumption can be stated as in a difference in differences approach (parallel trends): without the capital injection, explained variables would have followed the same trend during and after the recession.

However, banks and firms are not randomly matched, therefore in principle both groups may indeed be affected by different unobservable demand shocks. For example, BECH clients could be more sensitive to the business cycle and react differently to the aggregate shock in ways not captured by firms' observable characteristics, which will bias downwards the estimates on output, capital and employment. This could be the case if due to its public role, BECH focuses on more vulnerable firms. Alternatively, firms more sensitive to the business cycle could anticipate the shock and self-select into a banking relationship with BECH. In both cases, estimates would provide a lower bound of the causal effect. To address these identification concerns, I proceed in several steps.[12]

In order to obtain a comparison group for treated firms, I take advantage of the large number of firms without a banking relationship with BECH, and use a simple matching technique to find a similar counterfactual for each treated firm. I match firms on levels and growth rates of a large number of observables before the shock, which yields groups with similar trends and distributions in almost every firm characteristic before the capital injection. The matched sample not only complements the identification strategy but also allows me to compare the evolution of other moments different than averages within each group over time. I provide details on the construction of the matched sample in Section 3.2.3.

Moreover, the large number of firm characteristics in the data reduces the set of potential unobservable demand shocks that might affect both groups differently. In order to test to what extent these observables capture unobserved demand shocks, in Section 3.3 I follow Jiménez et al. (2011), Chodorow-Reich (2014) and Cingano et al. (2016), and show that the estimate of the credit supply shock at the firm-bank level does not change significantly when substituting the observables with a firm fixed effect.

Additionally, for the years before the shock, I check that both parallel trends and placebo tests are satisfied for all dependent variables at the firm-level. Furthermore, I check that parallel pre-trends are satisfied for variables observed at the monthly frequency, such as bank debt and employment. This evidence is provided in Section 4.

[12]The bank had behaved in a countercyclical way before, that is, lending relatively more than other banks during downturns and relatively less during expansions. In particular, before the 2008-09 crisis, the bank's countercyclical policy can be better seen during the expansion between 1993-97 and the Asian Crisis in 1998-99.

For the medium term, I check that shocks other than the capitalization are not driving the results. Also, I drop controls that switched to BECH right after the capitalization– around 6% of control firms—or consider them as treated firms, and check that firm-level results remain unchanged. Finally, I run regressions by dropping banks other than BECH to consider the possibility that subsequent negative bank-level shocks affect medium term results, which does not occur.

3.2 Data

3.2.1 Sources

This paper uses a new data set that links employment, banking and tax information for the universe of Chilean firms. The data are collected from administrative records from Servicio de Impuestos Internos (SII) - Chile's internal revenue service - from Administrador del Seguro de Cesantía (SC) - the unemployment insurance administrator - and from Superintendencia de Bancos e Instituciones Financieras (SBIF)- Chile's Bank Supervisor. These institutions use the information for their auditing or supervising activities, which means that most of the data is of high quality. Data sets are merged using the *Rol Único Tributario (RUT)*, a unique identifier for all observations. The panel covers from 2005 to 2012.

Tax record information from the SII has annual frequency and comes from the F22 form, which contains income tax declarations by corporations and individuals. This information is used to construct variables on firm characteristics such as value added, sales, total assets, fixed assets, wage bill, profits, age, industry and export status. I also use information on firms' employment from the DJ1879 and DJ1887 as a robustness check for the results obtained using the information from the unemployment insurance. The firms directly report all these variables except for the value added, which is constructed by subtracting total spending in inputs - directly reported in the F22 form - to the annual sales.

Employment information comes from different tables of the unemployment insurance database.[13] Unemployment insurance contributions are mandatory for all salaried workers in the private sector in Chile, therefore the information covers the universe of employer-employee matches for salaried jobs in the formal private sector.[14] The data has monthly frequency and worker information includes the wage, tenure on the firm and type of contract.

[13]The detailed content of each table is available (in Spanish) at www.safp.cl.

[14]A comparison of the aggregate employment information with that on the National Employment Survey of the National Institute of Statistics shows that the Unemployment Insurance represents approximately 90% of total salaried employment in the private sector.

Banking data includes the universe of bank-firm pairs and comes from debtor records of the Chilean Superintendency of Banks and Financial Institutions.[15] All banks are required to send the same information to the Supervisor with monthly frequency. Characteristics of the loans such as the interest rate or maturity are not available. The information on the amount of debt for each bank-firm pair is divided into several categories depending on the type of the loan (commercial, consumer and mortgage) and repayment status (days since the amount was declared non-performing). Throughout the paper banking information considers commercial debt only. To construct lending flows from the stock of debt, I use the change in the stock of debt at the firm-bank level, at the monthly frequency.[16]

The time unit in this paper is a year. For flow variables, I consider the total sum of flows over each year. These variables include sales, value added, wage bill and bank lending. For variables at the monthly frequency, I take averages over each year. These include employment, wages and delinquency rate. Stock variables such as fixed and total assets are measured in December of each year.

3.2.2 Full sample

A drawback of working with Chilean administrative records is that there is no unique definition of a firm. In particular, it is not possible to directly identify "productive firms" as many observations in the tax records correspond to "shell firms", created for investment/regulatory arbitrage purposes (usually tax avoidance). For example, roughly three-quarters of the observations identified as firms by the SII in a given year do not have employees. Thus, I start by constructing a full sample of firms with observations that can be defined as productive units. I use the full sample not only as a starting point for a matched sample but also as a robustness check for some of the firm-level results.

In this paper an observation in the tax records is considered to be a firm if it has at least one employee - from the unemployment insurance database—and if it is not a natural person, which means that the firm's RUT is different from the RUT of the owner(s). More important, it also means that all firms in the sample have limited liability. Additionally, I only consider firms that had outstanding commercial debt with the banking system at any point in time between January 2005 and December 2008—thus, that had at least one banking relationship before the policy change. According to these parameters, there are 85,472 firms in 2007. Starting from here, I drop observations with

[15] The "Manual del Sistema de Deudores" available (in Spanish) at www.sbif.cl contains a detailed description of the banking data.

[16] The amount of the new loan is the minimum between 0 and the positive change in the stock at the firm-bank level.

missing data on annual sales, industry and all observations in the Financial, Defense, Education and Health care and Public administration sectors. This procedure yields 77,634 firms in 2007.

As in other papers that study the impact of resource misallocation on TFP over time, such as Gopinath et al. (2017) and Oberfield (2013), I use a "permanent" sample of firms, that is I only keep firms that are active in every year from 2006 to 2012, which means that all firms in the sample have positive sales and at least one employee in each of these years.[17] A permanent sample of firms is justified by the fact that this paper's focus is on the intensive margin effect of finance on TFP, that is, on firms' growth and their ability to adjust optimally. Other channels involving firms' entry or exit, like technology adoption by new entrants or more productive firms exiting due to credit constraints are not considered in this study.[18]

A second reason that justifies the use of a permanent sample is that "shell firms" tend to have larger entry and exit rates, which distorts firm-level estimates. Therefore, the no-exit condition helps to a great extent with the identification of productive units. A potential issue related to the use of a permanent sample is survivorship bias. In this case, if any, we should expect a bias towards finding no effect of the credit supply shock on financial and real variables. If the credit crunch forced some of the firms to shut down, then the probability of exit should be lower for treated firms. In turn, this implies that there is a positive selection of firms in the control group, that is, on average these firms should be less vulnerable to the recession than in the treated group.

After dropping firms that exit the dataset, there are 40,127 firms in 2007. Table 1 provides summary statistics for treated and control firms in the full sample.

3.2.3 Matched sample

In order to minimize potential biases due to non-random firm-bank matching, I start by dropping all single-relationship firms in the full sample and all treated firms that have BECH as their primary lender. Thus, all treated firms in the matched sample have a relationship with BECH and at least one other bank. Firms in the control group have at least two relationships, and none of them is with BECH.

[17] Thus, I do not allow for exits. Given the design of the experiment, there are no entries in the sample because the treatment variable ($BeRel$) is defined for all firms before January 2009 only.

[18] Extensive margin mechanisms have been pointed out as relevant drivers of TFP. Midrigan and Xu (2014) argue that in the long run technology adoption by new entrants is more important than credit constraints to explain dispersion in marginal productivities. Eslava et al. (2015) and Hallward-Driemeier and Rijkers (2013), argue that credit -constrained high-productivity firms may be forced out of the market during recessions, hampering TFP. On the entry side, Queralto (2013) and Siemer (2016) propose models where financial shocks during recessions may prevent the entry of highly productive firms.

Then, I use a simple matching algorithm by estimating a probit regression for the probability of having a banking relationship with BECH in 2008. Explanatory variables include employment, average wage, the percentage of workers in indefinite contracts, sales per worker, fixed assets, total assets, age, lending, the percentage of non-performing loans and the number of active banking relationships in 2008. I also include two-year growth rates for lending, sales, employment, fixed assets, total assets and average wage. Finally, I include fixed effects for main lender, size (small, medium and large based on annual sales), industry and export status. For each firm related to BECH I pick the firm with the closest propensity score as the control firm.

I am not able to find a comparable firm for each treated firm in the full sample because I drop observations out of the common support. In particular, very large firms have many banking relationships and BECH is one of them in every case. Also, given that the propensity score estimation uses a main lender fixed effect as an explanatory variable, treated firms whose main lender was BECH are out of the common support, which further helps to balance the two groups. After the matching procedure, I have around 5,000 firms in each group.[19].

Table 2 provides summary statistics for the matched sample, while Figure 4 shows that treated and control firms are similarly distributed across industries.

3.3 Credit supply during the downturn

A typical exercise in studies that estimate the effects of bank-driven credit supply shocks is the quantification of the bank lending channel. I start the analysis of how the capitalization of BECH translated into more funding for some firms by quantifying how much more did firms borrow from BECH relative to other banks during the downturn. I do this by following the approach pioneered by Khwaja and Mian (2008), which recover credit supply shocks by controlling for observed and unobserved credit demand shocks. In this section, I also follow the related literature and use a test of omitted variables to assess the extent to which observables firm characteristics can capture unobserved credit demand shocks.

In order to quantify the impact of the capitalization on credit supply during the downturn, I run the following regression at the firm-bank level on the subset of firms that borrowed from BECH and at least one other bank before 2009:

$$GrL_{ib} = \alpha_i + \beta BE_b + \epsilon_{ib} \qquad (\textit{within borrower estimator}) \qquad (1)$$

[19]Main lender is defined as the bank that provided the largest amount of total loans to the firm between January 2006 and December 2008.

Where $GrL_{ib} = \frac{L_{ib,2009} - L_{ib,2007}}{0.5*(L_{ib,2009}+L_{ib,2007})}$ is the growth rate of credit—the sum of new commercial loans in a year—either from BECH or all other banks to firm i.[20] Thus, for each firm i at time t, $L_{ib,t}$ takes two values: the total amount of new loans in year t from BECH, and the total amount of new loans in year t from all other banks.[21] Following this idea BE_b is a dummy equal to 1 if b is BECH, and 0 for all other banks. α_i is a firm fixed effect intended to capture all observable and unobservable credit demand shocks (δ_i) between 2009 and 2007. Therefore, we are comparing changes in credit by different banks to the same borrower, and β is a measure of the credit supply shock due to the capitalization at the firm-bank level.

Columns (1) and (3) in Table 3 show the estimates of β for the full and the matched sample, respectively. Their magnitude confirms what was already suggested by Figure 3: after the capitalization, BECH was able to increase its commercial lending while other banks were contracting their credit supply. In fact, according to both samples, on average, firms were able to increase their borrowing from BECH by more than 20% relative to the rest of the banking system during the crisis in 2009. Also, it is important to notice that the estimates of the credit supply shock for the full sample and the matched sample are very similar—23% and 22%, respectively—which supports the matching procedure.

Furthermore, diving the sample into small, medium and large firms, and running Equation (1) separately for each group we can assess the heterogeneous effect of the capitalization.[22] Table 4 shows the results of this exercise for the full and the matched sample. The credit supply shock was larger for larger firms, which confirms the idea that BECH gave priority to medium and large firms when lending out the fresh funds due to their relevance in the trade credit and production network.

Using the specification in Equation (1) it is possible to assess whether unobserved credit demand shocks differ between BECH's and other banks' borrowers in this subset of firms—that is, whether $Corr(\delta_i, BE_b \neq 0)$. Jiménez et al. (2011) proposed a test for this purpose, which has been used in other studies like Chodorow-Reich (2014) and Cingano et al. (2016). The idea is to compare the estimate of β in Equation (1) with

[20]This measure—proposed by Davis et al. (1996)—corresponds to a second order approximation of the growth rate. It is bounded between -2 and 2, and can handle entries and exits. These features make it particularly convenient in this setting with many outliers and where exits are possible ($L_{ib,2009} = 0$).

[21]Some related papers in the literature recover credit supply shocks at the firm-bank level using banks exposition to an exogenous shock. In that case, the right-hand side variable in Equation (1) corresponds to an ex-ante measure of the exposure of each bank to the exogenous shock. The specification in this paper can be thought analogously as an extreme case where there are only two banks and only one of them is affected by the shock. This also means that a measure of exposure is not required in this case.

[22]Small: less than 25000 UFs in annual sales; Medium: between 25000 and 100000 UFs in annual sales; Large: more than 100000 UFs in annual sales. UF is an inflation-indexed unit of account widely used in Chile. 1 UF $\approx$ 33 USD in Dec. 2008.

$\tilde{\beta}$ in the following regression

$$GrL_{ib} = \tilde{\psi} X_i + \tilde{\beta} BE_b + \tilde{\epsilon_{ib}} \tag{2}$$

which replaces the firm fixed effect (α_i) in Equation (1) by a set of observable firm characteristics (X_i). A significant difference between the two estimates would imply that observables are not able to capture unobservable differential credit demand shocks. Moreover, following the same idea, comparing $\hat{\beta}$ with the estimate of a regression of GrL_{ib} on BE_b (that is, Equation (2) without the controls X_i) in the matched sample provides an idea of how well the matching procedure captures these unobserved demand shocks.

Table 3 shows that the difference between the estimates of all three specifications is not significant— and again the estimates for the full and the matched sample are very similar—which suggest that observable firm characteristics capture unobserved credit demand shocks. This result is not surprising given the large number of firm characteristics available in the data. Importantly, firm-level regressions in the following sections—where it is not possible to include a firm-fixed effect—use the same set of firm characteristics as controls. Thus, we should not expect large differences in the magnitude of unobserved credit demand shocks affecting BECH's clients and other firms. These observable characteristics include main lender, size (small, medium and large based on annual sales), industry and export status fixed effects before 2009.

4 Firm-level effects of the credit crunch

4.1 Specification

The estimate for Equation (1) shows that the capitalization of BECH translated into a sizable credit supply shock in 2009, but it does not show its effect on total bank credit at the firm-level. In turn, any differential impact of the crisis on real variables such as capital, employment and productivity should be explained by differences in the total availability of credit between firms connected to BECH and other firms. In contrast with the previous exercise, the impact on total borrowing depends on the relative credit supply effect of BECH versus all other banks. Thus, on average we should expect a smaller effect when measuring the impact of the capitalization on total bank credit.

In order to measure the impact of the capitalization at the firm-level I use the same specification for both financial and real variables. I run a different regression for each year after the shock to assess the cumulative impact of credit availability over time. I always take 2007 as the base year, before Chile was hit by the global recession. The firm-level impact is estimated with the following specification:

$$GrY_{it} = \hat{\psi}^Y X_i + \hat{\beta}_t^Y BeRel_i + \hat{\delta_{it}^Y} + \hat{\epsilon}_{it}^Y \qquad (\textit{between borrower estimator}) \qquad (3)$$

Where $GrY_{i,t} = \frac{Y_{i,t} - Y_{i,2007}}{0.5*(Y_{i,t}+Y_{i,2007})}$ is the growth rate of variable Y_i from 2007 to year $t \in \{2009, 2010, 2011, 2012\}$. $BeRel_i = 1$ if firm i had at least one outstanding loan with BECH between January 2005 and December 2008, and 0 otherwise; X_i is the same set of fixed effects used for the omitted variables test in the previous section; and $\hat{\delta_{it}^Y}$ is an unobservable shock affecting Y_{it} but not captured by X_i. All covariates are measured before the policy change. In addition, for all dependent variables I run two placebo regressions for the growth rate for years 2007 and 2008 with base year 2006 and no controls ($\hat{\psi}^Y = 0$). Finally, standard errors for all regressions are clustered at the firm-level.[23]

Main explained variables Y_i include: the sum of new commercial loans to firm i - L_i; value added and revenue, as measures of output; the book value of fixed and total assets as measures of capital stock; and number of workers from both tax records and unemployment insurance records, average wage and wage bill as measures of employment. In Section 11 in the Appendix I report estimates of the elasticity of real variables to bank credit during the downturn, obtained from a two-stage least squares specification.

Parameters β_t^Y measure the average cumulative impact at year t of credit availability during the downturn on variable Y. Identification of causal effects requires that $Corr(BeRel_i, \hat{\delta_{it}^Y}) = 0$. To support the identification assumption, in addition to including the covariates that arguably capture unobservable credit demand shocks, I provide evidence of no differential effect between treated and control firms in the two years previous to the capitalization of BECH, by running placebo experiments in years 2007 and 2008, and by showing that pre-trends of explained variables are similar for both groups. Moreover, bank loans and employment, the two variables I observe at the monthly frequency, co-move before the policy change. Figures 5a and 5b show graphical evidence of same pre-trends for these variables at the monthly frequency. They also show how the credit crunch and the capitalization of BECH affected treated and controls firms differently. In particular, bank credit falls sharply for both groups after September 2008, but start recovering faster for treated firms immediately after BECH started its counter-cyclical policy. Employment instead, falls for both groups during the crisis starting in January 2009, and starts recovering faster for treated firms around

[23]The small number of banks in Chile, around 20, does not allow clustering at the bank-level.

September 2009, once the adverse aggregate demand shock had passed.[24]

Although in this setting it is not possible to completely rule out a correlation between $BeRel_i$ and unobservable shocks, the fact that placebo and pre-trends test are satisfied for all variables provides reassuring evidence that this is not the case.

4.2 Credit

Figure 6a shows the evolution of the average (normalized log of) annual flow of new commercial loans for treated and control firms between 2007 and 2012. The vertical distance between the dashed and solid lines provides a good idea of the differential impact of the crisis on credit growth over time. The same rapid growth during 2007 and 2008 provides compelling graphic evidence supporting the parallel trends assumption. It also reflects the boom and expansionary credit conditions that the Chilean economy experienced before the crisis, which ended abruptly in 2009. In line with the evolution of aggregate lending shown in Figure 2, credit falls on average 25% in the sample in 2009 and does not recover its pre-crisis level in the years following the shock.[25]

Table 5 contains the estimates of the differential impact of the crisis. For each year between 2007 and 2012, in addition to the specification in Equation (3), Panel (b) in the table contains results from an alternative specification without controls ($\hat{\psi}^L = 0$). The insignificant difference between these estimates for all years provides further evidence supporting the assumption that $Corr(BeRel_i, \hat{\delta^L_{it}}) = 0$. Consistently, all placebo estimates (years 2007 and 2008) are insignificant.

Estimates in Panel (a) are the counterpart of the dramatic difference in credit growth in 2009 depicted in Figure 6b. They are significant at the 1% level and imply that on average, credit fell 14.7% less in firms connected to BECH. The estimate is lower but not too far from the effect at the firm-bank level shown in section 3.3 ($\sim$ 21%), which shows that to some extent BECH was able to offset the contraction in lending from other banks for treated firms, while control firms could not immediately switch to BECH.

Columns (1)-(4) contain the estimates of credit growth in the years following the

[24] In a related study, using the same natural experiment, I provide a detailed analysis of the effect of the credit crunch on employment and the labor market. I show that the initial reaction to the downturn was similar for both groups of firms, with layoffs of workers in temporary contracts, followed by a faster recovery of hirings in treated firms. This difference in the hiring rate towards the end of the recession in Chile explains the difference in the level of employment documented in section 4.3 of this paper.

[25] On the contrary, aggregate lending in the economy recovered by the end of 2011. Different facts explain this difference. Firstly, the permanent sample does not have entries. Secondly, a composition effect affects firm-level credit growth, as due to the logarithm, all firms are given equal weights, and average debt decreased more and more persistently for small firms. In fact, average lending—weighted by the total amount of debt— recovers for treated firms in the sample and follows a pattern similar to that of aggregate lending for the Chilean economy.

recession. They show that the credit supply shock of 2009 had a persistent effect on firm's banking debt: lending did not catch up with the treated for control firms, even after overall lending conditions improved in the economy. Figure 6b contains a graphic representation of these results.

4.3 Output, employment and capital

Figures 7a, 8a and 9a show the evolution of the average (normalized log of) annual sales, total assets and number of workers for treated and control firms between 2007 and 2012, respectively. Similar trends before the shock for both groups of firms provide further graphic evidence of no correlation between firm growth and having a lending relationship with BECH. Rates of growth of firm-level output, employment and capital in the sample also echo the evolution of the same aggregate variables for the Chilean economy. On average, annual sales fall 5.8% in 2009 but recover in 2010, while employment stops growing during the crisis. The growth rate of average total assets falls during 2009 but remains positive, consistent with larger adjustment costs and the slowdown in investment.

Tables 6, 7 and 8 show the estimates of the differential impact of the crisis on different measures of firm-level output, employment and capital. Again, the irrelevance of controls for most regressions and the finding of no real effects in 2007 and 2008 provide evidence supporting the identification assumption.

Columns (1)-(2) in tables 6, 7 and 8 show that the lack of external credit during 2009 had immediate real effects, which persisted over the following years. Coefficients of all variables except the average wage are significant at the 1% level for all years after 2008. The 14.7% difference in lending during 2009 translated into approximately 3% more output growth, 2.6% more employment growth and 4.2% more capital growth for treated firms during the crisis. These coefficients are similar to those reported by authors studying the direct effects of banking shocks during the Great Recession in different countries, like Bentolila et al. (2017) for Spain, and Huber (2018), for Germany. In particular, Huber uses a similar experiment and specification to assess the effects on clients of a single bank that experienced a negative balance sheet shock in 2008. Huber finds that a negative 20% shock on total banking debt in the short run translates into 5.5% lower growth of employment over the four years following the shock, implying an elasticity to credit of around 0.28. In this case, the average effect on employment in the four years following the capitalization is 4.25%, while the short run effect on credit is 14.7%, implying the same elasticity.

Columns (3)-(4) in tables 6, 7 and 8 provide evidence showing that firms with less access to external finance became not only more affected by the adverse aggregate shock, but they also grew more slowly during the recovery phase. Point estimates for 2010 show that the differential impact increases during the recovery and tends to stabilize in years 2011-2012. These results are consistent across all measures of output, capital and employment at the firm-level, and lead to persistent real effects in the medium term. During the four years following the shock, on average, sales grow 6.7% more for treated firms, while the wage bill and fixed assets increase 4.85% and 8.7% more respectively.

5 Productivity

5.1 Technical efficiency and firm-level productivity

The finding that firms more affected by the credit crunch grew less in the years following the recession shows that credit shocks can have persistent effects on output at the firm-level. To what extent is the effect on output explained by changes in firm-level physical productivity? A credit crunch may damage firms' productivity by reducing innovation, by destroying different forms of firm-specific capital or by preventing firms undertaking productivity-enhancing adjustments that should be optimally concentrated during downturns.[26] In turn, changes in individual productivity aggregate into changes in technical efficiency at the economy-level.[27]

In order to quantify the impact of the credit shock on firm-level physical productivity, I perform a simple calculation. Assuming a constant returns to scale Cobb-Douglas value added production function with output elasticity to capital α, the growth of a firm's productivity is approximately

$$\Delta a_{it} = \Delta y_{it} - \alpha \Delta k_{it} - (1-\alpha)\Delta l_{it} \tag{4}$$

where lowercase denote logs and Y_{it} is the physical output of firm i in year t. Unfortunately, Y_{it} is not observable. Instead, I observe value added, that is PY_{it}.[28] To

[26]See Caballero and Hammour (1996), where firms optimally concentrate adjustment during recessions due to the lower opportunity cost.

[27]A higher level of technical efficiency (i.e. physical productivity) allows a firm to generate more output holding inputs constant. Baqaee and Farhi (2017), decompose aggregate productivity into a "pure" technology component and a misallocation component. The former corresponds to the (cost-based) weighted average of individual physical productivities. Note however that estimates in the previous section correspond to firm-level growth rates, and therefore to equally-weighted averages.

[28]Foster et al. (2008) emphasize the importance of distinguishing between physical productivity (TFPQ) and revenue productivity (TFPR). Changes in TFPR can be measured by replacing physical output with value added in Equation (4), which shows that changes in TFPR are explained by changes in TFPQ and changes in the prices charged by the firm. In the aggregate, changes in firms' TFPQ would affect productivity through technology, while changes in prices would reflect changes in the

circumvent this issue, I assume that in normal times all firms charge the same time-invariant markup μ over their marginal cost. Optimization implies that

$$\Delta^{\%} PY_{it} \approxeq \Delta^{\%} Y_{it}(1/\mu) \tag{5}$$

and therefore

$$\Delta a_{it} = \mu \Delta py_{it} - \alpha \Delta k_{it} - (1-\alpha)\Delta l_{it}$$

which allows me to quantify the impact of the credit shock on firm-level physical productivity in the medium term. Using the estimates in the previous section:

$$\mathbb{E}[\Delta a_{it}^{BeRel=1} - \Delta a_{it}^{BeRel=0}] = \mu \beta_t^{valadd} - \alpha \beta_t^{capital} - (1-\alpha)\beta_t^{labor} \tag{6}$$

I try to obtain values for α and μ from the data. As in De Loecker and Warzynski (2012), differentiating Y with respect to any non-dynamic factor of production in Equation (5) yields the relationship between α and μ in the absence of perfect competition,

$$\frac{P_i^X X_i}{P_i Y_i} = \mu^{-1} \alpha_i^X \tag{7}$$

which shows that markups drive a wedge between the share of the expenditures on total sales of any variable input and α^X, the elasticity of that input to output.

I obtain values for this expression considering a gross output production function and using materials (M) as the variable input factor, as they are not expected to be subject to adjustment costs, or to be affected by distortions due to the credit shock. I obtain the average share of expenditures from the sample in 2007, which is 0.597. In order to obtain the average value of α^M, I take advantage of the exogenous shock to credit in 2009 and use the fact that

$$\frac{\beta_{2009}^{Sales}}{\beta_{2009}^{M}} = \frac{\mathbb{E}[\Delta^{\%} PY_{2009}]}{\mathbb{E}[\Delta^{\%} M_{2009}]} = \frac{0.02975}{0.03233} = 0.9201 \approx \mathbb{E}[\alpha^{Rev,M}]$$

where βs correspond to the estimates for sales and materials in the previous section,

allocative efficiency of the economy.

and $\mathbb{E}[\alpha^{Rev,M}]$ is the expected value of the elasticity of sales to materials. Assuming a similar price setting behavior during the recession between treated and control firms, implies $\mu = 1.82$, which can be used to obtain the consistent value for α in Equation (4).[29] Using the share of labor expenditures on total sales in 2007 (0.27) and Equation (7), yields $\alpha = 0.51$. Finally, replacing α and μ in Equation (6) for $t = 2012$ yields the average cumulative effect on firms' physical productivity,

$$\mathbb{E}[\Delta a_{i,2012}^{BeRel=1} - \Delta a_{i,2012}^{BeRel=0}] = 1.82 * 2.95\% - 0.51 * 8.7\% - 0.49 * 4.85\% \approx 1.52\%$$

Therefore, credit availability during the downturn could have had a moderately positive effect on individual productivity in the medium term. However, given that a value for μ of 1.82 is relatively high, 1.52% is likely an upper bound of the true effect, which implies that the impact of the credit shock on firm-level output is largely explained by factor accumulation rather than changes in firms' productivity.[30]

In summary, the evidence analyzed in this section suggests the absence of a strong technology effect that might explain protracted recoveries after financial crises, as put forward by related papers in the literature such as Huber (2018), Manaresi and Pierri (2018) and Levine and Warusawitharana (2014). In the next section I study the impact of the credit crunch on allocative efficiency and quantify its effect on total factor productivity. To do this, I depart from firm-level estimates.

5.2 Allocative efficiency and TFP

A credit crunch may also affect an economy's ability to allocate its resources to their most productive use. All the more so during recessions, when the opportunity cost of reallocating resources is lower. Firms with high marginal productivity of capital

[29] This assumption might not be unreasonable in the context of the empirical setting in this paper. Much of the price adjustment during the downturn is likely to be explained as a reaction to the adverse aggregate demand shock, which on average affects treated and control firms in the same fashion.

[30] For firms in the US, De Loecker and Warzynski (2012) obtain values of μ lower than 1.3, while Garcia-Marin and Voigtländer (forthcoming) obtain industry-average values around 1.45 for Chilean exporters. One drawback of using Chilean tax record information for estimating production functions is measurement error. While this is not a problem for the estimates in the previous section, which capture differences over time, it might affect the estimated levels of μ and α, and therefore the estimated effect of the credit shock on productivity. In particular, expenditures on materials might include a firm's expenses on capital and labor renting - for example, leasing or outsourcing services. If this is the case, both $\frac{P_i^X X_i}{P_i Y_i}$ and α_i^X in expression (7), would be overestimated, biasing μ in an unknown direction. As a robustness check for the value of μ, I use information on material purchases for the sample from VAT records, which should be less affected by this particular form of measurement error. With this alternative source of information I can obtain a value for the elasticity but not for the expenditures share. I get $\alpha^M = 0.87$, which implies a markup of 1.46, and no effect on firms' physical productivity.

(MRPK) that lack access to external financing may not be able to acquire new units of capital. In turn, firms with low MRPK might not be able to sell the same units. Therefore, *"...financial frictions may generate differences in the returns of capital across individual producers, and thus efficiency losses due to misallocation"*.[31] In this section, I assess the magnitude and persistence of the effect of the credit crunch on allocative efficiency. To do so, I first examine how the dispersion of marginal productivities of capital within the treatment and control groups evolves over time. I show that it increases and remains persistently higher after the downturn within the group of firms more affected by the credit crunch, which suggests the existence of a relevant allocative efficiency channel on TFP. I then use the accounting framework in Hsieh and Klenow (2009)—henceforth HK—to quantify the effect of the credit crunch on TFP due to changes in allocative efficiency versus changes in technical efficiency. I find that differences in TFP growth four years after the shock explain roughly a third of the differences in total output growth between groups. In line with the results on firm-level productivity in the previous section, the allocative efficiency channel explains around two-thirds of the differential TFP growth, that is, twice as much as the technical efficiency channel.

5.2.1 The HK framework

The framework proposed by Hsieh and Klenow (2009) has become the standard methodology for a vast literature devoted to the study of the effects of misallocation on TFP. Although this framework has not been exempt from critics, its simplicity, combined with the fact that here I focus on changes in allocative efficiency rather than levels of misallocation, make it suitable for this paper.[32] I begin by briefly describing the HK framework and how it allows recovering marginal productivities of capital from firm-level data.

The model assumes that there is a single final good Y_t in a competitive market that is produced combining the output of S industries:

[31] Midrigan and Xu (2014)

[32] For a critical discussion of the framework see Albagli et al. (2019), Haltiwanger et al. (2018) and Foster et al. (2016). Notice however that some of these critics do not apply to this paper, as I use the HK methodology to find effects that are differentiated over treated and controls firms, and over time (as in a differences in differences setting). For example, any misspecification of the model, or assumptions on demand and supply structure will affect both groups in the same fashion and therefore disappear when comparing the evolution of variables for each group of firms over time. In particular, the fact that the levels of TFP, allocative and technical efficiency might not be correctly estimated, does not affect the main results in this paper.

$$Y_t = \prod_{s=1}^{S} Y_{st}^{\theta_{st}}$$

with $\sum_s \theta_s = 1$. Each industry s at time t is populated by N_s monopolistically competitive firms.[33] Industry output Y_{st} is a CES aggregate of each firm's differentiated good:

$$Y_{st} = \left(\sum_{i=1}^{N_s} Y_{sit}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$$

where σ denotes the elasticity of substitution between varieties, which is assumed to be constant across all firms and over time. Each firm has a constant returns to scale Cobb-Douglas production technology:

$$Y_{sit} = A_{sit} K_{sit}^{\alpha_s} L_{sit}^{1-\alpha_s}$$

Firms face distortions that increase the marginal productivity of capital relative to labor (τ_K), and distortions that increase the marginal product of both factors proportionally (τ_Y). These show up as wedges in the firm' f.o.c.. Optimization implies that

$$P_{sit} = \frac{\sigma}{\sigma - 1} \frac{1}{A_{sit}} \left(\frac{R_t}{\alpha_s} \right)^{\alpha_s} \left(\frac{w_t}{1 - \alpha_s} \right)^{1-\alpha_s} \frac{(1 + \tau_{Ksit})^{\alpha_s}}{1 - \tau_{Ysit}}$$

$$Y_{sit} \propto \frac{A_{ist}^{\sigma} (1 - \tau_{Ysit})^{\sigma}}{(1 + \tau_{Ksit})^{\alpha_s \sigma}}$$

$$\frac{K_{sit}}{L_{sit}} = \frac{\alpha_s}{1 - \alpha_s} \frac{w_t}{R_t} \frac{1}{(1 + \tau_{Ksit})} \tag{8}$$

where $\mu = \frac{\sigma}{\sigma-1}$ is a constant markup over the firm's marginal cost, R is the rental rate of capital and w is the wage.[34] Both the price and output of firm i depend not only

[33] Here N_s does not vary over time, as there is no exit or entry in the sample.

[34] Wages are assumed constant across firms, which in this case is supported by the data. In particular,

on its physical productivity level A_{it} - a firm's measure of technical efficiency - but also on the magnitude of distortions it faces. Firm-level marginal productivity of capital and labor, revenue productivity and physical productivity can be measured directly in the data:

$$MRPK_{sit} \triangleq \alpha_s \frac{\sigma - 1}{\sigma} \frac{P_{sit} Y_{sit}}{K_{sit}} \tag{9}$$

$$MRPL_{sit} \triangleq 1 - \alpha_s \frac{\sigma - 1}{\sigma} \frac{P_{sit} Y_{sit}}{L_{sit}} \tag{10}$$

$$TFPR_{sit} \triangleq P_{sit} A_{sit} = \frac{P_{sit} Y_{sit}}{K_{sit}^{\alpha_s} (w L_{sit}) 1 - \alpha_s} \propto (MRPK_{sit})^{\alpha_s} (MRPL_{sit})^{1-\alpha_s} \tag{11}$$

$$TFPQ_{sit} \triangleq A_{sit} = \frac{(P_{sit} Y_{sit})^{\frac{\sigma}{\sigma-1}}}{K_{sit}^{\alpha_s} (w L_{sit})^{1-\alpha_s}} \tag{12}$$

Finally, sector and economy level TFP can be obtained with the following expressions:

$$TFP_{st} = \left[\sum_{i=1}^{N_s} \left(A_{sit} \frac{\overline{TFPR_{st}}}{TFPR_{sit}} \right)^{\sigma-1} \right]^{\frac{1}{\sigma-1}} \tag{13}$$

$$TFP_t \triangleq \prod_{s=1}^{S} TFP_{st}^{\theta_{st}} \tag{14}$$

where $\overline{TFPR_{st}}$ is the geometric average of $TFPR_{sit}$ in sector s at time t. Equations (11) and (13) show that differences in $TFPR_{st}$ across firms reflect the existence of distortions that move TFP_{st} away from its technically efficient level, $\overline{A}_{st} = \left(\sum_{i=1}^{N_s} A_{st}^{\sigma-1} \right)^{\frac{1}{\sigma-1}}$. The impact of these distortions on aggregate productivity is captured by the ratio $\Gamma_{st} = \overline{A}_{st} / TFP_{st}$.

there is no difference in firm-level average wages between the treated and controls before or after the recession. R is also constant across firms, therefore credit shocks are captured by τ_{Ksit}.

5.2.2 The credit crunch and the dispersion in MRPKs

A negative credit supply shock, such as the one suffered by controls relative to treated firms during the downturn, is expected to have a large—positive—effect on τ_K, which would reduce the capital to labor ratio, as shown in Equation (8). On average, control firms should exhibit a lower capital to labor ratio while being more credit constrained. Tables 7 and 8 show that this is precisely what happened to the firms in the sample: for all measures of capital and labor, $\beta^K > \beta^L$ in Equation (3) during and after the recession. Thus, reduced-form estimates for capital and labor obtained in Section 4.3 are consistent with a large shock to τ_K due to the credit crunch.

Furthermore, firms are heterogeneously affected by credit shocks, which implies that a credit crunch may affect not only the average value of τ_K, but also its dispersion in the economy, therefore also increasing the dispersion in $TFPR_{si}$ and misallocation. In turn, the loss in allocative efficiency would be reflected in a lower level of TFP_s. I use Equation (9) to obtain values of MRPK for each firm for all years in the sample. As in HK and other papers in the literature, I estimate α_s from the data in 2007 and assume $\sigma = 3$. Table 9 shows α_s for each sector in the sample.[35]

Figure 10a shows the evolution of the interquartile range of MRPK for treated and control firms over time. The dispersion in marginal productivities follows the same trend before the crisis in both groups of firms. However, during the downturn and the year after, it increases within the group of firms more affected by the credit shock. Notably, the largest increase is in 2010, the first year after the crisis. It then remains at the same level until the end of the sample period. Thus, the evidence suggests that the lack of credit during the recession harmed the optimal reallocation of resources. Moreover, the effect seems to be persistent over time.[36]

What is driving this result? Figures 10b and 10c show the evolution of the 75th and 25th percentiles of MRPK for both groups of firms. While the trends of the 25th percentile for treated and controls are similar all over the sample period, the 75th percentile increases for control firms after 2008. Thus, the increase in the dispersion of MRPK is driven by credit constrained firms that have too few capital relative to their optimal level. On the other hand, the credit shock does not seem to affect the lower tail of the MRPK distribution. These results are likely to be explained by the heterogeneous effect of the credit shock, with only some firms receiving a large τ_K shock, and are consistent with the existence of an allocative efficiency channel through which credit crunches affect TFP. Next, I turn to the quantification of this channel, and its

[35] As in Section 4, capital is measured as the book value of fixed assets.

[36] The dispersion and the level of MRPL remain virtually equal for treated and control firms over time.

importance versus other channels.

5.2.3 Allocative versus technical efficiency

Combining and differentiating Equations (13) and (14) yields

$$\Delta^{\%}TFP_t \approxeq \Delta^{\%}\overline{A}_t - \Delta^{\%}\Gamma_t \tag{15}$$

where $\overline{A}_t$ is the level of technical efficiency in the economy, and Γ_t is a measure of misallocation in the economy. Thus, changes in TFP can be decomposed into changes of an average "scale" component that captures changes in technical efficiency; and changes in the dispersion of $TFPR_i$, which capture how efficiently are capital and labor allocated within the economy. Furthermore, taking advantage of the fact that treated and control firms have similar distributions for most observable characteristics before the credit crunch (see Table 2) it is possible to compare the evolution for each of these components over time, as in a differences in differences setting:

$$\begin{aligned} \Delta^{\%}TFP_t^{BeRel=1} - \Delta^{\%}TFP_t^{BeRel=0} \approxeq & \left(\Delta^{\%}\overline{A}_t^{BeRel=1} - \Delta^{\%}\overline{A}_t^{BeRel=0}\right)\dots \\ & - \left(\Delta^{\%}\Gamma_t^{BeRel=1} - \Delta^{\%}\Gamma_t^{BeRel=0}\right) \end{aligned} \tag{16}$$

Thus, differences between treated and control firms in Equation (16) provide a partial equilibrium measure of the impact of the credit crunch on TFP through each channel. The second term in the right hand side of Equation (16) captures differences in the dispersion of $TFPR_i$ within each group of firms over time.[37] Therefore, it measures the effect of the credit shock on productivity through changes in allocative efficiency. Importantly, any other difference in TFP between treated and controls will be captured by the first term in parenthesis, which corresponds to changes in the difference of the efficient level of TFP in each group. Thus, the effect of the credit shock on TFP through changes in technical efficiency may include other effects and could be overestimated.[38]

Furthermore, it is possible to quantify the impact of the credit shock on output

[37] More precisely, it captures changes in the dispersion of $TFPR_i$ and in its correlation with $TFPQ_i$. This can be seen in the following expression for misallocation:
$\Gamma = \frac{1}{\sigma-1}\left[log\left(\mathbb{E}\left[A^{\sigma-1}\right]\mathbb{E}\left[\left(\frac{\overline{TFPR}}{TFPR}\right)^{\sigma-1}\right] + Cov\left(A^{\sigma-1}, \left(\frac{\overline{TFPR}}{TFPR}\right)^{\sigma-1}\right)\right)\right] - \frac{1}{\sigma-1}log\left(\mathbb{E}\left[A^{\sigma-1}\right]\right)$.

[38] For example, changes in average markups between treated and controls, which in this settings are assumed constant over time, should appear as changes in technical efficiency. In other settings, such as in Baqaee and Farhi (2017) or in Petrin and Levinsohn (2012) changes in flows to firms with different markups are accounted by as changes in allocative efficiency.

growth through TFP growth versus factor accumulation with the following expression:

$$\begin{aligned}\Delta^{\%}Y_t^{BeRel=1} - \Delta^{\%}Y_t^{BeRel=0} \approxeq \Delta^{\%}TFP_t^{BeRel=1} - \Delta^{\%}TFP_t^{BeRel=0} \dots \\ + \sum_s \alpha_s \theta_{st}^{BeRel=1} \Delta^{\%} K_{st}^{BeRel=1} \dots \\ - \sum_s \alpha_s \theta_{st}^{BeRel=0} \Delta^{\%} K_{st}^{BeRel=0} \dots \\ + \sum_s (1-\alpha_s)\, \theta_{st}^{BeRel=1} \Delta^{\%} L_{st}^{BeRel=1} \dots \\ - \sum_s (1-\alpha_s)\, \theta_{st}^{BeRel=0} \Delta^{\%} L_{st}^{BeRel=0}\end{aligned} \tag{17}$$

where everything in the right hand side of expression (17) can be obtained from the data, while physical output Y_t is not observed. In order to calculate output, TFP and its components in Equations (16) and (17), I follow Hsieh and Klenow (2009) and drop firms in the upper 2% of the distributions of $TFPQ_i$ and $TFPR_i$ each year and assume $R = 0.1$ constant over time.

Figure 11 shows the evolution of aggregate value added—observed in the data—and aggregate TFP for treated and control firms. As in the case of firm-level averages for the sample, both aggregate variables follow similar trends before the recession for treated and control firms, but depart afterwards. Thus, aggregated output growth for control firms resembles that of countries after a financial crisis, like the US after the Great Recession. From 2007 to 2012, value added grew 11.7% more for treated firms.

During the downturn, TFP falls sharply for both groups—likely as a result of lower capacity utilization—but remains higher for the group of firms that did not experience a credit crunch. As shown in Table 10, four years after the shock, physical output growth—obtained using Equation (17)—was 22% higher, while TFP growth was 7.2% higher for the same group of firms. Therefore, around a third of the impact of the credit crunch on medium term output growth is explained by TFP growth.

Finally, Table 10 also shows the decomposition of the impact of the credit crunch on TFP. Consistent with the firm-level evidence in sections 5.1 and 5.2.2, four years after the shock, allocative inefficiencies explain around 65% of the TFP effect, that is twice as much as the difference in technical efficiency due to the credit crunch.

6 Conclusion

This paper provides evidence of the effects of a credit crunch on output and productivity, assessing its impact through changes in technical efficiency versus changes in allocative efficiency.

I use a large-scale natural experiment during the 2008-09 recession in Chile to show that a large and sharp contraction in the supply of credit during the downturn leads to a persistent effect on output. As in the aftermath of financial crises documented for some countries, output of more affected firms does not catch up with that of less affected firms. This persistent effect is only partially explained by differences in firm-level physical productivity growth. Moreover, the dispersion of the marginal productivity of capital increases within firms more affected by the credit crunch. These findings suggest that other factors different than lower innovation due to the lack of credit may help to explain the protracted recovery pattern of output after financial crises. In fact, a quantification of the effect of the credit crunch on TFP using the framework in Hsieh and Klenow (2009) shows that the effect through allocative efficiency can be twice as large than that through technical efficiency.

Even though these results abstract from general equilibrium considerations, they contribute to the understanding of how the lack of credit might affect growth in the aftermath of financial crises.

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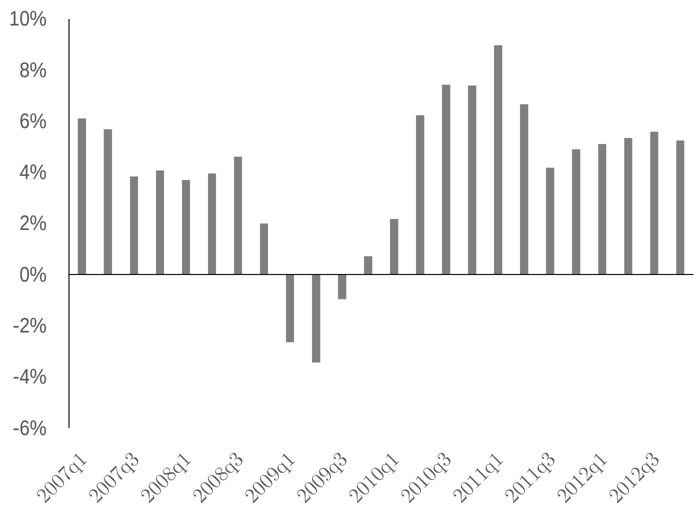
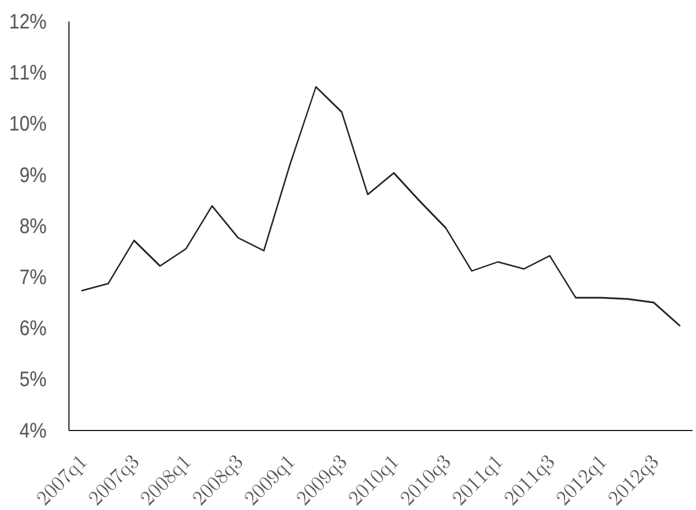
7 Appendix

7.1 Figures

Figure 1: The 2008-09 crisis in Chile

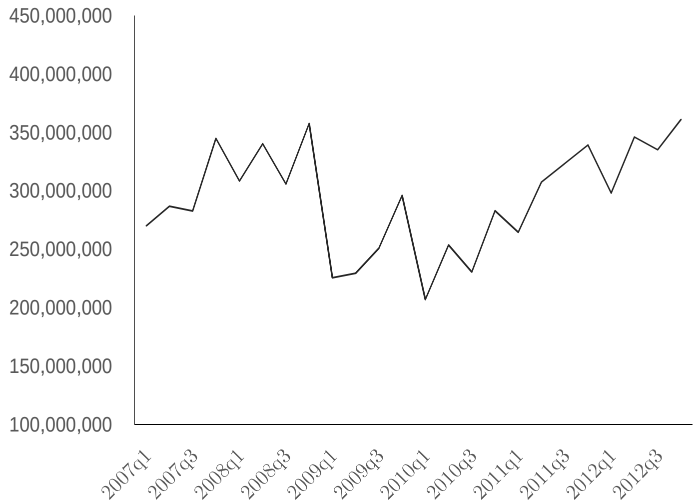


The figure above shows the evolution of Chile's real GDP. Annual growth rate, chained 2013. Source: Central Bank of Chile.



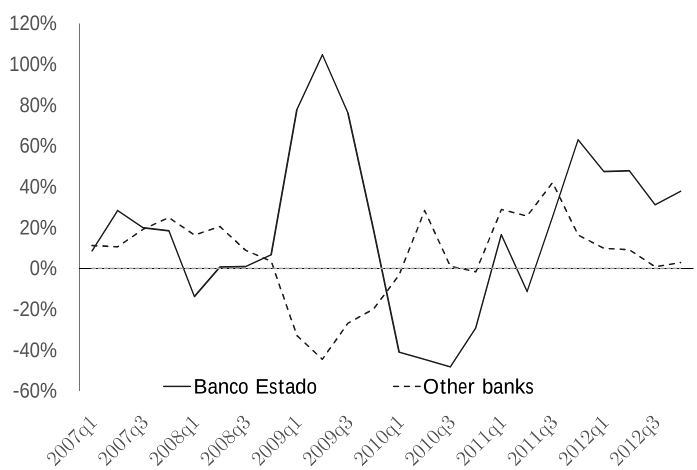
The figure above shows the evolution of Chile's unemployment rate. Source: Central Bank of Chile.

Figure 2: Bank commercial lending during the crisis



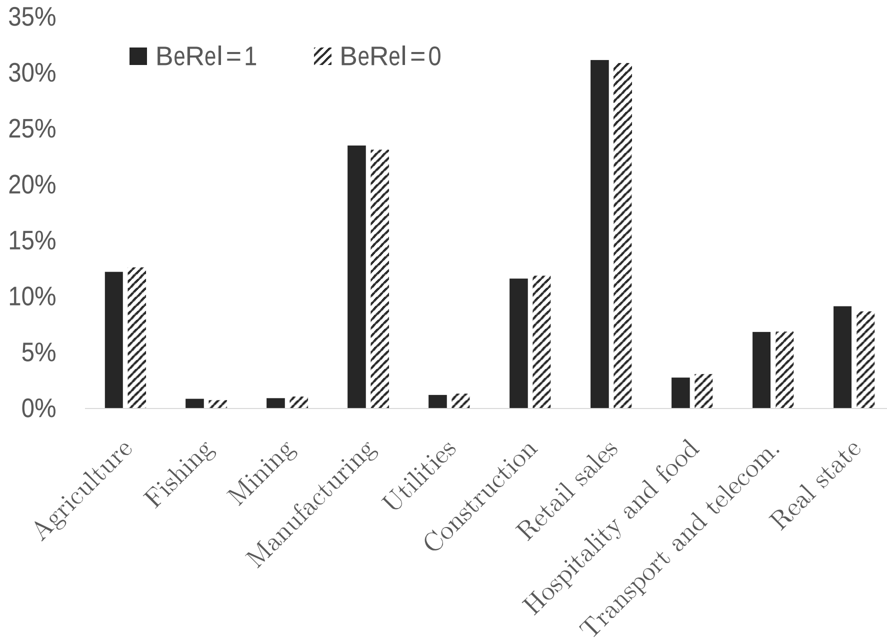
The figure above shows the evolution of new commercial loans by banks in Chile, in UFs. Following the fall of Lehman Brothers in September 2008, there was a large and sharp contraction in banking credit. Note: UF is an inflation-indexed unit of account, 1 UF $\approx$ 33 usd in Dec. 2008. Source: author's calculation based on credit registry records.

Figure 3: Commercial lending, BECH vs other banks



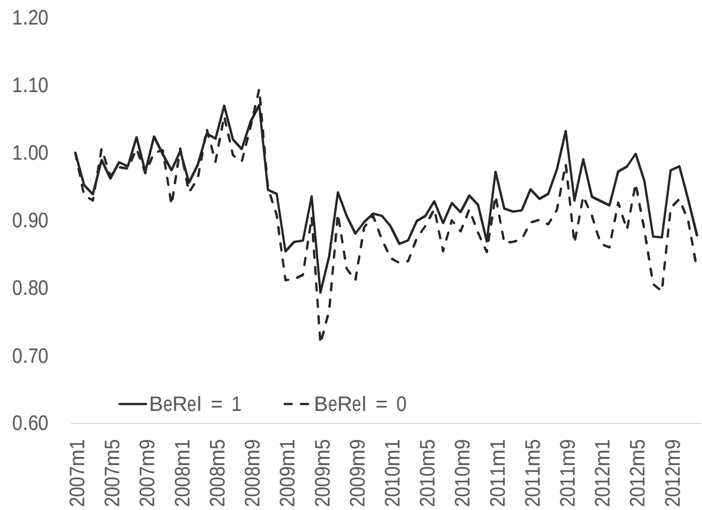
The figure above shows the evolution of the year-over-year growth rate of new commercial loans by BECH and all other banks in Chile. BECH increased lending dramatically while other banks were contracting their credit supply. Source: author's calculation based on credit registry records.

Figure 4: Distribution across industries, treated vs controls

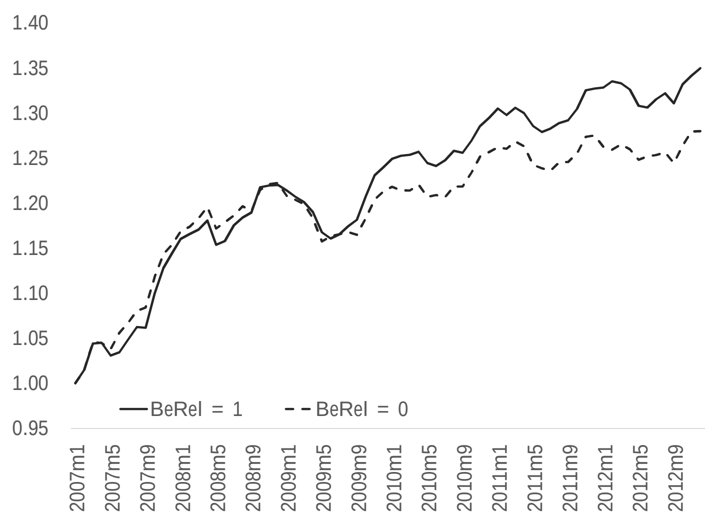


The figure above shows the distribution across industries of treated ($BeRel = 1$) and control ($BeRel = 0$) firms in the *matched sample*. Defense, Public administration, Education and Health care sectors have been dropped from the sample. The number of firms in each group is 5,109.

Figure 5: Pre and post trends at the monthly frequency



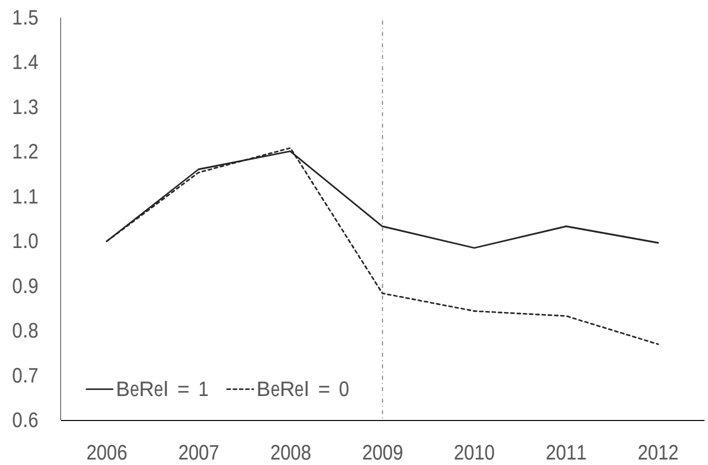
(a) Mean log bank credit



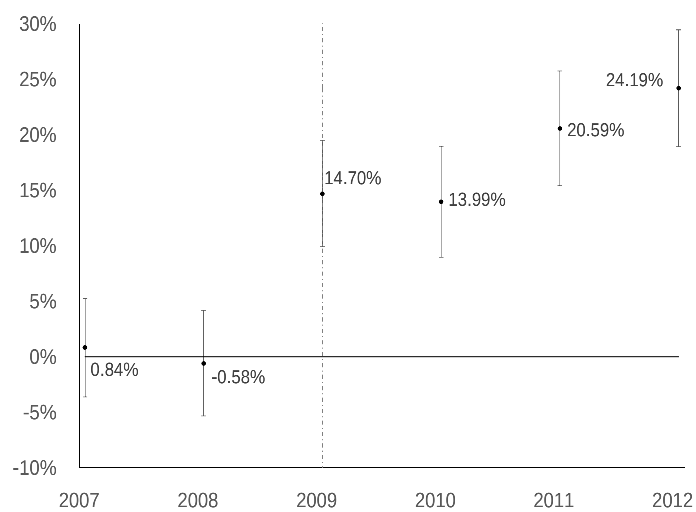
(b) Mean log number of workers

The figure above shows the evolution of new commercial loans and employment at the monthly frequency for treated ($BeRel = 1$) and control ($BeRel = 0$) firms in the matched sample. Bank credit and employment react similarly to the negative aggregate demand shock before the announcement of the capitalization of BECH in Dec. 2008, but diverge afterwards. *Note:* Graphs depict raw means. Variables are in logs and have been normalized such that Jan2007=1.

Figure 6: Firm-level effects, treated vs controls: bank credit



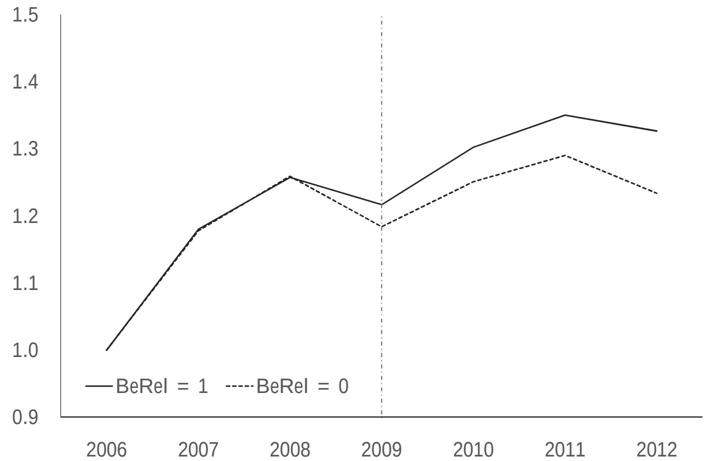
(a) Mean log level



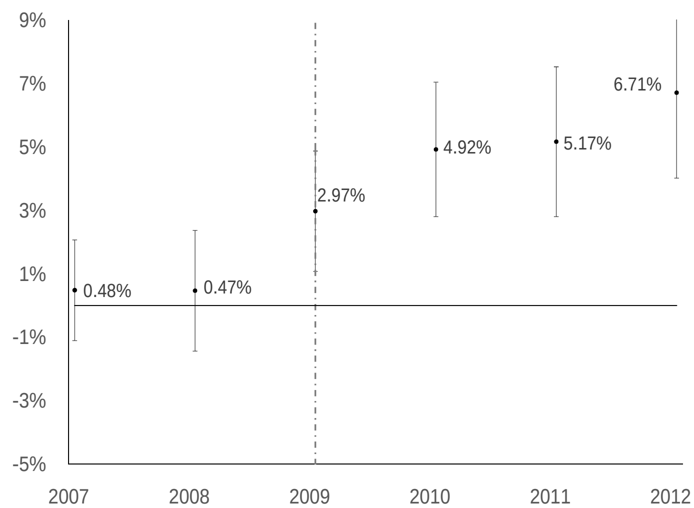
(b) Estimates

The figure above shows the impact of having a bank relationship with BECH before the policy change on total bank credit at the firm-level. Treated firms ($BeRel = 1$) had a bank relationship with BECH, while controls ($BeRel = 0$) did not. Total bank credit correspond to the sum of new commercial loans in UFs by the banking system to firm i in year t. Panel (a) depicts the evolution of the raw mean of the log of the variable, normalized such that Jan2006=1. Panel (b) shows the estimates from Equation (3). Error bars represent 5% robust confidence intervals for $H_0 : \hat{\beta}_t = 0$. *Note:* UF is an inflation-indexed unit of account, 1 UF $\approx$ 33 usd in Dec. 2008.

Figure 7: Firm-level effects, treated vs controls: sales



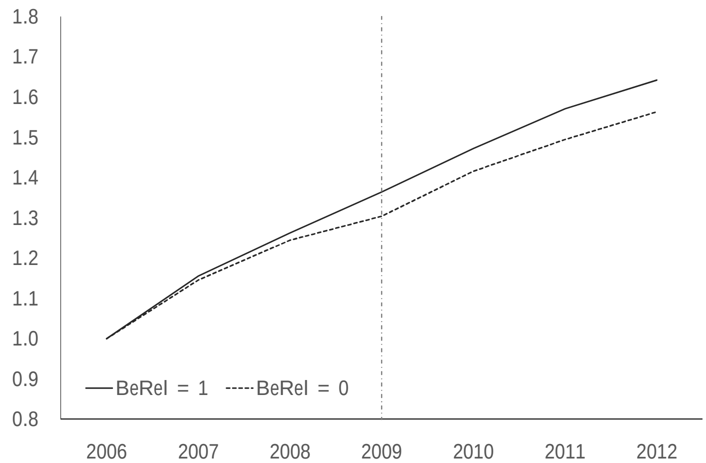
(a) Mean log level



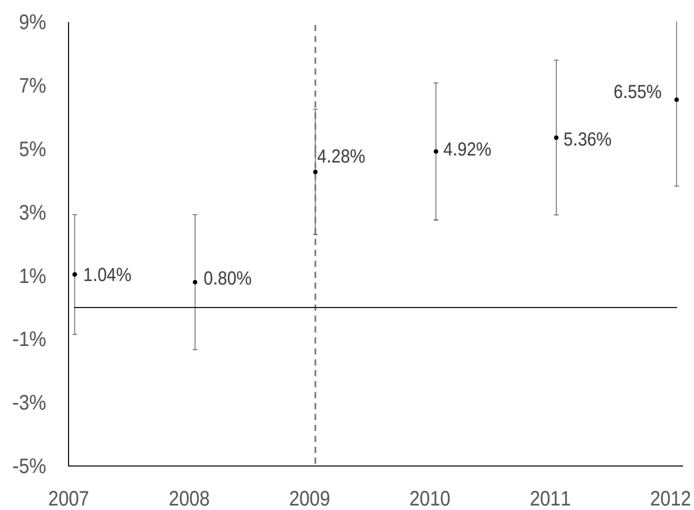
(b) Estimates

The figure above shows the impact of having a bank relationship with BECH before the policy change on sales in UFs at the firm-level. Treated firms ($BeRel = 1$) had a bank relationship with BECH, while controls ($BeRel = 0$) did not. Panel (a) depicts the evolution of the raw mean of the log of the variable, normalized such that Jan2006=1. Panel (b) shows the estimates from Equation (3). Error bars represent 5% robust confidence intervals for $H_0 : \hat{\beta}_t = 0$. *Note:* UF is an inflation-indexed unit of account, 1 UF $\approx$ 33 usd in Dec. 2008.

Figure 8: Firm-level effects, treated vs controls: capital stock



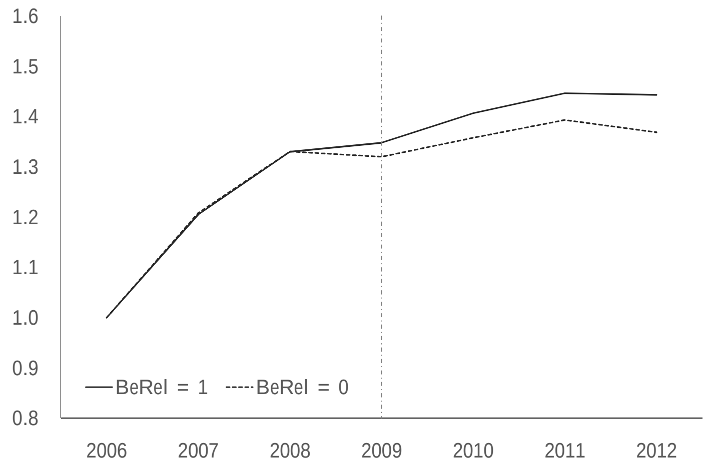
(a) Mean log level



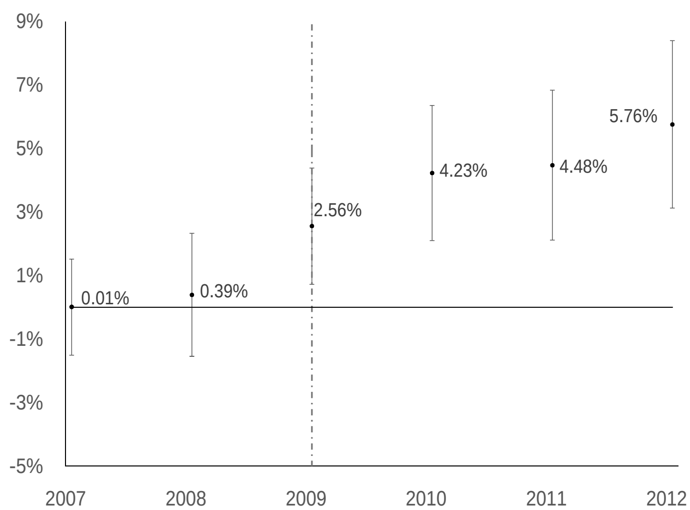
(b) Estimates

The figure above shows the impact of having a bank relationship with BECH before the policy change on total assets in UFs at the firm-level. Treated firms ($BeRel = 1$) had a bank relationship with BECH, while controls ($BeRel = 0$) did not. Total assets measured in December of year t. Panel (a) depicts the evolution of the raw mean of the log of the variable, normalized such that Jan2006=1. Panel (b) shows the estimates from Equation (3). Error bars represent 5% robust confidence intervals for $H_0 : \hat{\beta}_t = 0$. *Note:* UF is an inflation-indexed unit of account, 1 UF $\approx$ 33 usd in Dec. 2008.

Figure 9: Firm-level effects, treated vs controls: employment



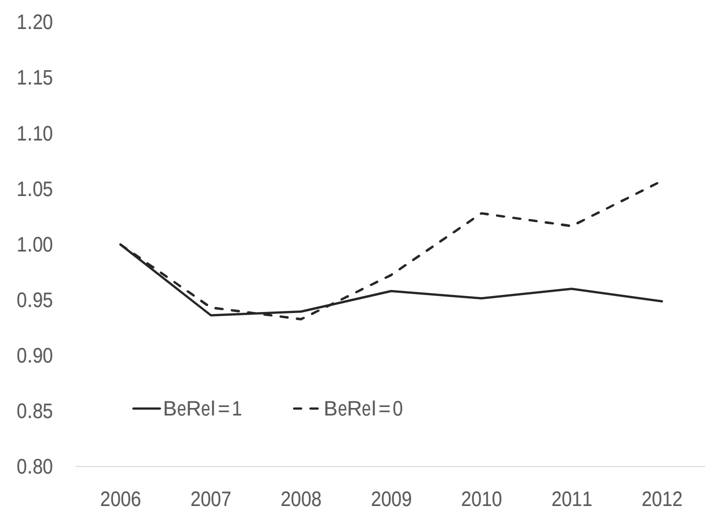
(a) Mean log level



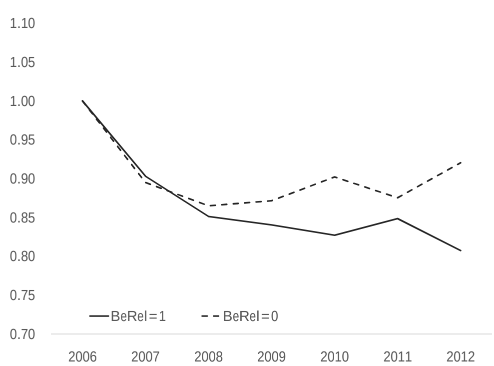
(b) Estimates

The figure above shows the impact of having a bank relationship with BECH before the policy change on the number of workers at the firm-level. Treated firms ($BeRel = 1$) had a bank relationship with BECH, while controls ($BeRel = 0$) did not. Panel (a) depicts the evolution of the raw mean of the log of the variable, normalized such that Jan2006=1. Panel (b) shows the estimates from Equation (3). Error bars represent 5% robust confidence intervals for $H_0 : \hat{\beta}_t = 0$.

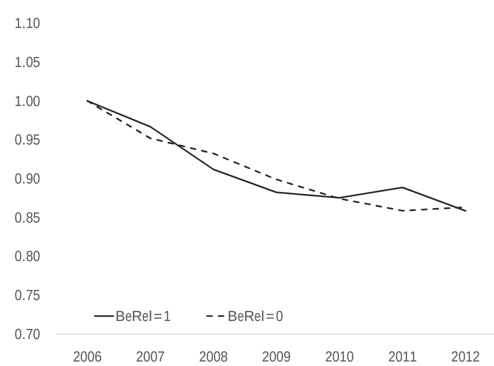
Figure 10: Marginal productivity of capital: dispersion within treated and controls



(a) Interquartile range of log MRPK



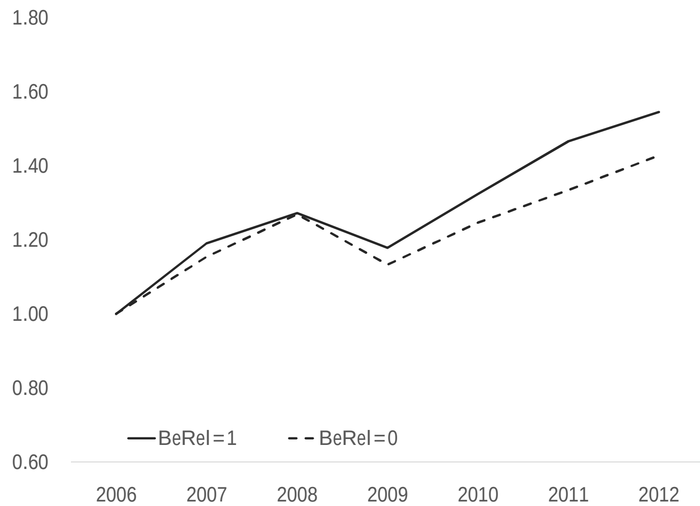
(b) 75th percentile of log MRPK



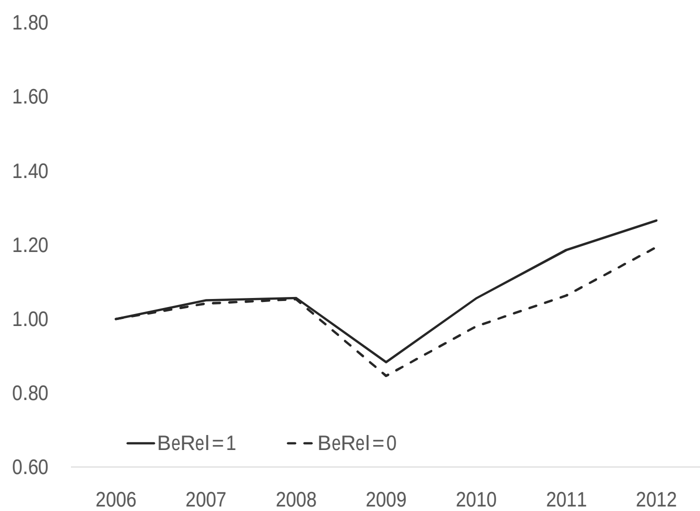
(c) 25th percentile of log MRPK

Panel (a) in the figure above shows the evolution of the interquartile range of the log marginal productivity of capital. Panels (b) and (c) show the evolution of the 75th and 25th percentiles, respectively. All statistics are normalized such that Jan2006=1.

Figure 11: Evolution of aggregated output and TFP, treated vs controls



(a) Value added



(b) TFP

The figure above shows the evolution of aggregated value added and total factor productivity for treated ($BeRel = 1$) and control ($BeRel = 0$) firms. Aggregated value added (in UFs) is calculated by subtracting total material expenses to total sales in each group of firms. TFP is obtained from Equation (13). Variables are normalized such that Jan2006=1. *Note:* UF is an inflation-indexed unit of account, 1 UF $\approx$ 33 usd in Dec. 2008.

7.2 Tables

Table 1: Summary statistics full sample, 2008

	Borrowed from BECH before 2009 ($BeRel = 1$)					
	Mean	SD	p10	p50	p90	N
Sales (UF)	675,531	8,311,347	3,343	24,161	565,571	7,191
Value added (UF)	220,844	2,650,635	1,758	11,152	202,264	7,162
Employment (#)	92	420	2	13	163	7,191
Wage bill (UF)	34,336	171,585	53	3,306	52,834	7,162
Total assets (UF)	1,378,204	20,300,000	973	17,647	718,975	7,162
Fixed assets (UF)	304,078	3,738,267	0	3,675	128,548	7,162
Bank credit (UF)	76,519	497,586	1	770	86,819	7,191
Default rate	0.004	0.047	0	0	0	7,191
Bank relationships (#)	3.7	2.5	1.0	3.0	8.0	7,191

	Did not borrow from BECH before 2009 ($BeRel = 0$)					
	Mean	SD	p10	p50	p90	N
Sales (UF)	100,034	860,079	2,285	13,720	128,307	32,936
Value added (UF)	42,613	372,785	7,127	55,124	55,124	32,771
Employment (#)	29	113	8	55	55	32,936
Wage bill (UF)	10,456	52,888	2,122	18,350	18,350	32,771
Total assets (UF)	757,340	102,000,000	10,425	150,606	150,606	32,771
Fixed assets (UF)	40,949	643,168	1,504	32,675	32,675	32,771
Bank credit (UF)	6,706	49,416	257	8,201	8,201	32,936
Default rate	0.004	0.05	0	0	0	32,936
Bank relationships (#)	1.9	1.2	2.0	3.0	3.0	32,936

This table shows summary statistics for firms in the *full sample* that borrowed from BECH before 2009 ($BeRel = 1$) and those that did not ($BeRel = 0$). Sales, value added and bank credit correspond to the total flow for 2008. Employment is the number of workers. Wage bill corresponds to total expenses in labor for 2008. Total and fixed assets are measured as the book value in Dec 2008. Default rate of firm i is the fraction of its outstanding debt with the banking system that is due more than 90 days late. Bank relationships corresponds to the number of banks that have outstanding debt with firm i. The table presents averages in 2008 for employment, default rate and bank relationships. Note: UF is an inflation-indexed unit of account, 1 UF $\approx$ 33 usd in Dec. 2008.

Table 2: Summary statistics matched sample, 2008

	Borrowed from BECH before 2009 ($BeRel = 1$)					
	Mean	SD	p10	p50	p90	N
Sales (UF)	201,501	596,676	5,135	33,848	460,722	5,109
Value added (UF)	80,708	268,259	2,314	15,448	165,926	5,109
Employment (#)	62	133	3	16	159	5,109
Wage bill (UF)	21,083	54,354	508.1	4,709	48,871	5,109
Total assets (UF)	385,614	3,020,751	3,208	27,144	591,822	5,057
Fixed assets (UF)	92,506	712,668	380.69	6,172	126,170	4,951
Bank credit (UF)	32,401	98,926	23	2,546	74,035	5,109
Default rate	0.003	0.04	0	0	0	5,109
Bank relationships (#)	3	2.59	1	3	7	5,109

	Did not borrow from BECH before 2009 ($BeRel = 0$)					
	Mean	SD	p10	p50	p90	N
Sales (UF)	192,674	753,121	5,143	35,197	375,755	5,109
Value added (UF)	78,295	306,962	2,374	15,080	148,124	5,109
Employment (#)	61	158	3	16	145	5,109
Wage bill (UF)	21,858	70,997	548	4,719	48,905	5,109
Total assets (UF)	314,099	2,037,743	3,320	30,616	464,896	5,071
Fixed assets (UF)	76,118	664,272	364	5,966	99,180	4,970
Bank credit (UF)	23,958	109,573	0	1,525	43,383	5,109
Default rate	0.003	0.04	0	0	0	5,109
Bank relationships (#)	3	2	1	2	5	5,109

This table shows summary statistics for firms in the *matched sample* that borrowed from BECH before 2009 ($BeRel = 1$) and those that did not ($BeRel = 0$). Sales, value added and bank credit correspond to the total flow for 2008. Employment is the number of workers. Wage bill corresponds to total expenses in labor for 2008. Total and fixed assets are measured as the book value in Dec 2008. Default rate of firm i is the fraction of its outstanding debt with the banking system that is due more than 90 days late. Bank relationships corresponds to the number of banks that have outstanding debt with firm i. The table presents averages in 2008 for employment, default rate and bank relationships. Note: UF is an inflation-indexed unit of account, 1 UF $\approx$ 33 usd in Dec. 2008.

Table 3: Credit supply shock at the firm-bank level in 2009

Bank credit growth rate (GrL_{ib})

	Full sample		Matched sample		
	(1)	(2)	(3)	(4)	(5)
BE_{ib}	0.2316***	0.2316***	.2158***	.2158***	.2158***
	(0.0195)	(.0199)	(.022)	(.0225)	(.0225)
Firm FE	Yes	No	Yes	No	No
Sector FE	No	Yes	No	Yes	No
Size FE	No	Yes	No	Yes	No
Young FE	No	Yes	No	Yes	No
Export FE	No	Yes	No	Yes	No
Main Bank FE	No	Yes	No	Yes	No
N	13,462	13,462	10,218	10,218	10,218
R2	0.527	0.017	0.526	0.017	0.009
F	141.77	7.54	96.08	6.04	91.79

Notes. This table shows estimates from Equation(1). All regressions are at the firm-bank level. GrL_{ib} is the symmetric growth rate of new loans from bank BECH or all other banks to firm i between 2009 and 2007. $BE_{ib} = 1$ if bank b is BECH and 0 otherwise. BE equals 1 if BECH is bank b and 0 otherwise. Sector, Size, Young, Export and Main Bank fixed effects for 2008 (before the policy change). Robust standard errors in parenthesis. *,**,*** denote significance at 10%, 5% and 1% respectively.

Table 4: Credit supply shock at the firm-bank level in 2009, heterogeneity

Bank credit growth rate (GrL_{ib})						
	Full sample			Matched sample		
	(1)	(2)	(3)	(4)	(5)	(6)
BECH	0.0402	0.27***	0.53***	-0.0061	0.25***	0.52***
	(.03)	(.0387)	(.035)	(.0426)	(.0533)	(.0886)
Size	Small	Medium	Large	Small	Medium	Large
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Young FE	Yes	Yes	Yes	Yes	Yes	Yes
Export FE	Yes	Yes	Yes	Yes	Yes	Yes
Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
N	6,438	3,370	3,654	4,364	3,020	2,834
R2	0.005	0.027	0.088	0.009	0.027	0.083
F	1.34	3.87	11.8	4.54	3.55	9.12

Notes. This table shows estimates from Equation(2). All regressions are at the firm-bank level. GrL_{ib} is the symmetric growth rate of new loans from bank BECH or all other banks to firm i between 2009 and 2007. $BE_{ib} = 1$ if bank b is BECH and 0 otherwise. BE equals 1 if BECH is bank b and 0 otherwise. Small: less than 25000 UFs in annual sales; Medium: between 25000 and 100000 UFs in annual sales; Large: more than 100000 UFs in annual sales. UF is an inflation-indexed unit of account. 1 UF $\approx$ 33 usd in Dec. 2008. Sector, Size, Young, Export and Main Bank fixed effects for 2008 (before the policy change). Robust standard errors in parenthesis. *,**,*** denote significance at 10%, 5% and 1% respectively.

Table 5: Firm-level results: bank credit

	Differential impact on bank credit					
	2007	2008	2009	2010	2011	2012
	Placebo		(1)	(2)	(3)	(4)
Panel A: Bank credit growth rate, full specification						
Be_Rel	0.008	-0.006	0.147***	0.140***	0.206***	0.242***
SE	0.023	0.024	0.024	0.025	0.026	0.027
Obs.	10218	10218	10218	10218	10218	10218
F-stat	8.510	9.730	9.800	8.060	8.350	8.880
R^2	0.029	0.032	0.028	0.021	0.024	0.025
Panel B: Bank credit growth rate, no controls						
Be_Rel	0.010	-0.006	0.149***	0.146***	0.215***	0.255***
SE	0.035	0.037	0.038	0.039	0.041	0.042
Obs.	10218	10218	10218	10218	10218	10218
F-stat	0.885	0.547	18.598	16.598	32.833	45.040
R^2	0.000	0.000	0.004	0.004	0.007	0.010

Notes. This table shows estimates from Equation(3). All regressions are at the firm level. The dependent variable is the symmetric growth rate of new commercial loans from the banking system to firm i between 2009 and 2007. $BeRel = 1$ if firm i borrowed from BECH before 2009 and 0 otherwise. Panel (A) includes Sector, Size, Young, Export and Main Bank fixed effects for 2008 (before the policy change). Robust standard errors in parenthesis. *,**,*** denote significance at 10%, 5% and 1% respectively.

Table 6: Firm-level results: output

	Differential impact: sales and value added					
	2007	2008	2009	2010	2011	2012
	Placebo		(1)	(2)	(3)	(4)
Panel A: Annual sales growth rate, full specification						
Be_Rel	0.005	0.005	0.030***	0.049***	0.052***	0.067***
SE	0.008	0.010	0.010	0.011	0.012	0.014
Obs.	10218	10218	10218	10218	10218	10218
F-stat	19.600	30.790	6.340	7.100	11.360	10.360
R^2	0.099	0.115	0.021	0.023	0.022	0.018
Panel B: Annual sales growth rate, no controls						
Be_Rel	0.006	0.003	0.029*	0.049***	0.052***	0.069***
SE	0.013	0.016	0.015	0.017	0.019	0.021
Obs.	10218	10218	10218	10218	10218	10218
F-stat	2.520	1.870	4.638	10.887	10.178	12.475
R^2	0.001	0.001	0.001	0.002	0.002	0.003

Panel C: Value added growth rate, full specification						
Be_Rel	0.004	-0.001	0.029**	0.039***	0.037***	0.046***
RSE	0.011	0.010	0.012	0.013	0.014	0.015
Obs.	10215	10215	10198	10182	10142	9998
F	14.190	5.850	4.840	4.190	5.230	6.650
R^2	0.059	0.019	0.015	0.013	0.015	0.018
Panel D: Value added growth rate, no controls						
Be_Rel	0.005	-0.005	0.027	0.039**	0.038*	0.047**
SE	0.017	0.015	0.019	0.020	0.022	0.024
Obs.	10215	10215	10198	10182	10142	9998
F-stat	1.315	2.583	3.770	6.838	5.263	5.325
R^2	0.000	0.001	0.001	0.002	0.001	0.001

Notes. This table shows estimates from Equation(3). All regressions are at the firm level. The dependent variable is the symmetric growth rate of firm's i sales—Panels (A) and (B)—and value added—Panels (C) and (D)—between 2009 and 2007. $BeRel = 1$ if firm i borrowed from BECH before 2009 and 0 otherwise. Panels (A) and (C) include Sector, Size, Young, Export and Main Bank fixed effects for 2008 (before the policy change). Robust standard errors in parenthesis. *,**,*** denote significance at 10%, 5% and 1% respectively.

Table 7: Firm-level results: capital stock

	2007	2008	2009	2010	2011	2012
	Placebo		(1)	(2)	(3)	(4)
Panel A: Fixed assets growth rate, full specification						
Be_Rel	0.001	0.003	0.042**	0.076***	0.070***	0.087***
SE	0.016	0.017	0.017	0.019	0.020	0.022
Obs.	10038	10218	10117	10106	10116	10100
F-stat	6.480	12.720	6.870	7.740	8.470	7.190
R^2	0.025	0.044	0.024	0.026	0.028	0.023
Panel B: Fixed assets growth rate, no controls						
Be_Rel	-0.000	0.000	0.043*	0.073***	0.068**	0.085**
SE	0.024	0.027	0.025	0.028	0.031	0.034
Obs.	5019	5109	5059	5053	5058	5050
F-stat	0.530	0.117	4.093	8.988	6.152	8.162
R^2	0.000	0.000	0.001	0.002	0.001	0.002

Differential impact: fixed and total assets

	2007	2008	2009	2010	2011	2012
Panel C: Total assets growth rate, full specification						
Be_Rel	0.010	0.008	0.043***	0.049***	0.054***	0.066***
SE	0.010	0.011	0.010	0.011	0.012	0.014
Obs.	10166	10218	10177	10165	10124	9973
F-stat	11.830	21.360	7.930	13.640	12.650	10.740
R^2	0.049	0.078	0.029	0.044	0.041	0.036
Panel D: Total assets growth rate, no controls						
Be_Rel	0.010	0.008	0.042***	0.049***	0.054***	0.067***
SE	0.015	0.017	0.015	0.017	0.019	0.022
Obs.	5083	5109	5089	5083	5062	4987
F-stat	3.065	2.678	9.023	11.188	10.573	11.925
R^2	0.001	0.001	0.002	0.002	0.002	0.003

Notes. This table shows estimates from Equation(3). All regressions are at the firm level. The dependent variable is the symmetric growth rate of firm's i fixed assets—Panels (A) and (B)—and total assets—Panels (C) and (D)—between 2009 and 2007. Fixed and total assets measured as the book value in Dec. of year t. $BeRel = 1$ if firm i borrowed from BECH before 2009 and 0 otherwise. Panels (A) and (C) include Sector, Size, Young, Export and Main Bank fixed effects for 2008 (before the policy change). Robust standard errors in parenthesis. *,**,*** denote significance at 10%, 5% and 1% respectively.

Table 8: Firm-level results: employment

	Differential impact: wage bill and number of workers					
	2007	2008	2009	2010	2011	2012
	Placebo		(1)	(2)	(3)	(4)
Panel A: Wage bill growth rate, full specification						
Be_Rel	0.001	0.002	0.021**	0.036***	0.039***	0.048***
SE	0.008	0.010	0.010	0.011	0.013	0.014
Obs.	10218	10218	10218	10218	10218	10218
F-stat	12.370	16.900	6.420	8.320	8.920	10.160
R^2	0.042	0.051	0.019	0.022	0.023	0.025
Panel B: Wage bill growth rate, no controls						
Be_Rel	0.002	0.003	0.022	0.039**	0.043**	0.054**
SE	0.012	0.016	0.015	0.017	0.019	0.022
Obs.	5109	5109	5109	5109	5109	5109
F-stat	0.365	0.463	2.730	6.677	6.817	8.495
R^2	0.000	0.000	0.001	0.002	0.002	0.002

Panel C: Number of workers growth rate, full specification						
Be_Rel	0.000	0.004	0.026**	0.042***	0.045***	0.058***
SE	0.008	0.010	0.009	0.011	0.012	0.013
Obs.	10218	10218	10218	10218	10218	10218
F-stat	13.340	17.840	6.290	7.900	8.260	9.110
R^2	0.046	0.055	0.021	0.023	0.023	0.026
Panel D: Number of workers growth rate, no controls						
Be_Rel	0.001	0.004	0.025*	0.044***	0.046***	0.060***
SE	0.012	0.015	0.014	0.017	0.019	0.021
Obs.	5109	5109	5109	5109	5109	5109
F-stat	0.720	0.458	3.890	8.195	7.723	9.782
R^2	0.000	0.000	0.001	0.002	0.002	0.002

Notes. This table shows estimates from Equation(3). All regressions are at the firm level. The dependent variable is the symmetric growth rate of firm's i wage bill—Panels (A) and (B)—and number of workers—Panels (C) and (D)—between 2009 and 2007. $BeRel$ = 1 if firm i borrowed from BECH before 2009 and 0 otherwise. Panels (A) and (C) include Sector, Size, Young, Export and Main Bank fixed effects for 2008 (before the policy change). Robust standard errors in parenthesis. *,**,*** denote significance at 10%, 5% and 1% respectively.

Table 9: Capital share (α_s) in 2007

	Mean	p50	SD
Agriculture	0.621	0.654	0.239
Fishing	0.684	0.718	0.235
Mining	0.710	0.744	0.222
Manufacturing	0.619	0.633	0.195
Manufacturing (metalic)	0.599	0.606	0.193
Utilities	0.756	0.812	0.204
Construction	0.581	0.592	0.231
Retail sales	0.649	0.667	0.192
Hospitality and food	0.597	0.611	0.188
Transport and telecom.	0.626	0.652	0.231
Real state	0.614	0.626	0.261

This table shows unweighted capital shares for the sample in 2007. The capital share is defined as 1 less the fraction of total labor expenses over value added.

Table 10: Decomposition of aggregated output growth, treated vs controls

	2009	2010	2011	2012
Output (Y)	7.32%	15.21%	21.16%	21.89%
Factor accumulation	3.60%	7.65%	8.88%	14.67%
TFP	3.72%	7.56%	12.28%	7.22%
Allocative efficiency (Γ)	1.69%	3.98%	4.28%	4.59%
Technical efficiency ($\bar{A}$)	2.04%	3.59%	7.99%	2.62%

This table shows results of the decomposition in Equations (17) and (16). Entries in the table correspond to the difference in the the growth rate from 2007 to year t of aggregates between treated and control firms. Physical output (Y) is not observed, therefore it is obtained from Equation (17).

7.3 Elasticity to credit

In order to obtain the short run elasticity of output, capital stock and employment variables to credit I take advantage of the natural experiment and implement a two stage least squares (2SLS) estimation. I run the following regression:

$$GrY_i = \psi^Y X_i + \beta^Y Gr\hat{Credit}_i + \epsilon_i^Y \tag{18}$$

Where $GrY_{i,2009} = \frac{Y_{i,2009}-Y_{i,2007}}{0.5*(Y_{i,2009}+Y_{i,2007})}$ is the growth rate of variable Y_i from 2007 to year 2009. Similarly, $GrCredit_{i,2009} = \frac{Credit_{i,2009}-Credit_{i,2007}}{0.5*(Credit_{i,2009}+Credit_{i,2007})}$ and $Credit_{it}$ is the sum of all commercial loans to firm i in year t by the banking system—the same dependent variable in Section 4.2. X_i is the same set of fixed effects used throughout the paper. Here $BeRel_i$ is used as an instrument for the endogenous regressor $GrCredit_i$.[39] Thus, estimates in Section 4.3 correspond to reduced form effect of credit on the dependent variable, and the estimate in section 4.2 corresponds to the first stage in the 2SLS estimation. In particular, given that both left hand side variables and $GrCredit_i$ are growth rates, β^Y has the direct interpretation of the short run elasticity of variable Y to credit. Finally, standard errors for all regressions are clustered at the firm-level.

The identification of these elasticities relies on the two key assumptions of instrumental variables settings. Firstly, that $\mathbb{E}[BeRel_i * GrCredit_i] \neq 0$, which is shown in Section 4.2. Secondly, that $\mathbb{E}[BeRel_i * \epsilon_i^Y] = 0$, which is equivalent to the assumption in Section 4.1. Table 11 shows the elasticity estimates I obtain.

Table 11: Elasticities to credit

Growth rate of:	Sales	Fixed assets	Total assets	Wage bill	N. work
OLS (Bank credit)	0.056	0.064	0.063	0.059	0.056
SE	0.004	0.007	0.004	0.004	0.004
2SLS (Bank credit = $BeRel$)	0.202	0.285	0.290	0.143	0.174
SE	0.070	0.119	0.077	0.067	0.066
N	10218	10117	10177	10218	10218
F first stage ($BeRel$)	18.6	18.6	18.6	18.6	18.6

[39] $BeRel_i = 1$ if firm i had at least one outstanding loan with BECH between January 2005 and December 2008, and 0 otherwise

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