# Titel der Arbeit:

# Three Essays on Cross-Firm Interactions

**Schriftliche Promotionsleistung zur**
**Erlangung des akademischen Grades**
**Doctor rerum politicarum**

**vorgelegt und angenommen an der**
**Fakultät für Wirtschaftswissenschaft**
**Otto von Guericke Universität Magdeburg**


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| Verfasser: | William McShane, M.Sc. |
| Geburtsdatum und -ort: | 05.08.1992, Bellingham, USA |
| Arbeit eingereicht am: | 17.08.2023 |



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| Gutachter der schriftlichen Promotionsleistung: | Prof. Reint Gropp, PhD<br>Prof. Dr. Lena Tonzer |
| Datum der Disputation: | 10.11.2023 |

# Three Essays on Cross-Firm Interactions

# Contents

# Preface

Firms do not pursue growth in isolation. A firm's growth is the outcome and driver of innumerable cross-firm interactions over channels such as product markets, knowledge spillovers, mergers and acquisition, and supply chains, among others. Understanding how the outcome or behavior of one firm impacts other firms is crucial to understanding the impact of economic policy and shocks.

For example, researchers and policy makers interested in the general equilibrium impact of a credit crunch need to understand the spillovers induced by financial constraints. Quantifying spillovers can inform researchers as to which effects to include in general equilibrium models (Huber, 2021). In Paper 1, I present evidence that in the event of a financial crisis, those firms least afflicted by financial constraints capture market share from financially constrained firms. This competitive interaction may thereby mitigate the aggregate impact of financial crises on output.

Moreover, it has long been argued that the dynamism of competitive interactions across firms is the fundamental driver of productivity growth through a continuous process of 'creative destruction' (Schumpeter, 1942). By unseating incumbent firms, innovative entrepreneurs release new resources for more productive uses. In Paper 2, I measure this business dynamism across a sample of European countries.

Finally, policy that does not directly reach one set of firms may still impact those firms if other firms within their network are impacted. For instance, in Paper 3, I present evidence that established firms respond to R&D tax credits by acquiring startups, which would otherwise be unlikely to directly benefit from these tax credits.

Accordingly, together with my co-authors, I seek to untangle cross-firm interactions empirically. The ultimate goal of these papers is to provide insights that can improve our understanding of the influence of economic policies and shocks on firm behavior and outcomes.

To provide a more detailed account of the papers, in Paper 1, I study the product market spillovers of the 08/09 credit crunch among non-financial firms. I find that firms whose product market *peers* had weaker lenders observe greater growth in output. This suggest that product market spillovers dampen the aggregate impact of a credit contraction on output.

However, I also find similar results when looking at market share, profitability, and markups. These impacts persist years after the credit crunch. Given that small, young, and private firms are the most vulnerable to a credit contraction, I argue that the product market spillovers of a credit contraction may serve to increase product market concentration.

To better understand the persistence of these results, I show that firms with credit constrained peers end up investing more than their estimated growth opportunities and realized growth would predict. This is consistent with theories of entry deterrence, which contend that incumbent firms invest in excess capacity to credibly commit to a more aggressive strategy in the event of entry.

Ultimately, I am interested in the causal impact of peer credit constraints. I resolve possible endogeneity concerns related to lender-borrower assortative matching by instrumenting for changes in aggregate corporate lending using bank exposure to the pre-crisis mortgage-backed securities market. This should be orthogonal to banks' corporate loan portfolios and has been shown in the literature to be unrelated to firm observables.

In Paper 2, together with Reint Gropp, I measure the intensity of business dynamism across a number of European countries. Business dynamism is, in part, the outcome of competition across firms. When an innovative new enterprise enters a market, it puts pressure on incumbent firms, pushing them to innovate or exit. When incumbent firms shrink or fail, its resources can be reallocated to more productive uses. The magnitude of this dynamism is reflected in how rapidly firms shrink and grow, as well as the share of firms that enter and exit the market. Interestingly, we find a relative absence of business dynamism in Germany across multiple metrics.

However, this does not appear to translate into relatively low levels of productivity growth, as Germany has relatively low productivity growth for the sample. A positive relationship between productivity growth and business dynamism is a robust finding in the literature (see for example Bravo-Biosca et al. (2016), Da-Rocha et al. (2019), and Foster et al. (2016), among others).

This suggests that productivity gains in Germany depend uniquely little on competitive interactions across firms. We speculate that differences in management practices and exposure to global exports may play an important role in explaining Germany's productivity growth without accompanying business dynamism.

In Paper 3, together with Merih Sevilir, I provide evidence that established firms respond to R&D tax credits by acquiring venture capital backed startups. As startups typically lack the income to benefit from R&D tax credits (Bankman and Gilson, 1999), they often cannot directly take advantage of these tax credits to expand. Hence, the interaction between established firms and startups through acquisitions may play a valuable role in allocating R&D capital to startups. This exemplifies how policy impacting one set of firms may spillover onto other firms.

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# Paper 1: Long-run Competitive Spillovers of the Credit Crunch

# Long-Run Competitive Spillovers of the Credit Crunch*

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This Draft: August 7, 2023

**Abstract**

Competition in the U.S. appears to have declined. One contributing factor may have been heterogeneity in the availability of credit during the financial crisis. I examine the impact of product market peer credit constraints on long-run competitive outcomes and behavior among non-financial firms. I use measures of lender exposure to the financial crisis to create a plausibly exogenous instrument for product market credit availability. I find that credit constraints of product market peers positively predict growth in sales, market share, profitability, and markups. This is consistent with the notion that firms gained at the expense of their credit constrained peers. The relationship is robust to accounting for other sources of inter-firm spillovers, namely credit access of technology network and supply chain peers. Further, I find evidence of strategic investment, i.e. the idea that firms increase investment in response to peer credit constraints to commit to deter entry mobility. This behavior may explain why temporary heterogeneity in the availability of credit appears to have resulted in a persistent redistribution of output across firms.

**JEL Classification: E22, E24, L10, G01**

**Keywords:** Credit contraction, competition, spillovers

*A previous version of this paper has been published as IWH Discussion Paper No. 10/2023. I am especially grateful to my advisor, Reint Gropp, for his persistent support and guidance. I thank attendees of IWH Doctoral Research Seminars and the Riezlern Allgäu 2020 Seminar. For helpful comments, I would also like to extend special thanks to Merih Sevilir, Felix Noth, Moritz Stiglitz, Konstantin Wagner, Michael Koetter, and Felix Bransch.

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# 1 Introduction

Across a wide spectrum of measures, competition in the United States appears to have declined. Profit shares (Barkai, 2020), markups (De Loecker, Eeckhout, and Unger, 2020), and industry concentration (Autor et al., 2020; Grullon, Larkin, and Michaely, 2019) have all risen in recent decades. Starting sometime in the 2000s, increasing price competition and growing productivity of industry leaders appears unable to explain this trend (Covarrubias, Gutiérrez, and Philippon, 2020).

In this paper, I introduce a novel explanation for the increase in industrial concentration seen in recent years: the credit crunch. The financial crisis resulted in a shock to firms' ability to finance their activities. However, there was substantial variation in the availability of credit across borrowers (Huber, 2018) - with smaller firms being the most harmed by the credit crunch (Chodorow-Reich, 2014). Differences in access to credit during the crisis appears to explain firms' growth path years after the crisis subsided (Wix, 2017). As financial constraints have been shown theoretically and empirically to drive competitive outcomes and behavior (Benoit, 1984; Bolton and Scharfstein, 1990; Chevalier and Scharfstein, 1995; Frésard, 2010), the credit crunch may have had an important impact on firms' long-run product market outcomes.

To trace the product market spillovers of credit constraints across firms, I create an index of peer credit constraints. I find that changes in product market *peers'* access to credit is a first-order determinant of sales, market share, and profitability of the focal firm. This is the case regardless of whether or not I control for equivalent measures of the focal firm's own access to credit. Depending on the specification, a one standard deviation decline in the lending of a firm's peers' banks results in a 5.9 to 7.8 percentage point increase in sales of the focal firm. This indicates that compared to firms whose peers had stronger lenders over the financial crisis, firms whose peers borrowed from weak lenders observed greater changes in sales over the post-crisis period relative to the pre-crisis period. This redistribution in output is persistent. The effect of the peers' lender shock is observable into 2016-Q4, the last quarter of the sample period.

On its own, a random distribution of credit supply shocks could be thought to have an ambiguous impact on the concentration of aggregate output and profit. However, credit constraints are not random. In reality, small, young and private firms are far more likely to become financially constrained in the event of a credit crunch (Chodorow-Reich, 2014). It then should follow that if large scale credit contractions do result in a redistribution of output and rents within product markets, in aggregate this should serve to increase concentration.

Consistent with the idea that peer credit constraints weaken competition, I find evidence that firms whose peers had weaker lenders observed a greater increase in markups. Hence, it appears that not only did the credit crunch redistribute market share, it also weakened the competition faced by benefiting firms.

The main contribution of this paper is to establish the importance of a credit crunch for long-run competitive outcomes. Starting as early as Tesler (1966), the theoretical literature has examined how financial constraints drive cross-firm strategic interaction. Benoit (1984) and Bolton and Scharfstein (1990) posit that financially unconstrained incumbents engage in price wars to deter the entry of financially constrained potential entrants. However, to the best of my knowledge, no research has connected the more recent credit crunch to wider developments in competition.

Following Chodorow-Reich (2014), I proxy for firm credit constraints using changes in the loan issuance of the firm's relationship lender over the financial crisis. I combine this proxy with the text-based network industry classification (TNIC) provided by Hoberg and Phillips (2016) to create a sales-weighted index of changes in the loan issuance of the lender of each firm's product market peers over the financial crisis. The idea is that I have a measure of the mean credit supply shock experienced by each firm's product market peers.

Lenders may specialize in lending to particular product markets. Accordingly, to ensure my results are driven by spillovers and not, for example, common lenders, I control for changes in loan issuance of the focal firm's lead lender.

To avoid endogeneity possibly related to lender-product market assortative matching, as in Chodorow-Reich (2014), I instrument for changes in loan issuance using three measures that shock bank's liquidity during the credit crunch. Namely, lender exposure to Lehman Brother's, lender exposure to the mortgage-backed securities (MBS) market, and net trading revenue. Banks' pre-crisis MBS exposure predict the volume of lenders' corporate loans during the financial crisis, but should be orthogonal to pre-crisis borrower characteristics. I instrument both at the firm and sales-weighted TNIC product market level. As firms are unlikely to influence their peers' choice of lender, the exposure of a peer's lender to the mortgage-backed securities market should only impact the focal firm via the credit availability of its peers.

The persistence of this redistribution of output and profit suggests a puzzle. Firms that lost market share should face little entry barriers and switching costs to retake their market share once credit conditions improve. As one potential explanation for this persistence, I look to the theoretical literature on entry deterrence and investigate whether peer lender exposure drives firms' strategic behavior. Dixit (1980) and Spence (1977) suggest that firms may preemptively invest in production capacity to deter entry and mobility. The central idea is that by lowering the marginal cost of production, investment credibly commits the incumbent firm to a more aggressive strategy in the event of entry, thereby deterring prospective entrants.

According to Etro (2006), when entry is endogenous to the capital decision of the firm, the leader will always find it optimal to pursue an aggressive investment strategy, regardless of whether or not the market is characterized by strategic substitutes or strategic complements. Hence, similar to Simintzi (2021), I focus on investment as an empirical measure of competitive actions as opposed to pricing or output strategies which are difficult to observe empirically and depend on whether competition is Cournot or Bertrand. While the theoretical context of entrants versus incumbents may not perfectly describe my empirical setting, the process of defending recently captured product market space from reentry of unseated peers should reasonably be approximated by a theory of entry behavior.

Consistent with the theory that firms invest to deter entry, product market peers' credit constraints appear to be associated with greater growth in investment of the focal firm during the crisis. My preferred estimate suggests that a one standard deviation in peer exposure to the credit crunch is associated with roughly a 0.86 percentage point change in investment ratios. This is equivalent to approximately 35% of the average decline in investment observed in this sample over the credit crunch. Hence, a credit crunch may offer a "first-mover" advantage to firms with credit constrained peers who then strategically invest to deter entry and capacity expansion.[1] This may then explain the persistence of the redistribution of market share and profit that I identify.

However, empirically distinguishing between strategic versus non-strategic investment is a challenge. Peer constraints may also increase the firm's expectations about its marginal productivity of capital by decreasing competition and thereby increase expected future profits (Nickell, 1996). This would predict that peer constraints would increase investment absent any strategic considerations of the firm.

Following Frésard and Valta (2016), I seek to distinguish empirically between strategic and non-strategic investment by controlling for variables which capture growth opportunities. If investment is entirely driven by non-strategic considerations, including variables such as various proxies for Tobin's q and the ex-post change in sales and profitability should result in a much smaller coefficient on peer credit crunch exposure. While I do find that measures of growth opportunities predict investment, the coefficient on peer

[1] As in Tirole (1988), I define "strategic" behavior as actions taken with consideration to its impact on product market peers.

credit crunch exposure remains statistically significant and does not decline in magnitude. This provides at least suggestive evidence that firms indeed strategically invest to deter entry.

Additionally, my results contribute to the growing empirical literature examining spillovers of financial constraints across firms. Direct estimation of spillovers from large scale shocks can inform macroeconomic models as to which general equilibrium effect should be included (Huber, 2021).

An important question for understanding the aggregate impact of a credit crunch is whether or not the product market peers of credit constrained firms pick up the slack or are afflicted by agglomeration spillovers. Chevalier and Scharfstein (1995) and Frésard (2010) find that firms with more liquidity relative to industry peers gain market share, suggesting that financially unconstrained firms pursue aggressive product market strategies to capture market share. Using a sample of German firms, Sonderhaus (2019) finds a reduction in employment and investment among firms whose county-industry peers' lenders benefited more from unconventional monetary policy, suggesting competitive spillovers. Huber (2018) and Berg, Reisinger, and Streitz (2021) find that firms operating in the same county as borrowers facing a lending cut saw a decline in employment and sales. They interpret this as evidence of agglomeration spillovers related to reduced local demand.

These differing set of results suggests that whether competitive or agglomeration effects will dominate following a credit shock depends on which relevant peers are examined. My sample consists of large, publicly-listed U.S. firms. Understanding how spillovers propagate across publicly-listed firms is of particular importance given their large role in the US economy: Publicly-listed firms' value-added represents roughly one quarter of GDP and their share in total employment is nearly one third as of 2019 (Schlingemann and Stulz, 2022). As publicly-listed firms compete on a national, if not global level, it is intuitive that competitive spillovers dominate any possible agglomeration spillovers in this sample.

This paper also contributes to the empirical literature examining the impact of the financial crisis and the great recession on *long-run* firm outcomes. It is now well established that bank finance during the crisis mattered in the short-run for firm employment and output (Cingano, Manaresi, and Sette, 2016; Huber, 2018), there is growing evidence that heterogeneity in the supply of credit can have persistent effects on output and employment. Chodorow-Reich (2014) documents that employment losses from financial frictions had not dissipated at all after two years and concludes that future research should seek to explain this persistence.

One notable paper in this area is Wix (2017), who observes that firms exposed to rollover risk during the credit crunch end up on persistently lower output trajectories and points to wage rigidities as a reinforcing factor. Joseph, Kneer, and van Horen (2021) find that SMEs with greater pre-crisis cash holdings relative to industry peers are considerably more profitable and have greater market share than cash-poor industry peers years after the crisis. This provides suggestive evidence that financially unconstrained firms enjoy long-run gains at the expense of their constrained peers.

I propose that the reallocation of market share and strategic behavior along credit constraints may explain some of the persistence in output and employment losses at the microeconomic level. Intuitively, if a credit constrained firm loses market share to a product market peer, it is unclear whether or not that firm will be able regain their market share once they are no longer constrained. I provide evidence that this reallocation of market share is persistent.

Consequently, this papers suggests that there is a trade off to the reallocation of output associated with the recovery of a credit crunch. However, absent the reallocation of market share from credit constrained firms, aggregate economic recovery would hinge solely on the ability of constrained firms to resume operations to pre-recession levels. The reallocation of market share should accelerate the recovery by circumventing many of the frictions associated with being credit constrained.

Hence, at the macroeconomic level, the welfare impacts of this reallocation is ambiguous. Policy makers should thus be cautious to interpret the increase in profitability and market share along peer credit constraints as warranting antitrust action.

In the following section, I describe the data and empirical setting. Section 3 provides descriptive statistics and empirical results. Section 4 includes a battery of robustness test. Section 5 provides a brief discussion and conclusion.

## 2 Data and Empirical Specification

I use the Text-Based Network Industry Classifications (TNIC) (Hoberg and Phillips, 2010, 2016) to identify firms' product market peers. This database is based on text-based analysis of product descriptions available in annual 10-K reports of publicly-listed firms and assigns similarity scores of product descriptions ranging from 0 to 1 to firm-by-firm pairs for each year. Specifically, I use the TNIC-3 product market classification which defines product markets to be as granular as the SIC 3-digit industry classification such that only firms with a minimum similarity score threshold are considered to be in the same product market.

Compared to traditional classifications of product markets such as NAICS and SIC classifications, this classification has the advantages that it is updated annually. This means that firms are assigned product markets each year rather than at the inception of the firm or the classification system. Most importantly for my purposes, it is non-transitive, meaning that if a firm shares product market space with firm A and firm B, this does not imply that firm A and B share product market space. Accordingly, each firm has its own unique set of product market peers. This is especially useful for capturing the relevant product market peers of conglomerates. Hoberg and Phillips (2016) show that the TNIC better explain product market characteristics such as profitability, sales growth, and risk relative to the NAICS and SIC.

I proxy for firms' credit availability using changes in their relationship lender's percent change in loan issuance over the financial crisis, specifically over October 2005 to June 2007 relative to October 2008 to June 2009. Insofar as the cost of switching lenders is high (Sharpe, 1990), firms with relationships with liquidity constrained lenders should face an increase in borrowing costs. My sample of lenders consists of Chodorow-Reich's (2014) data-set of the most active lead lenders in the syndicated loan market.[2] I infer a firm's relationship lender as the lead arranger of the firm's last syndicated loan in Thomson Reuter's LPC Dealscan database prior September 2008.[3] Hence, each firm receives a single value for the change in lending of their relationship lender which serves as a proxy for the credit availability of the firm. I refer to this measure as "lender health."

[2] Chodorow-Reich and Falato (2022) show that this sample of 43 lenders captures over 90% of the loan volume of covenant-specified loans in the Shared National Credit Program dataset (the universe of syndicated loans) and that the sample of loans provided by these lenders are almost identical along observables to those of the whole dataset.

[3] In some instances, syndicated loans involve multiple lead arrangers. In order to bring lender characteristics, e.g. changes in total loan issuance, to the firm-level, I weigh the lender characteristics by the credit share of each lead arranger. Similar to Chodorow-Reich's (2014), in cases where credit shares are missing, I impute credit shares based on loans with the same arranger-participant lender structures.

Using the lender to infer a borrower's credit constraints, as opposed to firm balance sheet data, has two advantages. First, compared to a peer's lender choice, the balance sheet health of a firm's peer is plausibly endogenous to the product market outcomes of the firm. Balance sheet measures such as profitability and cash reserves have been repeatedly demonstrated in the literature to cluster along product markets (Bates, Kahle, and Stulz, 2009; Hoberg and Phillips, 2016). Moreover, an aggressive competitor may impact the sales and profitability of its product market peers (Benoit, 1984), but it is less obvious how it would drive a peer's choice of lender. Second, balance sheet outcomes may reflect an endogenous response to credit constraints. Both Kahle and Stulz (2013) and Kim (2021) find that firms raise liquidity in response to negative lender shocks. Kim (2021) provides evidence that this is the outcome of fire sales to increase cash flow in response to credit constraints.

I obtain balance sheet data on public US firms from Compustat. I link Compustat with Dealscan using the gvkey link provided by Chava and Roberts (2008). I then combine the index of peer groups and lender health. I measure product market credit constraints as the sales-weighted mean lender health of product market peers. Product market peers are defined using the aforementioned TNIC-3 product market classification as of 2007. The idea is that I have a proxy for the average credit availability of the focal firm's product market peers. Moreover, I exclude the focal firm from its own measure of product market health, i.e. the measure is a 'leave-out mean.'

Firms presumably have little influence over which lender their product market peers borrow from. Hence, the availability of credit to a firm's peers over the financial crisis is arguably exogenous to the focal firm.

The most important identification problem here is omitted-variable bias from the correlation between changes in lending of the product market's lenders and other product market characteristics, such as the unobservable risk of firms in the product market. For example, a bank may reduce its corporate lending if its lending was concentrated in product markets that were particularly susceptible to an economic downturn. However, group-level risk should be positively correlated with firm-level risk. This implies that measures correlated with negative group-level outcomes should predict worse outcomes for each member of the group in the absence of competitive spillovers. Hence, estimates of the impact of changes in lending of product market peers' lenders is most plausibly biased against finding competitive spillovers.

Still, to ensure that my results are not driven by assortative matching along lender-product market characteristics, I use three measures of lender exposure to the financial crisis from Chodorow-Reich (2014) to instrument for changes in loan issuance. The first indicator of lender health measures the bank's exposure to Lehman Brothers as the share of the lender's syndicated loans in which Lehman Brothers was the lead lender. Ivashina and Scharfstein (2010) argue that banks with loans co-syndicated with Lehman lost liquidity following the collapse of Lehman as these banks had to meet commitments that would have been met by Lehman when firms drew down their already existing credit lines. The second indicator measures exposure to mortgage-backed securities inferred by the correlation of the bank's daily stock return with the return of the ABX AAA 2006-H1 index over Q4-2007. This index tracks the price of AAA rated mortgage-backed securities issued over the last two quarters of 2005. This correlation should indicate the degree to which the market perceives the bank as exposed to toxic mortgage-backed securities. The third measure captures asset write downs using the 2007-08 trading revenue as a share of total assets, following from the fact that most write down occurred in trading accounts. Arguably, all three measures of lender exposure to the crisis are unrelated to the lender's corporate loan portfolio and should therefore be exogenous to firm characteristics.

Following Chodorow-Reich and Falato (2022), I extract the first principal component of all three measures to create a rank-normalized lender exposure indicator in which the first principal component rank is divided by the total number of lenders. Hence, the worst exposed lender has a value of 1 and the least exposed lender has a value of 0. Chodorow-Reich and Falato (2022) confirm that this measure is unrelated to pre-crisis borrower observables such as borrower leverage, size, and risk rating, but do explain cross-sectional variation in firms' access to credit during the crisis.

In Figure 1, I plot percent changes in the annualized number of new loans over October 2005 to June 2007 relative to October 2008 to June 2009 along the rank-normalized change in lending of each bank. Intuitively, one observes a negative relationship between the ranked measure of bank exposure to the mortgage-backed securities market and a decline in new lending over the crisis period relative to the pre-crisis period.

This proxy for lender MBS market exposure is then weighted by product market peers' sales and used to instrument changes in the sales-weighted lending of product market peers' banks. Should banks specialize in lending to particular product markets, any instrument for product market lender health will be correlated with the lender health of the focal firm if they share common lenders or if changes in lending cluster along product markets. I address this potential violation of the exclusion restriction by also treating the focal firm's lender health as endogenous and including the exposure to the MBS market of the lender to the focal firm as an instrument for the focal firm's lender health.

My approach of instrumenting for both the direct effect and peer effect follows that outlined by Huber (2021). Using simulations, he demonstrates that this approach resolves bias related to multiple spill over types and measurement error as long as the individual-level instrument predicts individual treatment, but not group-level treatment. I confirm in the next section that the instrument for the loan issuance of the focal firm's lender is indeed uncorrelated with variation in the instrument of the the loan issuance of the lenders to the focal firm's peers.

Figure 2 compares the evolution in the mean log change in firm investment ratios relative to 2008-Q2 at the lowest and highest quartiles of lender loan issuance. I observe that firms which borrowed from lenders that saw a greater decline in loan issuance had a lower investment growth over the credit crunch relative to firms which borrowed from lenders which reduced lending less. These differences in investment ease potential concerns that variation in lender health may be irrelevant for the competitive strength of the large, publicly-listed firms that populate the Dealscan-Compustat universe.

Corroborating this interpretation, in a sample of publicly-listed firms borrowing in the US syndicated loans market, Wix (2017) finds that firms which had to refinance during the credit crunch saw a temporary gap in investment ratios compared to firms who did not need to refinance. He finds that this temporary gap in investment appears to have resulted in a persistent gap in growth trajectories.

My final data set consists of the combination of the Thomson Reuter's LPC Dealscan database, quarterly data on firms' balance sheets and income statements from Compustat's North America Fundamentals Quarterly database, Chodorow-Reich's database on lender health, and the TNIC-3 product market definition database. Depending on the specification used, the sample consists of 1,217 to 1,491 firms. I define each variable in Table 1 and winsorize continuous variables at the 1% level.

The main regression specification is as follows:

$$\Delta Y_i = \beta_0 + \beta_1 \, \Delta Market\bar{L}_i + \beta_2 \Delta L_i + \boldsymbol{\beta X_i} + \sigma_i + \epsilon_i \tag{1}$$

where $\Delta Y_i$ is defined as the log change in dependent variables of post-crisis (2010-Q2:2016-Q4) over pre-crisis (2006-Q4:2008-Q2) period means. $\Delta L_i$ is the percentage change in the annualized number of loans made by firm $i$'s lender between the periods October 2005 to June 2007 and October 2008 to June 2009. $\Delta$Market $\bar{L}_i$ , the central variable of interest, is firm $i$'s TNIC-3 product market peers' sales-weighted leave-out mean of the equivalent measure. For interpretability, in all regressions, I standardize $\Delta L_i$ and $\Delta$Market $\bar{L}_i$ to have a mean of zero and a standard deviation of one.

Additionally, $\boldsymbol{X_i}$ is a battery of controls which consists of the log of total assets of firm $i$, the net leverage of firm $i$, the sales-weighted mean of net leverage of firm $i$'s competitors, and the natural log of the total number of product market peers. I provide precise variable definitions in Table 1. All control variables are as of the last quarter of the pre-crisis period (2008-Q2).

I also control for whether or not the firm is bank dependent,' which I define as not having access to bond markets. Similar to Schwert (2018), I infer firms as having access to bond markets if they have any rated debt in the S&P Credit Rating database prior June 2008.

The variable $\sigma_i$ captures SIC single-digit sector fixed effects. While the independent variable of interest is essentially a product market effect, ideally one would compare firms in similar product markets that differ only with respect to their peer's exposure to the credit crunch. Hence, in a number of specifications, I control for the overall sector to capture the variation related to product offerings without subsuming all variation in my more granular TNIC 3-digit product market measure.

By controlling for the firm's own lender health and for the balance sheet characteristics of firm $i$ and its competitors, I seek to address any possible cluster of bank health along variation in firm financials or product markets. Hence, I am interested not in firms' financial constraints, but rather spillovers from plausibly exogenous variation in the degree of constraints of its product market peers. The main coefficient of interest is thereby $\beta_1$.

$\Delta Lending_i$ is instrumented by the previously described index of lender exposure to the MBS market. $\Delta$Market $\bar{L}_i$ is then instrumented by the sales-weighted leave-out mean of the same index across TNIC-3 product market peers of firm *i*.

Table 2 Panel A provides summary statistics with all control and outcome variables as of the last pre-crisis observation, 2008-Q2. The average firm in my sample is large, with roughly $1.28 billion in sales and $5.69 billion in assets. However, size is highly right-skewed: the mean of sales and assets is above the 75th percentile. As of 2008-Q2, the average firm is profitable in my sample. The mean ROA, measured as operating income before depreciation and amortization over the previous quarter's assets is 4%.

Net leverage, i.e. debt minus cash scaled by assets, is positive for the majority of firms in my sample, with a mean of 0.15. This indicates that most firms would not be able to use to repay total debt with liquid assets. This observation is in line with Kahle and Stulz (2017) who observe that net leverage ratios were unusually high in 2008 and that large firms tend to have positive net leverage ratios.

Note that, similar to Frésard (2010), market share is defined as sales relative to the mean sales of the firm's 2007 product market peers. I use this definition for three reasons. First, fixing the set of relevant peers to a given year reduces measurement error. The TNIC defines product market proximity as a continuous variable ranging from 0 to 1. To define a set of relevant peers, the TNIC-3 applies a cut off to the proximity score such that each firm-firm pair is as likely to be product market peers as in the SIC 3-digit classification. Hence, for firm pairs close to the cut off, small changes in the product space proximity of a pair can introduce entrance into or exit from a product market. If a product market is small and a peer is large, this can result in large measured changes in market shares. Second, including the focal firm's sales in the denominator would introduce attenuation bias. Third, taking the peer average avoids a scenario where most of the variation in market share is driven by the number of peers that leave the sample - e.g. due to acquisitions or delistings. So while the level of market share of a firm may exceed one by this measure, the relevant development is how a firm's sales develop relative to its product market peers.

I observe that the median firm has 14 competitors and sales equivalent to 38% that of the sum of their TNIC 3-digit peers, although there is a long right tail with respect to sales and thereby market share.

I recover firm markups by estimating production functions as in De Loecker, Eeckhout, and Unger (2020) using standard assumptions of the proxy variable literature. Similar to the markup estimation procedure of De Loecker and Warzynski (2012), this procedure has the advantage that it does not rely on assumptions about the nature of competition nor firm-level price data to capture market power. This procedure is described in more detail in Appendix A2.

I find that the average firm in my sample has a mark up of 1.71 as of 2008-Q2, which is higher than that of the mean found by De Loecker, Eeckhout, and Unger (2020) for the same year using the entire Compustat sample. This is perhaps driven by the fact that all firms in my sample are active borrowers in the syndicated loan market and hence larger than the average Compustat firm. However, for my purposes and that of De Loecker, Eeckhout, and Unger (2020), changes in mark ups are of more interest than the level. I find that the average markup declines by 0.21. This need not contradict the thesis put forth by De Loecker, Eeckhout, and Unger (2020) that market power has increased, who find that the within firm change in existing firms only plays a small role in the rise in markups, with most of the change attributable to high markup firms capturing market share.

I observe that the median firm's bank saw a decline in lending volume between the periods October 2005 to June 2007 and October 2008 to June 2009 of approximately 54%. Intuitively, lender health variable of the focal firm shows more dispersion than the sales-weighted mean of product market lender health, as the latter is averaged out along product markets.

# 3 Results

## 3.1 First Stage

I begin by testing the relevance of my proxy of bank's exposure to the financial crisis for bank lending. The proxy is the first principal component of three measures: (1) lender exposure to Lehman Brother's, (2) lender exposure to the MBS market, and (3) net trading revenue as a share of total assets. To instrument for sales-weighted changes in lender to the firm's product market peers, I take the sales-weighted MBS market exposure proxy of the peers. Importantly, a firm's peers' lenders exposure to the financial crisis should be even further removed from any endogenous characteristics of the focal firm.

In Column 1 of Table 3, I find that the MBS exposure of the firm's peers is a stronger predictor of changes in the lending of the peers' lenders. The corresponding F-statistic is 428.97. Including controls in column 2, the corresponding F-statistic on MBS exposure remains significant at 374.17. Column 2 demonstrates that the relationship between changes in lending to the product market peers and the exposure of the focal firm's lender is small and statistically indistinguishable from zero.

Finally, I instrument for changes in loan issuance of the focal firm's lender. The F-statistic is again significant with a value of 175.54. The MBS exposure of the peers' lenders is statistically unrelated to changes in loan issuance of the focal firm's lender. Together, the first stage results are intuitive and speak strongly to the relevance of the instruments for the regressors of interest.

It is reassuring that the instrument of lender MBS predicts lending of the firm's lender, but does not predict that of its product market peers. Similarly the sales-weighted mean of the product market's lenders MBS exposure does not predict lending of the focal firm's lender. In simulations performed by Huber (2021), assuming relevance and exogeneity of the instruments, as long as the instrument predicts individual treatment, but not that of the group, and vice-versa, then the coefficient on the spillover should not be confounded by bias related to multiple spillover sources and measurement error.

## 3.2 Outcomes: Sales, Market Share, and Profitability

A glance at the evolution of firm sales along upper and bottom quartiles of loan issuance of product market peers' lenders provides evidence in favor of the hypothesis that firms gained in the long-run from having credit constrained peers. Figure 3 presents the unadjusted mean log changes in sales relative to 2008-Q2 over time for firms with values of $\Delta$Market $\bar{L}$ below the bottom quartile and above the bottom quartile. One sees visibly different long-run developments in firms' sales growth based on the credit crunch exposure of their peers alone. Even through 2018, there is no sign of this difference abating.

Moving into the empirical results for sales and market share, Table 4 presents results for log percentage changes over the post-(2010-Q2:2016-Q4) to pre-crisis (2006Q1:2008-Q2) periods in sales. The first column presents simple bivariate OLS results of changes in sales regressed on the sales-weighted average change in loan issuance of a firm's product market peers' lenders. The coefficient indicates that firms with more credit constrained peers observe greater long-term sales growth. As $\Delta$Market $\bar{L}$ is standardized, the coefficient can be interpreted as indicating that a one standard deviation difference in $\Delta$Market $\bar{L}$ is associated with a 3.27 percentage point change in sales over the post-crisis relative to the pre-crisis period. Column 2 demonstrates that adding control variables, such as changes in the focal in the loan issuance of the focal firm's lender, serves to increase the estimated magnitude of the coefficient on $\Delta$Market $\bar{L}$.

Columns 3 to 5 of Table 4 present second stage results from two-stage least squares (2SLS) specifications. All three columns instrument for changes in $\Delta$Market $\bar{L}$ using the sales-weighted mean of the product market's lenders MBS exposure. Columns 4 and 5 additionally instrument for ΔL, the direct effect of credit constraints, using lender MBS exposure. Finally, Column 5 also controls for SIC-1 digit sector effects. Depending on covariates included in the model, Columns 3 through 5 indicate that a one standard deviation change in the availability of credit to a firm's peers drives a 11.1% to 14.7% of a standard deviation change in sales.

I find that the coefficient on credit constraints spillovers is greater in the 2SLS least squares setting relative to equivalent OLS estimates. This could be interpreted as suggesting that OLS estimates are downward biased by negative assortative matching of peer lender health and the focal firm characteristics. However, given that the $R^2$ of the OLS regression in Column 1 is higher than the $R^2$ of the equivalent 2SLS regression in Column 3, it seems at least as plausible that OLS may simply be using more variation in $\Delta$Market $\bar{L}$, which results in estimating a lower coefficient.

Table 5 presents the results of our model regressed on percentage change in market share, the level of which is measured as firm sales divided by mean sales of the firm's TNIC-3 product market peers. The OLS (Columns 1 through 2) and 2SLS (Columns 3 through 5) results indicate that changes in the availability of credit to a firm's product market peers positively predict growth in the focal firm's market share.

The coefficients on $\Delta$Market $\bar{L}$ across the specifications in Table 4 conform to a similar pattern as that of Table 5. The coefficient is greater in magnitude with covariates than without. Also, the 2SLS results are greater in magnitude than the OLS results.

The coefficient of the spillover effect in these models is economically significant. For example, in the most saturated 2SLS version of the model (Column 5), one standard deviation change in peer credit availability is associated with a 4.74 percentage point change in market share over the post- to pre-crisis period. This is equivalent to 32.46% of a standard deviation of the variable.

Moving to ROA as a proxy for profitability in Table 6, I find that larger declines in the availability of credit to a firm's product market peers is positively associated with changes in the focal firm's ROA. This spillover effect is statistically significant at the 1% level in every specification. The most saturated 2SLS model indicates that a one standard deviation decline in peers' credit availability induces a 0.59 percentage point greater change in ROA. This is equivalent to 19.5% of a standard deviation in the change in ROA over the pre- to post-crisis period.

Together, the results observed in tables 4 through 6 lend strong support for the hypothesis that firms benefited from the credit constraints of their product market peers. These results speak against the credit crunch as primarily being a negative inter-regional product market shock due to agglomeration effects such as, for example, up-stream supply chain shocks and R&D spillovers.

That I find that firms appear to benefit from their peers' being constrained eases concerns that unobserved factors which drive systematic variation in product market exposure to weak lenders also drive firm outcomes. If weaker banks are more likely to lend to product markets with low growth potential then that should generate a positive correlation between measures of peers' health and focal firm outcomes.

One potential concern is related to estimates of the direct effect of credit constraints compared to that of the spillover effect of credit constraints of the firm's peers. Intuitively, I consistently find greater declines in the loan issuance of the focal firm's lender is associated with lower sales, market share, and profitability growth. However, this direct effect of credit constraints is statistically insignificant in most specifications and in all but one specification, the implied importance of the direct effect for explaining variation in the dependent variable is smaller than that of the spillover effect.

What may seem like a contradiction at first glance is likely the result of attenuation bias driven by measurement error. As discussed by Angrist (2014), results in which empirical estimates of peer effects exceed direct effects are commonplace in the peer effects literature. In this setting, changes in the total lending of a bank with which a firm has a borrowing-relationship does not perfectly measure the extent to which firms are credit constrained, in particular among the publicly-listed firms that populate this sample. This measurement error biases the coefficient toward zero.

When aggregating this measure at the product market level, much of this measurement error is averaged-out, converging to its mean value of zero the more firms are in the product market. This mitigates attenuation bias in the spillover effect, which may explain why the spillover effect is statistically significant, while that of the direct effect is not.

These results should not be taken to imply that there is no direct impact of a credit shock on measures of firm performance. To the contrary, my results are consistent with that of (Chodorow-Reich, 2014), who finds that large publicly-listed firms were less impacted by the credit contraction. However, the contribution of this paper is to document the presence of product market spillovers of a credit contraction.

Note that the measurement error in the product market effect could bias the spillover estimate if there is a common component for members of the same TNIC-3 product market that determines each firm's respective $\Delta$L. Such a common component is plausible, as banks are likely to specialize in lending to specific product markets. The reason $\Delta$Market $\bar{L}$ may be biased by this common component is that by containing less measurement error than $\Delta$L, it has a higher loading of the common component in relative terms.

However, as argued by Huber (2021), the direction of the spillover estimate's bias should follow the coefficient of the direct effect. The coefficients on $\Delta$L and $\Delta$Market $\bar{L}$ have the opposite sign. Firms with peers subject to a greater credit shock do better and firms subject to a greater credit shock do worse. Hence, should measurement error induce a bias in estimates of the product market spillover, it would be biased toward zero relative to the estimates presented in this paper.

## 3.3 Outcome: Markups

That firms with credit constrained peers enjoy greater increases in sales, market share, and profitability suggests a redistribution of activity within product markets following a credit shock. This redistribution should serve to dampen the immediate economic harm of a credit crunch in aggregate. At first glance, this appears unambiguously welfare enhancing.

Still, credit shock spillovers could increase product market concentration and thereby weaken competition. Considering that large incumbents are less susceptible to becoming credit constrained than small entrants, one would expect the existence of credit shock spillovers through product markets to increase concentration on aggregate.

Still, on their own, these results do not demonstrate that credit constrained peers reduce competition. To assess the impact on competition faced by the focal firm, I examine changes in markups in Table 7.

The sample size is moderately reduced relative to previous specifications due to reduced coverage of the variables needed to estimate markups. The coefficient on $\Delta$Market $\bar{L}$ is consistently positive across the OLS and 2SLS specifications. However, it is only statistically significant at the 10% level or above with the inclusion of control variables and is insignificant on its own. The magnitude of the coefficient in the most saturated model appears however economically significant. A one standard deviation increase in $\Delta$Market $\bar{L}$ drives a change in markups equivalent to 7.69% of a standard deviation.

This provides at least some evidence that firms with credit constrained peers face reduced competition. Hence, it seems that not only did the credit crunch result in a redistribution of output and profitability, its spillovers may have also allowed some firms to extract rents.

Moreover, I find evidence that firms with better access to credit saw greater increases in markups. Specifically, the coefficient on $\Delta L$ is positive and statistically significant at the 10% level or 5% level across specifications in which it is included. Its economic magnitude also exceeds that of $\Delta$Market $\bar{L}$. Column 5 of Table 7 suggests that firms with a one standard deviation better access to credit saw an increase of markups equivalent to 14.8% of a standard deviation.

This result is consistent with that of Kim (2021), who, using an identification strategy similar to that of this paper, finds that firms subject to a credit shock reduced prices to liquidate inventory and generate more cash flow. It is also reassuring for the validity of the markup estimation strategy that markup estimates and credit constraints appear to follow a dynamic similar to that of prices and credit constraints.

## 3.4 Outcome: Investment

One could expect firms to return to their previous market shares following the credit crunch. However, my results suggest a persistent reallocation of output and rents related to peer credit constraints. Why does this impact appear to result in a persistent redistribution of output?

One potential explanation is labor market rigidities. Wix (2017) finds that firms facing more rigid wages during the Great Recession grew more slowly. Presumably the cost of firing and then rehiring would encumber firms' capacity to recapture market share once demand resumes. Similarly, switching costs among customer bases may result in a more persistent redistribution of output.

In this paper, I focus on one potential explanation for the persistence reallocation. Namely that firms which benefited from this redistribution engaged in behaviors which disincentived aggressive competition from their potential peers. I posit that firms facing credit constrained peers gained an incumbency or first-mover advantage: a temporary state in which their peers had little ability to compete on prices (Chevalier and Scharfstein, 1995), output, or other costly strategies due to financial constraints. I investigate whether firms faced with this scenario invested in capital to deter future entry mobility from existing peers or potential entrants. By investing in capital, firms credibly commit to compete aggressively should a firm choose to enter their market (Dixit, 1980; Spence, 1977). Capital investment as a measure of competitive aggression is empirically interesting in that, unlike prices and output, incumbents should invest to deter entry regardless of whether they compete in Cournot or Bertrand competition (Etro, 2006).

Using the same regression OLS and 2SLS specifications as in the previous section, Table 8 presents results for changes in investment from the pre-crisis period (2006Q1 to 2008Q2) mean over the crisis period (2008Q3 to 2010Q1) mean. This earlier time period would be point the point where any investment differential driven by peer credit constraints should be visible. I define investment as the rolling four quarter expenditure on capital and R&D scaled by lagged assets. I replace missing R&D values with zero.

I observe that peer credit constraints positively predict changes in investment over the crisis. Depending on the specification used, I observe that a standard deviation difference in peer credit constraints induces a change in investment equivalent to 6.8% to 19.8% of a standard deviation. This appears consistent with the notion that firms invest strategically to protect market share from constrained peers.

Intuitively, I also find that access to credit as proxied by $\Delta L$ positively predicts changes in investment. Moreover, in the most saturated specification, Column 5 of Table 8, the magnitude of the coefficient on $\Delta L$ exceeds that of $\Delta$ Market $\bar{L}$.

An alternative and not mutually exclusive interpretation of the relationship between peer credit constraints and firm investment is that peer credit constraints may drive investment by increasing growth opportunities. In other words, the marginal product of capital is likely to be higher if a firm is more likely to grow and be more profitable in the future. Hence, the competitive outcomes in market share and rents that I show are associated with peer credit constraints may be driving firm investment behavior by expanding investment opportunities, rather than the other way around. As such the above results with respect to investment behavior do not distinguish between strategic and non-strategic investment.

Similar to the approach of Frésard and Valta (2016), I contend that if the association between peer credit constraints and investment behavior is driven by non-strategic considerations as opposed to strategic considerations, then I should observe a significant reduction in the magnitude of the coefficient of peer constraints on investment once I include measures of growth opportunities. To plausibly capture growth opportunities, I include a battery of controls which proxy for expectations of firm growth.

First, I include changes in various empirical measures of Tobin's $Q$. The first is the standard measure of $Q$, which is the market value of the firm to total book assets as used in Chung and Pruitt (1994) and Gutiérrez and Philippon (2016), among others. The second measure, $Q_{Total}$, includes estimates of intangible capital from Peters and Taylor (2017) in the denominator, to address measurement error related to intangible assets. Finally, I include $Q_{Alt.}$, which is as the ratio of market value of productive assets to gross PP&E plus intangibles. All three measures suffer from acounting and economic issues in capturing Tobin's $Q$, but by using a three-pronged approach as in Gutiérrez and Philippon (2016), I hope to ease measurement concerns.

Similar to the approach of Frésard and Valta (2016), I contend that if the association between peer credit constraints and investment behavior is driven by non-strategic considerations as opposed to strategic considerations, i.e. the impact of investment on peers' entry choice, then I should observe a significant reduction in the magnitude of the coefficient of peer constraints on investment once I include measures of growth opportunities. To plausibly capture growth opportunities, I include a battery of controls which proxy for expectations of firm growth.

Second, I include the ex-post realized change in profitability from the pre-crisis over the post-crisis periods. Insofar as firms' ex-ante growth expectations are correlated with realized future growth in profitability, this should capture growth expectations of the firm.

The management earnings forecast literature has consistently found a correlation between management forecasts and future earnings (Hassell and Jennings (1986); Lee, Matsunaga, and Park (2012)). Hence, in choosing the firm's investment level, management should in most instances already have a reasonable approximation of the firm's growth, which can thereby be roughly approximated by the ex-post growth of the firm's profitability.

Table 9 presents 2SLS results with both changes in the loan issuance of the focal firm's and sales-weighted product average lenders instrumented by equivalent MBS exposure of the lenders. The specifications in Table 9 are the same as in Table 8 Column 5 with firm controls, product market controls, and SIC sector effects, except various combinations of the aforementioned proxies for non-strategic motives to invest are also included. Columns 1 through 5 of Table 9 include each aforementioned proxy and column 6 includes all the proxies together.

I find that changes in the standard measure of Tobin's Q is the strongest predictors of future investment, while changes in ROA and $Q_{Total}$ also predict investment. Changes in $Q_{Alt.}$ does not appear to positively predict changes in investment.

Most importantly however, the coefficient on $\Delta$ Market $\bar{L}$ is essentially unchanged with the inclusion of these proxies. Its value ranges from 0.722 to 0.866, which is approximate to its value of 0.864 in the same specification without growth proxies.

This lends support to the notion that the association between product market peer constraints and investment is not driven by differences in growth opportunities alone, but rather, there appears to be a strategic element to this difference in investment ratio growth. Firms with constrained peers appear to invest more in order to deter future entry mobility of potential peers.

# 4 Robustness

## 4.1 Omitted Spillovers

Product market proximity may be correlated with proximity across firms along other channels, in particular through supply chains and technology networks. Firms with similar products likely rely on similar technology inputs and supply chains. One possible concern could be that results presented in this paper are driven not by product market spillovers, but spillovers from alternative firm networks that cluster along product markets.
However, it is difficult to argue that my results are likely driven by these alternative channels. This is because presumably the most likely outcome is that firms' are harmed by negative shocks to their technological and supply-chain peers. Product market spillovers should induce competitive effects, whereas technology and supply chain spillovers are more likely characterized by agglomeration effects (Huber, 2021).

For example, Bloom, Schankerman, and Van Reenen (2013) find that firms benefit from R&D tax-subsidies to their technological peers, but are harmed by R&D tax-subsidies to their product market rivals. Similarly, it is unclear why firms should benefit from negative shocks to firms along their supply chain. Insofar as product market proximity coincides with proximity along these alternative networks, one would expect that my results underestimate the positive spillovers of a negative credit shock to one's product market peers.

Still, if the TNIC measure captures both horizontal and vertical relationships between firms, then it is possible to argue that my results could be driven by credit supply shocks to the focal firm's upstream suppliers or downstream customers. If suppliers are forced to liquidate inventories in the event of credit constraints and such suppliers are erroneously categorized as competitors to the focal firm due to textually similar product offerings, downstream customers could conceivably benefit from reduced input prices. To some extent the concern that product markets maybe overlapping with supply chain relationships should be mitigated by the fact that Hoberg and Phillips (2016) remove TNIC pairs that are in traditional industries classified as shipping to each other using BEA Input - Output tables.

To investigate the possibility that my results are driven by vertical, rather than horizontal, relationships across firms, I create a measure of changes in lending of the firm's vertically related peers' lenders. The variable's construction is the same as $\Delta$Market $\bar{L}$, except that rather than defining the relevant peers as TNIC product market pairs, I use the Vertical TNIC of Frésard, Hoberg, and Phillips (2020). The Vertical TNIC captures the vertical relatedness of firm-pairs by relating textual descriptions of commodities and sub-commodities in the

BEA Input-Output Tables to firms' 10-K product descriptions. I refer to the measure as $\Delta$VTNIC $\bar{L}$.

Another aforementioned possibility is that the peer effects captured in this paper are driven not by product markets, but by technology networks. Firms overlapping in product market space are also likely to overlap in technological space. It would appear plausible that firms could benefit from their technological competitors being subject to a negative credit supply shock. However, the empirical literature indicates that agglomeration spillovers of R&D investment of firms' peers are likely to dominate any competitive effects (Bloom, Schankerman, and Van Reenen, 2013).

In order to address this potential source of endogeneity directly, I follow Bloom, Schankerman, and Van Reenen (2013) in creating a measure of technological proximity of firms by measuring the extent to which their patenting activities overlap along technology classes. More specifically, I merge Compustat with PATSTAT using the DISCERN linking table provided by Arora, Belenzon, and Sheer (2021). I then measure the share of each firm's patents from 2003 to 2007 in each 3-digit IPC technology class to create the firm-specific technology vector $T_i = (T_{i1}, T_{i2}, ..., T_{i,126})$, where $T_{i,\tau}$ is the share of patents of firm $i$ in technology class $\tau$. Technological proximity is then defined as in the uncentered correlation for all firm pairs $i$ and $j$ as:

$$PROX_{i,j} = \frac{T_i T_j}{\sqrt{T_i T_i^{\top}} \sqrt{T_j T_j^{\top}}} \tag{2}$$

In order to gauge the relative potential for spillovers of each technology peer, I measure each firm's R&D stock using the perpetual inventory method described by Hall, Jaffe, and Trajtenberg (2005), in which past R&D spending is iterated forward with an annual depreciation rate of 0.15. The R&D stock is then defined as $G_t = R_t + (1-\delta)G_{t-1}$, where $\delta$ is the depreciation rate and $R_t$ is the R&D spending at time t. I then combine the two measures to create a measure of potential technology spill-ins for each focal firm, $SPILL_i = \sum_{j \neq i} PROX_{ij} G_j$, which I use to weigh the mean change in loan issues of the lenders to the focal firm's technology peers, which are defined as those firms with non-zero technological proximity to the focal firm. This measure provides a proxy for the credit access of the firm's technological peers that is weighted by an index of the potential magnitude and relevance of their research to the focal firm. I refer to the measure as $\Delta$TEC $\bar{L}$.

Table C1 of Appendix C presents summary statistics with respect to $\Delta$TNIC $\bar{L}$ and $\Delta$TEC $\bar{L}$. Given that a majority of the sample either does not issue patents or have no measured technological proximity with R&D spending firms in the sample, the sample of firms with non-missing $\Delta$TEC $\bar{L}$ is limited to 497.

In Table C2 of Appendix C, I examine the pairwise correlations of my three measures of changes in lending to firm networks, namely $\Delta$Market $\bar{L}$, $\Delta$TNIC $\bar{L}$, and $\Delta$TEC $\bar{L}$. I also include the instrument for the sales-weighted mean of the product market lenders' exposure to the MBS, my instrumental variable for $\Delta$Market $\bar{L}$. Excepting a weak correlation between $\Delta$Market $\bar{L}$ and $\Delta$TEC $\bar{L}$, all cross-correlations between the three measures of changes in loan issuance to given firm networks are statistically indistinguishable from zero. I take this as evidence that, at least with respect to changes in lending, these networks are distinct from one another with little overlap.

Interestingly, the correlation between $\Delta$TEC $\bar{L}$ and $\Delta$Market $\bar{L}$ is negative, albeit only statistically-significant at the 10%. This suggests that firms whose product market peers saw a greater contraction in credit access also had technological peers which saw a smaller contraction in credit access. Theoretically, if the agglomeration spillovers of technology peers dominate the competitive spillovers of technology peers, this could result in overestimating the importance of $\Delta$Market $\bar{L}$ for the focal firm when $\Delta$TEC $\bar{L}$ is omitted. However, as shown in Table C2, the variation in $\Delta$Market $\bar{L}$ explained by the instrument should be unbiased given that the correlation between $\Delta$TEC $\bar{L}$ is equal to zero.

Panels A of Table 10 presents the results of the 2SLS model for the main dependent variables with the measure of changes in loan issuance of the lender to the focal firm's vertical peers, labeled $\Delta$VTNIC $\bar{L}$. All estimates include the same control variables as in previous specifications in addition to SIC-1 digit sector fixed effects. Under all specifications, changes in lending to the firms' vertically related peers fails to predict changes in the focal firm's lending. This suggests that credit shocks to the focal firms' vertical peers is unlikely to be a first-order driver of firm outcomes. Importantly, the coefficient on $\Delta$ Market $\bar{L}$ remains qualitatively and quantitatively similar to the baseline specifications for all dependent variables.

Table 10 Panel B presents the baseline 2SLS for the main dependent variables with the inclusion of $\Delta$TEC $\bar{L}$. Despite the substantial decline in observations and inclusion of $\Delta$TEC $\bar{L}$, the results remain qualitatively similar. The coefficients on $\Delta$ Market $\bar{L}$ in explaining changes in investment, markups, ROA, and market share are of similar magnitude to previous specifications absent $\Delta$TEC $\bar{L}$, albeit with higher standard errors presumably due to the reduced sample size.

The coefficient on $\Delta$Market $\bar{L}$ is however roughly halved with respect to sales. It appears unlikely that this is due to a reduction in omitted variable bias given that $\Delta$TEC $\bar{L}$ has no explanatory power for changes in sales. Sample characteristics, such as heterogeneity in the impact of $\Delta$Market $\bar{L}$ among patenting versus non-patenting firms or simply sample size seems like more plausible candidate explanations. To investigate this possibility, in Table C3 I present the same sample of firms with non-missing values for $\Delta$TEC $\bar{L}$, but remove $\Delta$TEC $\bar{L}$ from the specification. I find that the coefficient on $\Delta$Market $\bar{L}$ with respect to sales is essentially unchanged in this sample irrespective of whether or not $\Delta$TEC $\bar{L}$ is included.

Finally, I also find some evidence of the importance of technological peers in explaining the focal firm's ROA. Firms whose technological peers were less subject to the credit contraction appear to observe greater growth in ROA, as suggested by Column 3 of Panel B in Table 10. This suggests the presence of agglomeration spillovers across technology peers and is consistent with the results of Bloom, Schankerman, and Van Reenen (2013), who find that firms benefit from the R&D spending of their technological peers.

## 4.2 Timing of Lender-Borrower Matching

One potential concern with the results presented in this paper is that firms may observe the extent to which potential lenders are exposed to the financial crisis, resulting in assortative matching. This is more likely to be the case the closer the period used for defining borrower-lender pairs is to the credit crunch. While I follow Chodorow-Reich (2014), Chodorow-Reich and Falato (2022), and Kim (2021) in matching borrower-lender pairs using the borrower's last syndicated loan before September 30, 2008, one could argue that lenders' exposure to the financial crisis was observable by borrowers by this point in time. If this results in higher quality firms switching to higher quality lenders, the 2SLS results could be biased due to assortative matching. For example, Lehman Brother's stock price lost over 83% of its value between June 2007 and August 2008. The potential for collapse of Lehman Brother's over this period may have already raised fears of risk among borrowers for those banks highly connected to Lehman Brother's through co-syndication.

However, assuming lender exposure was observable to firms, it is not clear in which direction this would bias the results in this paper. It sounds plausible that better firms would borrow from better banks. However, more financially robust firms and firms with better access to alternative sources of finance should be less concerned with the health of their lender. For instance, Schwert (2018) finds that firms with access to bond markets borrowed from less capitalized banks on average.

To ease concerns of possible assortative matching, as a robustness test, I infer the firm's relationship lender using its last syndicated loan prior June 2007. This is five quarters earlier than the main specification.

This approach may introduce measurement error by assigning firms to lenders that have less salience to the firm going into the crisis period. Borrowers who began new lending-relationships with a different bank between June 2007 and September 2008 will be treated as though their most recent lender is of no importance.

Using the most saturated versions of my main 2SLS specifications from Column 5 of Tables 4 through 8, I present results for changes in sales, market share, ROA, markups, and investment in Table 11 using the earlier matched borrower-lender sample. The coefficients on instrumented variation in changes in loan issuance of peers' lenders are generally of equivalent or larger magnitude to those of the baseline specifications, but with greater standard errors. I interpret the manitude of the coefficients as suggesting that the previously presented estimates in Table 4 through 8 are not upward biased by assortative matching. This should ease concerns that temporal proximity to the credit crunch of the formation of lender-borrower relationships could be resulting in assortative matching that may bias results. Additionally, the greater standard errors is consistent with a weaker quality matching between relationship lenders and borrowers.

# 5 Conclusion

This paper documents evidence of large, positive spillovers of credit contractions across firms within product markets. The empirical literature has previously documented negative intra-regional spillovers of the credit crunch and negative direct effects on firms. To the best of my knowledge, this paper is the first to document inter-regional product market spillovers from a credit crunch. These results suggest an important aspect of a credit crunch is the redistribution of output and profitability within product markets.

As firms whose product market peers are hit by credit shocks grow faster, this redistributional spillover should serve to dampen the negative impact of credit crunches on aggregate output. This is in contrast to other credit spillovers previously identified in the literature, in particular regional spillovers (Huber, 2018), which exasperate the aggregate impact of direct credit shocks.

However, the results in this paper may also raise issues related to competition. Because small firms are particularly sensitive to credit contractions, this redistribution should serve to increase product market concentration and may have played a meaningful role in the increase in concentration. I find that credit shock spillovers may have increased markups, which is in line with the hypothesis that this redistribution lowered competition. Hence, from a welfare perspective, the impact of this redistribution is ambiguous. A fruitful direction for future research may be to document the macroeconomic impact of banking crises on concentration and economic rents.

Moreover, I find that peer credit constraints are positively associated with investment growth during the credit crunch and that this relationship is unmitigated by proxies for growth opportunities. This is consistent with theories of strategic investment, which suppose that firms may invest to deter entry mobility by credibly committing to a more aggressive output strategy in the event of entry. This behavior may in part explain why the losses in output and employment documented by Wix (2017) and Chodorow-Reich (2014) are persistent: Once market share is lost, rivals invest strategically to ensure the new equilibrium persists.

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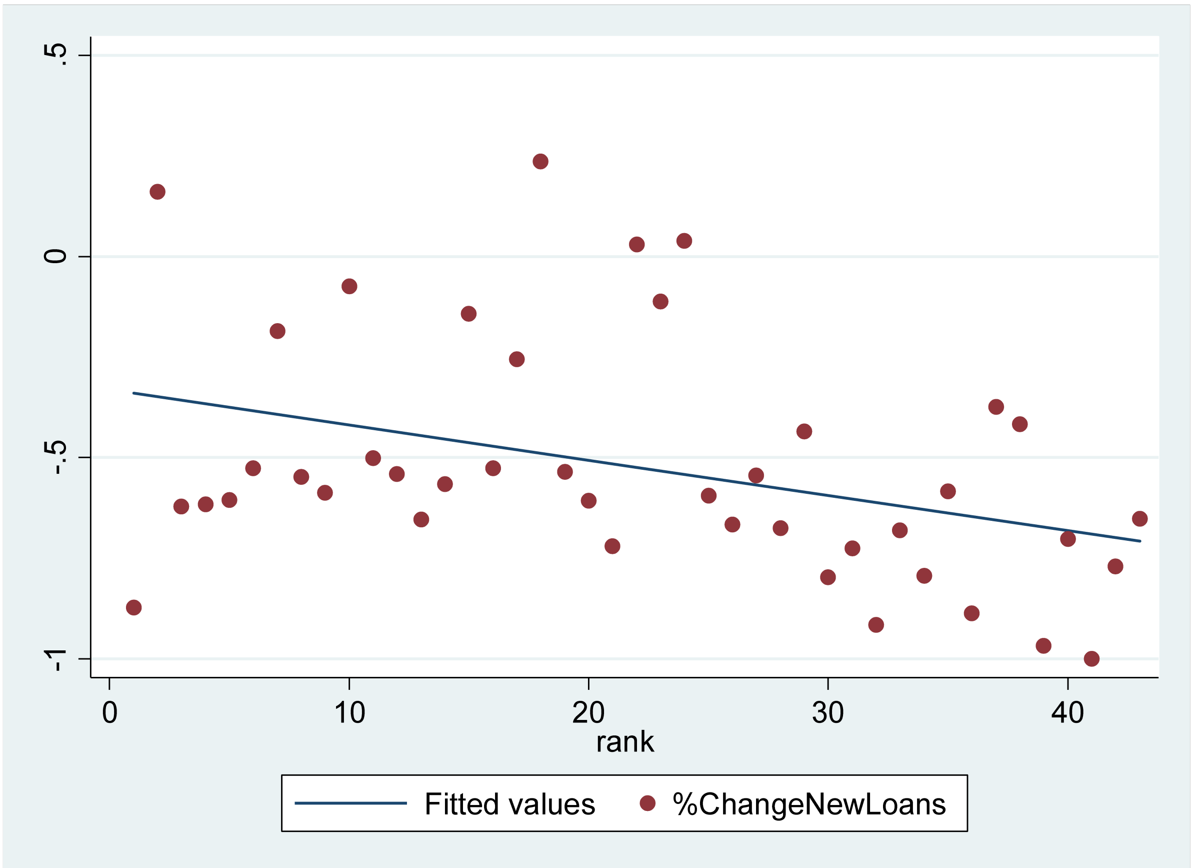
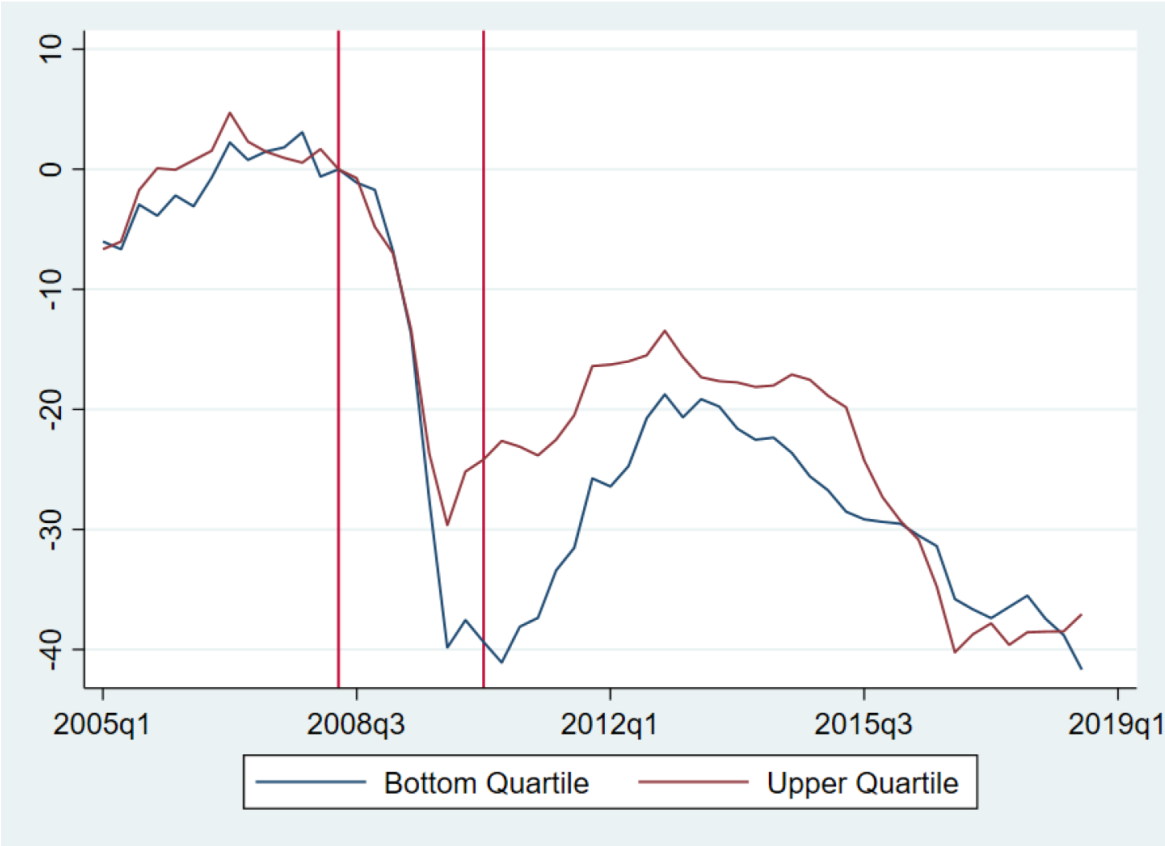
# Figures

**Figure 1:** Rank of First Principle Component Value and Percent Change New Loans



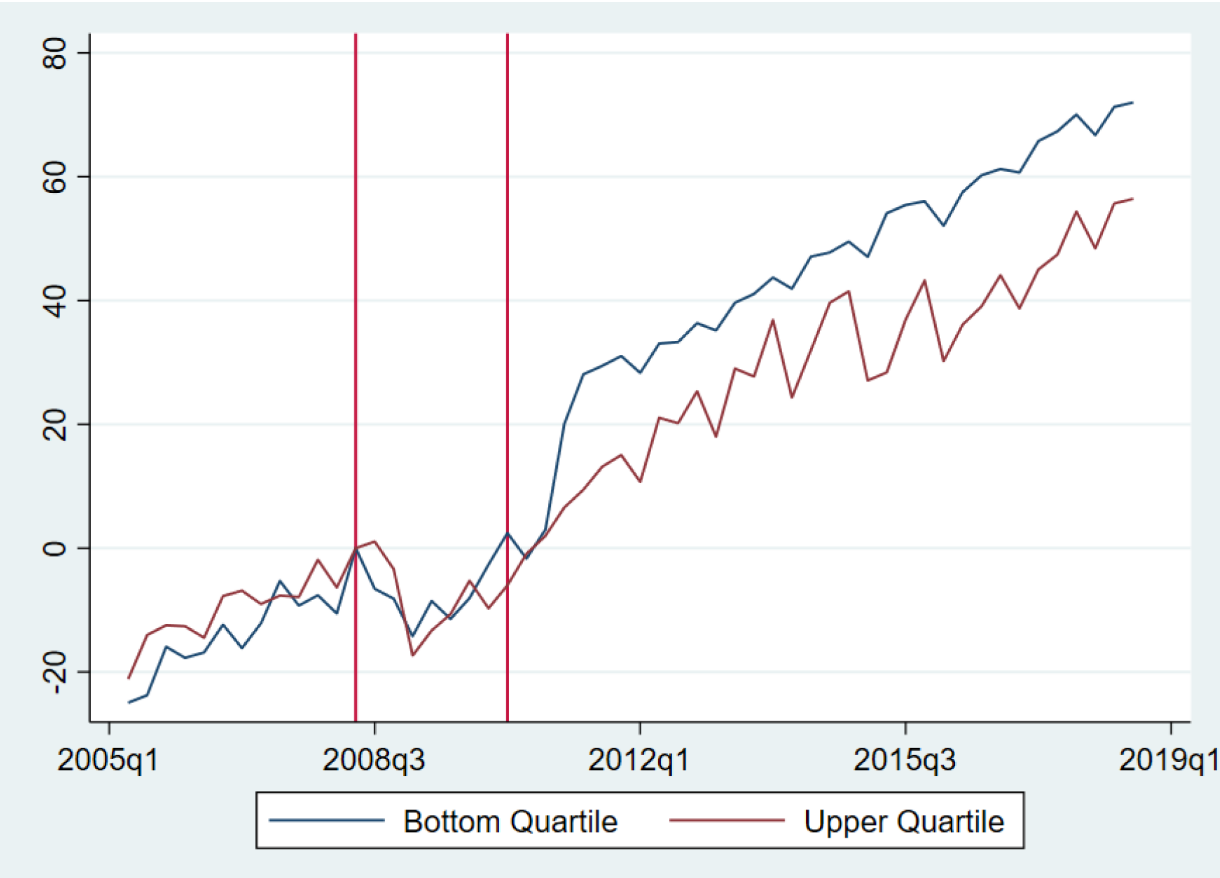

Percent change (annualized number of new loans), Oct-05 to Jun 07, to Oct-08 to Jun-09. The first principle component captures the banks exposure to the mortgage-backed securities market as measured by its share of syndicated loans where Lehman Brothers was the lead lender, the banks's stock price correlation with the ABX AAA 2006-H1 index over Q4-2007, and the share of revenue from trading in 2007-2008 over total assets. All data provided by Chodorow-Reich as from Chodorow-Reich (2014) https://scholar.harvard.edu/chodorow-reich/publications/loan-covenant-channel-how-bank-health-transmits-real-economy

**Figure 2:** Firm Investment Growth along Upper and Lower Quartile of Changes in Lender Loan Issuance




This figure shows the evolution of the mean log percentage growth in investment ratios relative to 2008-Q2 over time for firms with borrowing relationships with lead lenders in the bottom and top quartile of the distribution of lender health.

**Figure 3:** Mean log percentage growth of firms sales along changes in loan issuance of product market peers' lenders




This figure shows the evolution of the mean change in firms sales relative to 2008-Q2 over time of the lower and upper quartile of the distribution of changes in loan issuance of TNIC peers' lenders.

# Tables

**Table 1:** Variable Descriptions

This table shows the definitions of all variables. The definitions provide the Compustat Quarterly mnenomics when applicable. Firm financial data is sourced from Compustat. Changes in bank lending is sourced from Chodorow-Reich (2014). Lead lenders are connected to firms via pre-2008Q2 syndicated lending relationship.

| Variable | Definition |
|---|---|
| *Dependent Variables* | |
| ΔSales | $ln(\overline{SALEQ}_{(2010Q2:2016Q4)} + 1) - ln(\overline{SALEQ}_{(2006Q1:2008Q2)} + 1)$ |
| ΔMarket Share | ΔSales minus mean ΔSales of TNIC product market |
| ΔROA | $\ln(\overline{ROA}_{(2010Q2:2016Q4)} + 1 - ln(\overline{ROA}_{(2006Q1:2008Q2)} + 1)$ |
| ΔInvestment | $ln(\overline{Investment}_{(2010Q2:2016Q4)} + 1) - ln(\overline{Investment}_{(2006Q1:2008Q2)} + 1)$ |
| ΔL | Change in bank's lending: Oct/2005 - Jun/2007 over Oct/2008 - Jun/2009 |
| ΔMarket $\bar{L}$ | Sales-weighted leave-out mean of TNIC3 product market peers' ΔL |
| Size | $\ln(ATQ)$ |
| Net Leverage | $\frac{DLLTQ-CHEQ}{ATQ}$ |
| Market Net Leverage | Sales-weighted mean of peers' net leverage |
| No. of peers | ln(No. of peers in TNIC3 Product Market) |
| Investment | $\frac{(CAPXY_t+XRDY_t+CAPXY_{t-1}+XRDY_{t-1}+CAPXY_{t-2}+XRDY_{t-2}+CAPXY_{t-3}+XRDY_{t-3})}{ATQt-4}$ |
| Sales | $SALEQ$ |
| ROA | $\frac{OIBDPQ_t}{ATQ_{t-1}}$ |
| Q | $\frac{ATQ-CEQQ+(CSHOQ*PRCCQ)}{ATQ}$ |
| Alt. Q | $\frac{MKVALTQ+DLTTQ+DLCQ-ACTQ}{PPEGTQ}$ |
| Total Q | Market value to tangible + intangible capital (see Peters and Taylor (2017)) |

**Table 2:** Summary Statistics

This table shows summary statistics for the 1,491 firms used in the sample. Variable definitions as reported in Table 1. $\Delta$L refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. $\Delta$Market $\bar{L}$ refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. All other variables in Panel A defined as of the last pre-crisis quarter (2008-Q2) and are winsorized at the 1st and 99th percentile. Panel B reports percentage log changes in dependent variables. Changes in dependent variables are over the pre-crisis period (2006-Q1 to 2008-Q2) to the post-crisis period (2010-Q2 to 2016-Q4), except for $\Delta$Investment (Crisis), where the latter period is set as the crisis period (2008-Q3 to 2010-Q1).

| Panel A | Mean | Std.Dev. | p25 | Med. | p75 |
|---|---|---|---|---|---|
| Sales (Million USD) | 1283.61 | 3225.84 | 114.01 | 319.70 | 979.18 |
| Assets (Million USD) | 5694.23 | 15894.15 | 439.20 | 1305.44 | 3892.01 |
| $\Delta$L | -51.80 | 16.26 | -60.34 | -53.97 | -47.00 |
| $\Delta$Market $\bar{L}$ | -55.95 | 5.57 | -58.55 | -56.26 | -52.81 |
| Investment | 14.61 | 14.89 | 4.81 | 10.04 | 19.20 |
| Market Share | 1.12 | 2.71 | 0.13 | 0.38 | 1.01 |
| Net Leverage | 0.15 | 0.26 | -0.01 | 0.15 | 0.30 |
| Market Net Leverage | 0.15 | 0.15 | 0.05 | 0.16 | 0.26 |
| No. Competitors | 29.44 | 38.90 | 5.00 | 14.00 | 38.00 |
| ROA | 0.03 | 0.03 | 0.02 | 0.03 | 0.05 |
| Mark Up | 1.71 | 1.54 | 1.14 | 1.35 | 1.75 |
| Bank Dependent | 0.46 | 0.50 | 0.00 | 0.00 | 1.00 |
| Panel B | | | | | |
| $\Delta$ Sales | 8.15 | 53.38 | -17.69 | 9.23 | 35.97 |
| $\Delta$ Market Share | -6.82 | 21.23 | -13.90 | -2.38 | 2.48 |
| $\Delta$ ROA | -1.16 | 3.03 | -2.04 | -0.61 | 0.30 |
| $\Delta$ Investment (Crisis) | -2.45 | 7.38 | -4.24 | -0.95 | 0.92 |
| $\Delta$ Markup | -2.21 | 11.48 | -3.76 | -0.40 | 2.20 |

**Table 3:** First-Stage Results

This table reports the first-stage results of the two-stage least squares regressions. Lender Exposure refers to the rank-normalized first principal component of the focal firm's lender's exposure to three measures of the financial crisis. Market Lender Exposure refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Firm-level controls consist of log assets, net leverage, market net leverage, a dummy indicating whether or not the firm is bank dependent, and the log number of TNIC product market peers. The sample consists of the intersection of firms in the Compustat, Thomson Reuter's Dealscan, and the Hoberg and Phillips (2016) TNIC databases.

| | $\Delta$Market $\bar{L}$ | | $\Delta L$ |
|---|---|---|---|
| | (1) | (2) | (3) |
| Market Lender Exposure | 0.439*** | 0.421*** | 0.051 |
| | (0.022) | (0.022) | (0.051) |
| Lender Exposure | | -0.002 | 0.423*** |
| | | (0.007) | (0.032) |
| Constant | -35.432*** | | |
| | (2.213) | | |
| F-test of Instrument | 428.97 | 374.17 | 175.54 |
| Firm-level Controls | No | Yes | Yes |
| SIC-1 FEs | No | Yes | Yes |
| Observations | 1422 | 1409 | 1409 |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

**Table 4:** Sales

The table reports cross-sectional OLS and 2SLS regressions. Variables defined as in Table 1. Changes in sales defined over the pre-credit crunch period (2006Q1 to 2008Q2) to the post-credit crunch period (2010Q2 to 2016Q4). All control variables as of 2008Q2. ΔL refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. ΔMarket $\bar{L}$ refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Both ΔL and ΔMarket $\bar{L}$ are standardized. Columns 1 and 2 present OLS regressions results. Columns 3 to 5 present 2SLS results.

| | OLS | | 2SLS | | |
|---|---|---|---|---|---|
| | ΔSales: 2006Q1-2008Q2 to 2010Q2-2016Q4 | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| -(Δ Market $\bar{L}$) | 3.277** | 4.939*** | 5.914** | 7.862*** | 6.045** |
| | (1.418) | (1.479) | (2.394) | (2.616) | (2.850) |
| Δ L | | 1.964 | | 3.390 | 3.469 |
| | | (1.729) | | (4.075) | (4.120) |
| Log Assets | | -0.922 | | -0.926 | -0.769 |
| | | (1.125) | | (1.192) | (1.222) |
| Net Leverage | | 16.576** | | 17.642** | 15.143** |
| | | (6.650) | | (7.151) | (7.332) |
| Market Net Leverage | | -40.303*** | | -44.492*** | -39.047*** |
| | | (10.594) | | (10.581) | (11.101) |
| Log No. Competitors | | 2.180** | | 1.909* | 2.744** |
| | | (1.059) | | (1.080) | (1.277) |
| Bank Dependent | | 11.206*** | | 10.995*** | 10.554*** |
| | | (3.862) | | (3.877) | (3.902) |
| Constant | 8.188*** | 8.393 | 8.216*** | 9.739 | |
| | (1.414) | (9.740) | (1.415) | (10.232) | |
| Observations | 1422 | 1409 | 1422 | 1409 | 1409 |
| Adjusted $R^2$ | 0.003 | 0.029 | 0.001 | 0.026 | 0.017 |
| SIC 1-dig. FEs | No | No | No | No | Yes |

$^{*}$ $p < .10$, $^{**}$ $p < .05$, $^{***}$ $p < .01$.Heteroskedasticity-robust standard errors are reported in parentheses.

**Table 5:** Market Share

The table reports cross-sectional OLS and 2SLS regressions. Variables defined as in Table 1. Changes in market share are defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the post-credit crunch period (2010-Q2 to 2016-Q4). All control variables as of 2008-Q2. ΔL refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. ΔMarket $\bar{L}$ refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Both ΔL and ΔMarket $\bar{L}$ are standardized. Columns 1 and 2 present OLS regressions results. Columns 3 to 5 present 2SLS results.

| | OLS | | 2SLS | | |
|---|---|---|---|---|---|
| | ΔMarket Share: 2006Q1-2008Q2 to 2010Q2-2016Q4 | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| -(Δ Market $\bar{L}$) | 1.115 | 1.555** | 3.100*** | 4.111*** | 4.746*** |
| | (0.737) | (0.734) | (1.148) | (1.195) | (1.330) |
| Δ L | | 0.206 | | 0.951 | 0.465 |
| | | (0.478) | | (1.362) | (1.384) |
| Log Assets | | -3.722*** | | -3.740*** | -3.936*** |
| | | (0.481) | | (0.495) | (0.513) |
| Net Leverage | | 7.141*** | | 8.010*** | 8.060*** |
| | | (2.456) | | (2.683) | (2.696) |
| Market Net Leverage | | 2.337 | | -1.161 | -2.961 |
| | | (5.005) | | (5.212) | (5.610) |
| Log No. Competitors | | 1.569*** | | 1.406*** | 1.123** |
| | | (0.454) | | (0.453) | (0.530) |
| Bank Dependent | | -0.194 | | -0.331 | 0.050 |
| | | (1.345) | | (1.377) | (1.380) |
| Constant | -6.841*** | 14.973*** | -6.878*** | 15.977*** | |
| | (0.580) | (3.682) | (0.582) | (3.740) | |
| Observations | 1348 | 1337 | 1348 | 1337 | 1337 |
| Adjusted $R^2$ | 0.002 | 0.077 | -0.006 | 0.063 | 0.060 |
| SIC 1-dig. FEs | No | No | No | No | Yes |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

**Table 6:** ROA

The table reports cross-sectional OLS and 2SLS regressions. Variables defined as in Table 1. Changes in ROA defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the post-credit crunch period (2010-Q2 to 2016-Q4). All control variables as of 2008-Q2. $\Delta$L refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. $\Delta$Market $\bar{L}$ refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Both $\Delta$L and $\Delta$Market $\bar{L}$ are standardized. Columns 1 and 2 present OLS regressions results. Columns 3 to 5 present 2SLS results.

| | OLS | | 2SLS | | |
|---|---|---|---|---|---|
| | $\Delta$ROA: 2006Q1-2008Q2 to 2010Q2-2016Q4 | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| -($\Delta$ Market $\bar{L}$) | 0.350*** | 0.404*** | 0.578*** | 0.691*** | 0.591*** |
| | (0.083) | (0.085) | (0.125) | (0.132) | (0.135) |
| $\Delta$ L | | 0.089 | | 0.249 | 0.133 |
| | | (0.116) | | (0.222) | (0.221) |
| Log Assets | | -0.046 | | -0.044 | -0.072 |
| | | (0.062) | | (0.068) | (0.067) |
| Net Leverage | | 1.312*** | | 1.443*** | 1.282*** |
| | | (0.377) | | (0.416) | (0.422) |
| Market Net Leverage | | -0.437 | | -0.854 | -0.234 |
| | | (0.618) | | (0.616) | (0.623) |
| Log No. Competitors | | -0.298*** | | -0.326*** | -0.198*** |
| | | (0.064) | | (0.066) | (0.072) |
| Bank Dependent | | -0.380* | | -0.404* | -0.319 |
| | | (0.210) | | (0.209) | (0.204) |
| Constant | -1.157*** | -0.023 | -1.155*** | 0.092 | |
| | (0.080) | (0.548) | (0.080) | (0.577) | |
| Observations | 1416 | 1403 | 1416 | 1403 | 1403 |
| Adjusted $R^2$ | 0.013 | 0.041 | 0.007 | 0.031 | 0.014 |
| SIC 1-dig. FEs | No | No | No | No | Yes |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

**Table 7:** Markups

The table reports cross-sectional OLS and 2SLS regressions. Variables defined as in Table 1. Changes in mark ups defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the post-credit crunch period (2010-Q2 to 2016-Q4). All control variables as of 2008-Q2. $\Delta$L refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. $\Delta$Market $\bar{L}$ refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Both $\Delta$L and $\Delta$Market $\bar{L}$ are standardized. Columns 1 and 2 present OLS regressions results. Columns 3 to 5 present 2SLS results.

| | OLS | | 2SLS | | |
|---|---|---|---|---|---|
| | $\Delta$Markup: 2006Q1-2008Q2 to 2010Q2-2016Q4 | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| -($\Delta$ Market $\bar{L}$) | 0.187 | 0.592$^{**}$ | 0.233 | 0.870$^{*}$ | 0.883$^{*}$ |
| | (0.279) | (0.296) | (0.447) | (0.506) | (0.518) |
| $\Delta$ L | | 0.828$^{**}$ | | 2.297$^{**}$ | 1.705$^{*}$ |
| | | (0.401) | | (0.992) | (1.007) |
| Log Assets | | 0.019 | | 0.195 | 0.170 |
| | | (0.293) | | (0.312) | (0.312) |
| Net Leverage | | 2.824$^{*}$ | | 4.018$^{**}$ | 4.244$^{**}$ |
| | | (1.695) | | (1.861) | (1.890) |
| Market Net Leverage | | -7.724$^{***}$ | | -8.568$^{***}$ | -4.147$^{*}$ |
| | | (2.480) | | (2.500) | (2.417) |
| Log No. Competitors | | -1.644$^{***}$ | | -1.707$^{***}$ | -0.819$^{***}$ |
| | | (0.302) | | (0.307) | (0.297) |
| Bank Dependent | | -1.106 | | -1.327 | -1.021 |
| | | (0.844) | | (0.874) | (0.862) |
| Constant | -2.201$^{***}$ | 2.839 | -2.199$^{***}$ | 1.780 | |
| | (0.329) | (2.445) | (0.330) | (2.589) | |
| Observations | 1230 | 1217 | 1230 | 1217 | 1217 |
| Adjusted $R^2$ | -0.001 | 0.042 | -0.001 | 0.029 | 0.000 |
| SIC 1-dig. FEs | No | No | No | No | Yes |

$^{*}$ $p < .10$, $^{**}$ $p < .05$, $^{***}$ $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

**Table 8:** Investment Over the Crisis

The table reports cross-sectional OLS and 2SLS regressions. Variables defined as in Table 1. Changes in investment defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the credit crunch period (2008-Q3 to 2010-Q1). All control variables as of 2008-Q2. ΔL refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to June 2007 relative to Oct. 2008 to June 2009. ΔMarket L refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Both ΔL and ΔMarket $\bar{L}$ are standardized. Columns 1 and 2 present OLS regressions results. Columns 3 to 5 present 2SLS results.

| | OLS | | 2SLS | | |
|---|---|---|---|---|---|
| | ΔInvestment: 2006Q1-2008Q2 to 2008Q3-2010Q1 | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| -(Δ Market $\bar{L}$) | 0.509*** | 0.740*** | 1.078*** | 1.499*** | 0.864*** |
| | (0.183) | (0.186) | (0.285) | (0.306) | (0.303) |
| Δ L | | 0.255 | | 1.130** | 1.066** |
| | | (0.233) | | (0.545) | (0.525) |
| Log Assets | | 0.077 | | 0.132 | 0.112 |
| | | (0.133) | | (0.146) | (0.143) |
| Net Leverage | | -1.757** | | -1.006 | -1.809** |
| | | (0.838) | | (0.894) | (0.898) |
| Market Net Leverage | | -4.501*** | | -5.777*** | -4.793*** |
| | | (1.388) | | (1.468) | (1.461) |
| Log No. Competitors | | -0.760*** | | -0.840*** | -0.650*** |
| | | (0.160) | | (0.166) | (0.161) |
| Bank Dependent | | -2.230*** | | -2.363*** | -2.185*** |
| | | (0.514) | | (0.533) | (0.518) |
| Constant | -2.453*** | 0.879 | -2.453*** | 0.841 | |
| | (0.191) | (1.107) | (0.191) | (1.159) | |
| Observations | 1491 | 1480 | 1491 | 1480 | 1480 |
| Adjusted $R^2$ | 0.004 | 0.048 | -0.002 | 0.028 | 0.009 |
| SIC 1-dig. FEs | No | No | No | No | Yes |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

**Table 9:** Changes in Investment over the Crisis with Growth Expectations

The table reports cross-sectional 2SLS regressions. Variables defined as in Table 1. Changes in investment defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the credit crunch period (2008-Q3 to 2010-Q1). All control variables as of 2008-Q2. ΔL refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. ΔMarket L refers to the sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Both ΔL and ΔMarket $\bar{L}$ are standardized. Columns 1 and 2 present OLS regressions results. Columns 3 to 5 present 2SLS results.

| | ΔInvestment: 2006Q1-2008Q2 to 2008Q3-2010Q1 | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| -(Δ Market $\bar{L}$) | 0.722** | 0.815** | 0.823*** | 0.886*** | 0.754** |
| | (0.295) | (0.321) | (0.301) | (0.321) | (0.328) |
| Δ L | 0.902* | 1.299** | 1.013* | 0.956* | 1.028* |
| | (0.506) | (0.595) | (0.529) | (0.559) | (0.596) |
| Δ Q | 6.853*** | | | | 7.587*** |
| | (1.828) | | | | (2.377) |
| Δ Q Alt. | | -0.299 | | | -1.445*** |
| | | (0.368) | | | (0.410) |
| Δ Q Total | | | 1.537** | | 0.364 |
| | | | (0.605) | | (0.758) |
| Δ ROA | | | | 0.127 | 0.045 |
| | | | | (0.086) | (0.093) |
| Observations | 1480 | 1402 | 1464 | 1356 | 1285 |
| Adjusted $R^2$ | 0.035 | 0.002 | 0.012 | 0.014 | 0.030 |
| Controls | Yes | Yes | Yes | Yes | Yes |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

**Table 10:** Robustness: Vertical and Technological Spillovers

The table reports second-stage results of 2SLS regressions. Changes in dependent variables in columns 1 through 3 defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the post-credit crunch period (2010-Q2 to 2016-Q4). In column 4, changes in investment defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the credit crunch period (2008-Q3 to 2010-Q1). ΔL refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. ΔMarket $\bar{L}$ refers to the leave-out mean of the same index aggregated along the focal firm's product market peers. Both ΔL and ΔMarket $\bar{L}$ are instrumented by the respective lenders' exposure to the MBS market. ΔVTNIC $\bar{L}$ is the same as ΔMarket $\bar{L}$, but defined along the firm's vertical counterparts. ΔTec $\bar{L}$ is the same, but defined along the firm's technology peers. Variables otherwise defined as in Table 1. Control variables from baseline regressions included, but not shown.

| | (1)<br>Δ Sales | (2)<br>Δ Market Share | (3)<br>Δ ROA | (4)<br>Δ Markup | (5)<br>Δ Investment |
|---|---|---|---|---|---|
| | | *Panel A: Vertical Spillovers* | | | |
| -(Δ Market $\bar{L}$) | 5.920** | 3.286** | 0.653*** | 0.997* | 1.483*** |
| | (2.957) | (1.354) | (0.143) | (0.540) | (0.341) |
| Δ L | 1.456 | 0.762 | 0.180 | 2.165** | 1.182* |
| | (4.702) | (1.599) | (0.253) | (0.942) | (0.636) |
| Δ VTNIC $\bar{L}$ | 173.217 | -29.425 | 3.839 | -11.051 | -15.295 |
| | (114.337) | (55.343) | (5.375) | (23.600) | (13.469) |
| Observations | 1204 | 1142 | 1197 | 1036 | 1269 |
| Adjusted $R^2$ | 0.019 | 0.068 | 0.029 | 0.035 | 0.023 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| | | *Panel B: Technology Spillovers* | | | |
| -(Δ Market $\bar{L}$) | 2.774 | 3.380 | 0.784*** | 1.102 | 0.641 |
| | (5.112) | (2.867) | (0.192) | (0.879) | (0.422) |
| Δ L | -2.602 | 0.339 | 0.088 | 4.213** | 1.489** |
| | (6.861) | (2.304) | (0.302) | (1.657) | (0.746) |
| Δ TEC $\bar{L}$ | 17.572 | -83.629 | 20.484*** | 23.362 | -10.395 |
| | (138.605) | (62.278) | (7.191) | (31.300) | (16.716) |
| Observations | 456 | 423 | 450 | 443 | 478 |
| Adjusted $R^2$ | 0.010 | 0.041 | 0.060 | -0.009 | 0.024 |
| Controls | Yes | Yes | Yes | Yes | Yes |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

**Table 11:** Robustness: Adjusted Lender-Borrower Matching Timing

The table reports cross-sectional 2SLS regressions. Borrowers are matched to lenders over 2003-Q1 to 2007-Q2, one year earlier compared to previous specifications. Changes in dependent variables in columns 1 through 4 defined over the pre-credit crunch period (2005-Q1 to 2007-Q2), one year earlier than in other specifications, to the post-credit crunch period (2010-Q2 to 2016-Q4). In column 5, changes in investment defined over the pre-credit crunch period (2005-Q1 to 2007-Q2) to the credit crunch period (2008-Q3 to 2010-Q1). All control variables as of 2008-Q2. ΔL refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009 and is standardized. ΔMarket L refers to the standardized sales-weighted leave-out mean of the same index aggregated along the focal firm's product market peers. Both ΔL and ΔMarket $\bar{L}$ are instrumented by the respective lenders' exposure to the MBS market. Variables defined as in Table 1.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Δ Sales | Δ Market Share | Δ ROA | Δ Markup | Δ Investment |
| -(Δ Market $\bar{L}$) | 11.840* | 4.731* | 0.547* | 1.417* | 1.049* |
| | (6.495) | (2.777) | (0.281) | (0.837) | (0.543) |
| Δ L | 6.576* | 0.244 | 0.172 | 0.280 | 0.835* |
| | (3.990) | (1.384) | (0.238) | (0.841) | (0.484) |
| Log Assets | 0.397 | -3.927*** | -0.114 | -0.218 | 0.232 |
| | (1.244) | (0.584) | (0.074) | (0.285) | (0.156) |
| Net Leverage | 19.588** | 6.796** | 1.199*** | 1.799 | -2.383** |
| | (7.925) | (2.780) | (0.447) | (1.786) | (0.931) |
| Market Net Leverage | -38.463*** | -3.275 | -0.306 | -2.063 | -5.655*** |
| | (13.373) | (7.575) | (0.707) | (2.365) | (1.696) |
| Log No. Competitors | 2.078 | 0.874 | -0.263*** | -0.630** | -0.684*** |
| | (1.583) | (0.673) | (0.088) | (0.281) | (0.181) |
| Bank Dependent | 7.453* | 0.526 | -0.454** | -0.052 | -1.689*** |
| | (4.002) | (1.437) | (0.217) | (0.821) | (0.516) |
| Observations | 1300 | 1233 | 1287 | 1099 | 1383 |
| Adjusted $R^2$ | -0.013 | 0.069 | 0.000 | 0.000 | 0.001 |
| SIC 1-dig. FEs | Yes | Yes | Yes | Yes | Yes |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

# Appendices

## A Principal Component Analysis and Lender Health

For a sample of the 43 largest syndicated lenders, I estimate the first principal component along three measures of bank exposure to the financial crisis: the correlation of the bank's stock return with an index of AAA mortgage-backed securities, the share of syndicated loans the bank participated in where Lehman Brother's had a lead role, and the share of trading revenue relative to the lender's total assets.

The first principal component (eigenvalue 1.539) captures 51.33% of the variation in the three variables. It has a correlation coefficient of 0.88 with the share of syndicated loans the bank participated in which Lehman Brother's had a lead role, 0.73 with the correlation of the bank's stock return with an index of AAA mortgage-backed securities, and 0.48 with the share of trading revenue relative to the lender's total assets.[4]

Following Chodorow-Reich and Falato (2022), I then rank the banks according along the first principal component score. This ranking is able to explain variation in the change of lending across banks from the pre-crisis to the crisis period. Below is a scatter plot and univariate regression slope for the percent change in the annualized number of new loans from the period of October 2005 to June 2007 and October 2008 to June 2009 regressed on the banks PCA ranking, with the most exposed bank being ranked last.

[4] Chodorow-Reich's dataset on bank exposure to the financial crisis has four missing cells. With respect to the share of syndicated loans the bank participated in which Lehman Brother's had a lead role, there is no obvious value besides absent or 1. Additionally, Cobank, Utrecht-America, and WestLB are not publicly-listed, so it was not possible to calculate the correlation of the bank's stock return with an index of AAA mortgage-backed securities. Technically, this poses a problem to PCA, which requires that all variables be non-missing for each observation. While Chodorow-Reich and Falato (2018) also extract the first principal component for these variables with the same sample of banks, they do not allude to how they address the missing cells. I use the iterative imputation approach of Husson and Josse (2016), which essentially replaces the missing cell with its sample mean and adjusts it depending on values of the available variables for the given row, its sample correlation with the other variables, and the standard deviation of the missing variable.

## B Mark Up Estimation Procedure

Here I briefly outline the markup estimation procedure. The notation and procedure closely follows that of De Loecker, Eeckhout, and Unger (2020), who consider the following production function:

$$Q_{it} = F(\Omega_{it}, \mathbf{V}_{it}, K_{it}), \tag{1}$$

where $F(.)$ is the production technology which transforms inputs into outputs, $\Omega_{it}$ is a Hicks-neutral productivity term, $\mathbf{V}_{it}$ a vector of variable inputs, and $K_{it}$ is the capital stock.

Using standard first-order conditions and defining the markup, $\mu_{it}$, as the price over marginal cost, it can be shown that:

$$\mu = \theta^{v}_{it} \frac{P_{it} Q_{it}}{P^{V}_{it} V_{it}} \tag{2}$$

where $\theta^{v}_{it}$ is the output elasticity of the variable input, $P_{it} Q_{it}$ are firm revenues, and $P^{v}_{it} V_{it}$ are total variable cost expenditures. While the latter two variables are available in Compustat, $\theta^{v}_{it}$ must be estimated. Here, annual, industry-specific (NAICS 2-digit) Cobb-Douglas production functions are estimated:

$$q_{it} = \theta^{V}_{st} v_{it} + \theta^{K}_{st} k_{it} + \omega_{it} + \epsilon_{it} \tag{3}$$

where lower cases denote logs and $\epsilon_{it}$ is unanticipated shock to output or measurement error.[5] In estimating $\theta^{V}_{st}$ and $\theta^{K}_{st}$ we are faced with the endogeneity problem that the

[5]The industry-specific out elasticities from 1980 to 2016 as estimated in De Loecker, Eeckhout, and Unger (2020) are available at: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/5GH8XO

unobservable productivity shock term, $\omega_{it}$, may be correlated with the firm's input choice. Note that $k_{it}$ is fixed and dynamic (a state variable) at time $t$ and is chosen before productivity at time $t$ is known by the firm, whereas $v_{it}$ is chosen at time $t$. The productivity process is given by a first order Markov process:

$$\omega_{it} = g(\omega_{it-1} + \chi_{it}) \tag{4}$$

where productivity shocks, $\chi_i t$ are uncorrelated with input decisions chosen before period $t$. This gives rise to the moment condition:

$$\mathbb{E}[\chi_t | k_t, v_{t-1}, k_{t-1}, ...] = 0 \tag{5}$$

Intuitively, variable input demand can be written as a function of productivity and capital, e.g. $v_{it} = v(\omega_{it}, kit)$ where $v$ is strictly increasing in $\omega_{it}$. This gives rise to the "proxy structure" - where productivity can be modeled as the inverse of the input demand function and thereby estimated using observables: $v^{-1}(v_{it}, k_{it}) = a(v_{it}, k_{it})$. "Guesses" of the variable output elasticity ($\theta^v$) and capital elasticity ($\theta^k$) are then chosen to yield estimates $\hat{\omega}_{it}$ and $\hat{\epsilon}_{it}$ which satisfy (5) and (3). Finally, the estimated industry-specific output elasticities are used to obtain firm-level markup estimates as in equation 2.

# C Supply Chain and Technology Network Spillovers

**Table C1:** Summary Statistics

| | Mean | Std.Dev. | p25 | Med. | p75 | n |
|---|---|---|---|---|---|---|
| $\Delta$ TEC $\bar{L}$ | -0.50 | 0.02 | -0.51 | -0.50 | -0.49 | 497 |
| $\Delta$ VTNIC $\bar{L}$ | -0.56 | 0.01 | -0.57 | -0.57 | -0.56 | 1316 |

**Table C2:** Pairwise Correlations

| | $\Delta$ TEC $\bar{L}$ | $\Delta$ VTNIC $\bar{L}$ | $\Delta$ Market $\bar{L}$ | Market Lender Exposure |
|---|---|---|---|---|
| $\Delta$ TEC $\bar{L}$ | 1 | | | |
| $\Delta$ VTNIC $\bar{L}$ | 0.00842 | 1 | | |
| $\Delta$ Market $\bar{L}$ | -0.0786$^{*}$ | 0.0359 | 1 | |
| Market Lender Exposure | -0.0405 | -0.0140 | .5940$^{***}$ | 1 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Table C3:** Robustness: Technology Spillovers Sample Without Technology Spillovers

The table reports second-stage results of 2SLS regressions. Specifications and the sample are the same as in Table 10 Panel B except ΔTec $\bar{L}$ is excluded. Changes in dependent variables in columns 1 through 5 defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the post-credit crunch period (2008-Q3 to 2010-Q1). In column 4, changes in investment defined over the pre-credit crunch period (2006-Q1 to 2008-Q2) to the credit crunch period (2008-Q3 to 2010-Q1). ΔL refers to the percent change in loan issuance of the focal firm's lender over Oct. 2005 to Jun. 2007 relative to Oct. 2008 to June 2009. ΔMarket $\bar{L}$ refers to the leave-out mean of the same index aggregated along the focal firm's product market peers. Both ΔL and ΔMarket $\bar{L}$ are instrumented by the respective lenders' exposure to the MBS market. ΔTec $\bar{L}$ is change in lending to the firm's technology peers. Variables otherwise defined as in Table 1. Control variables from baseline regressions included.

| | (1)<br>Δ Sales | (2)<br>Δ Market Share | (3)<br>Δ ROA | (4)<br>Δ Markup | (5)<br>Δ Investment |
|---|---|---|---|---|---|
| -(Δ Market $\bar{L}$) | 2.790 | 3.263 | 0.802*** | 1.129 | 0.636 |
| | (5.076) | (2.855) | (0.195) | (0.880) | (0.423) |
| Δ L | -2.542 | 0.089 | 0.166 | 4.297*** | 1.447** |
| | (6.732) | (2.258) | (0.301) | (1.643) | (0.725) |
| Observations | 456 | 423 | 450 | 443 | 478 |
| Adjusted $R^2$ | 0.012 | 0.040 | 0.038 | -0.011 | 0.026 |
| Controls | Yes | Yes | Yes | Yes | Yes |

* $p < .10$, ** $p < .05$, *** $p < .01$. Heteroskedasticity-robust standard errors reported in parentheses.

# Paper 2: Business Dynamism in Germany: A Cross-Country Analysis

# Business Dynamism in Germany:
# A Cross-Country Analysis

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This Draft: August 17, 2023

**Abstract**

In this paper, we document remarkably low levels of business dynamism in Germany compared to a sample of European counterparts. Utilizing administrative German panel data of manufacturing and mining firms, our findings reveal that Germany has the lowest share of high-growth firms and the second-lowest share of rapidly shrinking firms among our sample of European countries. Moreover, this lack of dynamism in firm growth is not compensated by firm births and deaths, as Germany also ranks toward the bottom of the sample for business churn. These differences do not appear to be explained by firm composition, as we present evidence that weak dynamism in Germany is persistent across size and industry. To the best of our knowledge, this paper is the first to note low levels of business dynamism in Germany. We speculate that Germany's comparatively lower exposure to the Eurocrisis over our sample period may explain its lower business dynamism.

**JEL Classification: O40, O52, L93**

**Keywords:** Business dynamism, Europe, Germany

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# 1 Introduction

It is a widespread notion that business dynamism, as in the process of firm creation, growth, decline and death, and the reallocation of factors of production associated with this process, is a key to productivity growth (Schumpeter, 1942). The typical argument is that firms, often newly born, introduce new or better business models and products, which are, due to agency problems, more difficult to generate in large established firms. Based on their performance, firms grow or eventually decline or even exit the market. The valuable but scarce resources that these firms set free become available to other, new or more productive firms, which allows them to grow. Overall, the fast and efficient reallocation of factors of production associated with business dynamism improves the match between resources and their most productive use. This in turn stimulates entrepreneurship, experimentation, innovation and ultimately productivity growth. Hence, the overall performance of an economy is related to the cross-sectional dispersion of firm productivity 'shocks' since it shapes factor reallocation and the idiosyncratic incentives for the firms to create or destroy jobs or improve productivity.

Bravo-Biosca (2016) and Bravo-Biosca, Criscuolo, and Menon (2016) show that the share of rapidly growing and rapidly shrinking firms tend to be lower in Europe than in the US. They demonstrate that this lack of firm dynamism may at least in part explain the lower productivity growth in Europe compared to the US, especially in the face of disruptive innovation. In this paper, we update and expand upon Bravo-Biosca's main results for a number of European countries with a particular focus Germany.

While there is a large literature examining cross-country business dynamism (see for example Calvino, Criscuolo, and Menon (2018), Calvino, Criscuolo, and Verlhac (2020)), Germany has typically been excluded from these analyses despite being the fourth-largest economy globally.[1] For example, the OECD's DynEmp, which covers 16 OECD countries in addition to Brazil and Costa Rica, does not include Germany and is frequently used for studies of business dynamism. This is presumably related to strict privacy laws in Germany, which limit access to necessary micro-data.

We compile multiple vintages of Bureau van Dijk's Amadeus and Orbis, to conduct a cross-country analysis of business dynamism. To resolve apparent reporting problems

[1] One important exception is Biondi et al. (2022), who examine trends in business dynamism across Europe, including Germany, and and find a widespread decline in business dynamism across European countries. They report summary statistics that are consistent with Germany having a low level of business dynamism

specific to German firms in the sample, we combine the data with administrative German panel data of the universe of manufacturing and mining firms with over 20 employees.

Our findings point to markedly low levels of business dynamism in Germany. Specifically, we find that among our sample of European countries Germany has the lowest share of high-growth firms and second-lowest share of rapidly shrinking firms.

Moreover, this lack of dynamism in firm growth is not compensated by firm births and deaths, as Germany also ranks toward the bottom of the sample for business churn. This relative lack of dynamism in Germany does not appear to be explained by firm composition, as we present evidence that this pattern of weak dynamism in Germany appears to be persistent across size and industry, extending beyond the manufacturing and mining sectors. To the best of our knowledge, this is the first paper to note a lack of business dynamism in Germany.

Aside from the work by Bravo-Biosca, Criscuolo, and Menon (2016) and Bravo-Biosca (2016), this paper builds on a number of papers that focus on the relationship between entry, exit, net job creation and aggregate productivity. For example, Foster, Haltiwanger, and Krizan (2001) observe that business churn accounts for a quarter of changes in productivity in the US manufacturing industry. Using a similar approach, Brandt, Van Biesebroeck, and Zhang (2012), find that business churn accounts for an even greater share of changes in productivity in China's manufacturing sector. Similar results are found by Asturias et al. (2017) in Chilean and Korean contexts. Haltiwanger, Jarmin, and Miranda (2013), Decker et al. (2014), and Haltiwanger et al. (2016) document the disproportionate role of new businesses (i.e. start-ups) in creating new jobs and increasing aggregate productivity.

Another strand of the literature analyzes what shapes business dynamism and efficient factor reallocation. The literature so far suggests that these factors likely differ across countries. In fact, institutions, the functioning of markets, competition and selection mechanisms, which are actually supposed to ensure productivity-enhancing reallocation of valuable but scarce resources based on performance differentials, are likely to differ across countries. For example, Bartelsman, Haltiwanger, and Scarpetta (2013) show how differences in distortions across countries can shape the selection of firms in the market, the degree of churn (i.e. business dynamism), and the allocation of resources among firms. McGowan, Andrews, and Millot (2017) analyze how different policies in OECD countries can create 'zombie' firms, as in poor-performing and unprofitable firms that do not decline or exit the market, hold valuable resources (i.e., capital and labor). To the extent to which resources are scarce, this hampers the growth of more productive firms, which ultimately results in lower aggregate productivity growth and job creation. Haltiwanger, Scarpetta, and Schweiger (2014) provide evidence that stringent labor market institutions mitigate business dynamism. Similarly, Bartelsman, Gautier, and De Wind (2016) argue that labor market frictions that hamper the reallocation of labor across firms generate a static factor misallocation, reduce the incentives to invest in new and risky technologies and worsen the productivity distribution contributing to large aggregate productivity losses.

More recently, several papers seek to document and explain a decline in business dynamism. Decker et al. (2020) observe a decline in business dynamism in the United States. Akcigit and Ates (2019) point to declining knowledge diffusion as a culprit. Bijnens and Konings (2020), Biondi et al. (2022) and Calvino, Criscuolo, and Verlhac (2020) present evidence that declining business dynamism is a global phenomenon occurring within industries. Calvino, Criscuolo, and Verlhac (2020) find that declines in business dynamism are related to increases in market concentration and productivity dispersion.

Our paper aims to provide a comprehensive analysis of the level of business dynamism in Germany and compare it with a selection of European countries. We find that Germany is among the least dynamic of economies in our sample of European countries. Given that European countries, in general, have been found to have relatively low levels of business dynamism in comparison to other developed countries (Bravo-Biosca, 2016), one can reasonably infer that Germany has among the lowest levels of business dynamism in the developed world.

Given the implications of business dynamism for economic performance and productivity growth identified in the literature, there is a clear need for a closer investigation of the institutional factors hampering business dynamism in Germany. Existing literature emphasizes the importance of labor market regulations, financial development and bankruptcy regimes as potential areas in which policy may drive differences in dynamism. We find little evidence that Germany is an outlier across these dimensions. We speculate that a reduced exposure to the Eurocrisis and a tighter labor market over the sample period may have resulted in less disruption and more constraints on German firms' growth.

Finally, we document that Germany maintained the highest growth in total factor productivity in the sample over this period. This poses an intriguing question. Given that business dynamism is understood to be a central driver of productivity growth, how has

Germany managed to maintain significant productivity growth in spite of relatively low levels of dynamism?

We suggest that the answer might lie in higher productivity growth observed within German firms. Another possibility, not mutually exclusive from the first, could be that the dynamics of business and productivity are more tightly connected in Germany. On a speculative note, superior management practices could potentially fuel higher within-firm productivity growth in Germany compared to the rest of the sample. Another contributing factor could be Germany's strong presence in global export markets, which might not only enhance productivity growth within firms but also strengthen the relationship between business dynamics and productivity (see for example Akcigit and Ates (2019)).

The remainder of the paper is organized as follows. Section 2 presents the data and methodology. Section 3 presents the main results and explores the robustness and external validity of our findings. Section 4 provides a discussion and section 5 concludes.

## 2 Data and Methodology

Our main source of firm-level European data is Bureau van Dijk's Orbis Historical combined with the 2017, 2019, and 2023 vintages of Bureau van Dijk's Amadeus. Orbis Historical is constructed from every annual vintage of Orbis along guidelines suggested by Kalemli-Özcan et al. (2022).

These combined datasets considerably improve on reporting issues endemic to using individual vintages of Orbis and Amadeus separately. First, Amadeus and Orbis differ in coverage. Second, both are subject to reporting lags of two-years on average. Third, both are subject to survivorship bias. Orbis Global drops all firm-year observations older than 5 years and all firms which are no longer active in the business register. Amadeus deletes companies that fail to report after 5 years and keeps company-year observations for up to 10 years. Finally, different vintages can rely on different data providers for each country, such that individual vintages may differ in coverage for reasons unrelated to attrition. By combining 5 different Bureau van Dijk vintages/products, we are able to increase coverage for each country included in the sample.

To create representative samples from each country in the sample, we closely follow the instructions provided by Kalemli-Özcan et al. (2022). In cases of duplicates due to firms changing reporting calendar schedules, we keep the previous calendar schedule for firms at the end of the sample period and the ensuing schedule for firms at the start of the sample period. In addition, we limit the sample to countries that Bajgar et al. (2020) find have the most representative in Orbis data when compared to OECD Multiprod, which is based on administrative databases. These countries are Belgium, Portugal, Finland, Sweden, France, Italy, and Germany.

We observe a considerable number of German firms reporting precisely zero growth in employment over the observation window in Orbis/Amadeus. To illustrate this phenomenon, in Figure A1 we plot the share of firms with precisely zero growth among all firms and stagnant firms, as in those that grew or shrunk no more than 1% at annualized rate over an observation window from 2014 to 2017. While Germany has the highest share of stagnant firms with exactly zero growth, this phenomenon is especially pronounced for large firms. Taken at face value, Figure A1 would suggest that approximately 28.1% of stagnant German firms with 250 or more employees did not increase or decrease employment by one single employee. This is over 16 times the same share in France.

The implausibility of these figures suggests that data artifacts specific to German observations in Orbis/Amadeus may erroneously lead one to conclude that Germany has low business dynamism if these zero-growth firms did in fact grow. Following correspondence with Creditreform, Bureau van Dijk's source of German data, we learned that approximately 90% of German firm-year observations are based on interviews. These interviews may involve firms simply confirming the previous years' numbers or providing approximate estimates of their own revenue and employee counts. Accordingly, Creditreform data may be ill-suited for an investigation of firm growth.

For this reason, we turn to administrative data for Germany. This data is provided by the Research Data Centers of the Federal Statistical Office of Germany. It contains the universe of manufacturing and mining and quarrying firms (sectors B and C along the NACE Rev. 2 Classification) with over 20 employees up until the 2017.[2]

To combine the Orbis/Amadeus data with the German panel data, we adjust the Orbis/Amadeus data to be as comparable to that of the German administrative data as possible. Specifically, this means limiting the sample to firms in the manufacturing and mining and quarrying sectors and dropping all firm-years with less than 20 employees.[3] While these

[2]See https://www.destatis.de/DE/Methoden/Qualitaet/Qualitaetsberichte/Industrie-Verarbeitendes-Gewerbe/jahresbericht-mehrbetriebsunternehmen.html for a description.

[3]Additionally, because the German administrative data treats subsidiaries as distinct firms, we focus on unconsolidated accounts for those firms reporting both consolidated and unconsolidated accounts in Orbis/Amadeus.

criteria do restrict the breadth of our coverage, Bajgar et al. (2020) find that Orbis is most representative for manufacturing industries and firms with more than 10 employees. They further document that employment in firms with under 20 employees accounts for only approximately 11% of aggregate employment in the same sample of countries. Moreover, Bravo-Biosca (2016) cautions that including micro-firms results in the distribution of growth rates being most populated at the extremes: Most firms of only a few employees neither hire nor fire over a three-year window and those that do start from such a low base that their growth rates dwarf those of larger firms. The resulting bifurcated distribution does not lend itself to meaningful cross-country analysis, as the share of micro-firms would drive most of the variation in the distribution of growth rates. Hence, while we lose some coverage with these restrictions, we gain comparability and representativeness.

We focus on employment as our measure of firm size. This is for two reasons. First, Bajgar et al. (2020) find that Orbis is more representative of employment levels and dynamics than it is for sales and value-added. Second, the conversion and units of financial data in Orbis differ across vintages and are poorly documented. This poses a challenge to combining financial data across multiple vintages of Orbis/Amadeus with administrative data.

We follow Bravo-Biosca (2016) in estimating firm growth using the following equation:

$$[(\frac{empl_{i,2017}}{empl_{i,2014}})^{1/3} - 1] * 100 \tag{1}$$

in which $empl_{i,t}$ refers to firm employment of firm $i$ in year t.

There are several important caveats to this approach. First, it requires that firms have at least 20 employees both at the start and end of the growth window. This may result in survivorship bias, as firms that shrink considerably may simply fail and exit the sample. Truly small firms with less than 20 employees at the start of the growth window would not enter the sample. We later address this by looking at firm births and deaths using Eurostat data.

Another caveat is that we cannot distinguish between organic and inorganic growth with this approach. For example, if a firm acquires a large plant from another firm, the buyer would grow considerably and the seller would shrink considerably under our approach. However, the market for corporate control is arguably an important component of business dynamism and it is not clear that we would better capture business dynamism by excluding inorganic growth if we could.

# 3 Results

## 3.1 Distribution of Firm Growth

We begin by summarizing the levels of employment in the sample by country in Table 1 Panel A. With respect to firm size distribution, countries do not seem to differ considerably until the right tail of the distribution. Altogether, we observe that France tends to have larger firms across the distribution, whereas Portugal is characterized by relatively small firms.

In Table 2 Panel B, we look at firm growth. We observe considerable variation in mean growth rates across countries. In particular, we observe that Germany has the lowest mean annualized growth rate over the observation window with a value of 1.28%. In contrast, firms in Portugal saw an average annualized employment growth rate of 3.93%. However, at a glance, it is not clear if this is due to differences in aggregate growth, differences in attrition rates or differences in firm size.

We move onto histograms of firm growth distributions, allocating each firm in to one of 11 growth categories, in Figure 1a based on their annualized growth rates.[4] This approach closely follows that of Bravo-Biosca (2016).

Relative to other countries, we observe a bunching toward the center three growth intervals, ranging from -5% to 5% per annum, in Belgium and Germany. This suggests the two countries may have relatively weak business dynamism compared to the rest of the Sample. At a glance, Finland appears to have the flattest distribution, whereas the distributions of Italy and especially Portugal are shifted to the right relative to the rest of the sample.

In Figure 1b, we zoom in on the share of firms that grow or shrink rapidly, defined as more than 20% per year over the sample period. We observe that Germany has the lowest share of high-growth firms in the sample. Approximately 1.21% of German firms are high-growth firms in our sample, compared to approximately 4.27% in Portugal. Similarly, Germany has the second lowest share of rapidly shrinking firms, representing approximately 0.63% of German firms, compared to 2.78% in Finland. These results are consistent with a relative lack of creative destruction in Germany.

[4] The growth intervals are $]-\infty$;-20[, [-20;-15[,[-15;-10[,[-10;-5[,[-5;-1[,-1;1[1;5[5;10[,[10;15[,[15;20[,[20;$\infty$[.

To further explore the distribution of firm growth in Germany, we present the share of firms in each growth bracket relative to the rest of the sample in Figure 1c. We find that Germany has approximately 62.3% fewer high-growth firms and 49.5% fewer rapidly shrinking firms relative to the rest of the sample. Moreover, Germany has fewer firms with changes in employment greater than 10% in absolute values and instead has a greater share of firms that stagnate or grow modestly.

Together, these results suggest that Germany and also Belgium have markedly lower business dynamism than the other countries in the sample. In the following, we explore whether or not these results can be explained by differences in the type of firms that populate each country.

## 3.2 Firm Growth Across Firm Size

One plausible explanation for heterogeneity in business dynamism across countries is differences in the composition of firms that populate each country. For instance, if a country's firms tend to be larger for whatever reason, one may observe more modest average changes in firm growth simply because the firms on average have a larger base from which to grow. Notably, the median German firm is larger than that of the median firm in all countries in the sample except France (see Table 1 Panel A), so it seems ex-ante plausible that this hypothesis could explain at least some of Germany's low business dynamism.

We explore this possibility by breaking down the sample by firm size. As in Bravo-Biosca, Criscuolo, and Menon (2016), we define small firms as those with 20 to 49 employees, medium-sized firms as those with 50 to 249 employees, and large firms as those with 250 employees or more as of 2014. Results are displayed in Figures 2a through 2f.

Unsurprisingly, in Figure 2b, we find that a higher share of small firms undergo rapid growth of 20% or more per year over the observation window. We do also observe a lower share of rapidly shrinking firms among small firms compared to medium and large firms. This is presumably due to survivorship bias, as firms that end up with below 20 employees are not in the sample.

Strikingly, however, we find relatively consistent patterns in the ranking of business dynamism by country as measured by the share of rapidly growing and shrinking firms. Medium-sized German firms have the lowest proportion of rapidly growing and rapidly shrinking firms (see Figure 3d). Large German firms also have the lowest share of rapidly growing firms and the second lowest share of rapidly shrinking firms (see Figure 3f).

Together, we take the in Figures 3a through 3f speaks as speaking against the hypothesis that Germany's relative lack of business dynamism is driven by differences in the size distribution of its firms.

## 3.3 Firm Births and Deaths

One concern could be that by focusing on the dispersion of firm growth rates, we fail to fully capture business dynamism. Firm growth can be thought of as the intensive margin of business dynamism. However, the extensive margin, firm births and deaths, may in fact be even more important aspects of business dynamism. The two components of business dynamism may also be distinct from each other. This may be the case if, for instance, firms have a higher propensity to exit than to shrink in one country compared to another due to financial constraints or labor market rigidities.

To overcome this concern, we look to Eurostat's Business Demography statistics on firm births and deaths as a share of the population of firms for the manufacturing sector.[5] The data runs from 2014 to 2020 for all countries in our sample and covers the universe of firms with at least 10 employees. Deaths refer to the cessation of an existing firm but exclude exits related to mergers and restructuring. Similarly, births refer to new firms but exclude spin-offs. We add the death rate and birth rate to get the churn rate. Results are reported for every other year of the sample in Figure 3a.

We observe a correlation of 0.803 between the death rate and birth rate along country-years. We also note that the death rate is higher than the birth rate for all countries in all years. This could reflect a trend toward consolidation in the manufacturing and mining industries. Alternatively, it may simply reflect that the sample is conditional on having at least 10 employees and many newly formed firms may not reach 10 employees in the year in which they establish. Moreover, there is a high degree of persistence in the ranking of firms by churn rates. For example, Belgium has the lowest churn rate in each year observed and Portugal consistently has the highest.

Interestingly, we observe that despite having the lowest proportion of shrinking firms as observed in previous figures, Portugal consistently has the highest firm death rates. We speculate that this may be due to greater financial or labor market rigidities which force firms that would otherwise downsize to exit.

Most interesting, however, for our purposes is the relationship between business churn rates and measures of business growth. Countries with less dynamic distributions of firm growth appear to have low churn rates. Notably, Germany and Belgium tend to rank toward the bottom of the sample for both churn and high-growth firms. This is consistent with the notion that both churn and the distribution of firm growth capture the same phenomenon.

Importantly, it also indicates that Germany's low business dynamism is not just a within firm phenomenon. Not only do fewer German firms grow rapidly and shrink rapidly, but fewer of them fail and there are fewer new firms formed in a given year.

The persistence in the cross-country ranking across the dispersion of employment growth and churn also speaks to the validity of the employment data. One possible concern

[5]Data on mining and quarrying firms is also available, but cells are too sparsely populated to make meaningful comparisons across countries

is that our main exercises are comparing administrative data for Germany to commercial data for the rest of the sample. If the commercial data overstates changes in employment within firms, for example, due to measurement error, our main findings could potentially be a data artifact. That results are similar when using business churn suggests that this is unlikely to be the case. Both Germany and Belgium rank toward the bottom with respect to both dimensions, further reinforcing the idea that the two countries have low levels of business dynamism.

A relevant question for our findings is the extent to which they apply to sectors other than the manufacturing and mining industries that make up our sample. While we lack information on the dispersion of employment in other sectors for Germany, we use Eurostat to investigate whether or not our findings extend to other sectors, at least with respect to business churn.

Figure 3b follows the same procedure as Figure 3a, but covers all sectors with the exception of the manufacturing, mining, financial and real estate sectors.[6] Ranking countries by the level of churn in each year, we find that countries with more churn in the manufacturing sector also tend to have more churn in the rest of the non-financial economy. This suggests that our results with respect to business dynamism are relevant for other sectors as well. Notably, we observe that the finding that Germany ranks toward the bottom of business churn extends to other sectors.

We then compare mean churn, birth, and death rates of Germany to the rest of the sample in Table 2 with industry-years as the unit of observation.[7] We find that Germany has statistically significantly lower rates across all three variables compared to the rest of the sample. The differences also appear to be economically considerable. Germany's churn rate is approximately 41% lower than the sample average.

One possibility is that more dynamic industries have a higher share of employment in Germany and, if accounted for, the true churn rate faced by the German workers could actually be much more comparable to that of the rest of the sample than that suggested by this exercise. To investigate this possibility, we run a series of weighted regressions on the birth rate, death rate, and churn rate with an indicator variable equal to one for Germany

[6] Specifically we exclude NACE Rev. 2 single-digit sectors B, C, K, and L.

[7] In defining industries we seek to maximize both coverage and granularity. Accordingly, we take the most disaggregated NACE Rev. 2 classification with sample wide coverage. In some instance this means taking a 'single-digit' sector, such as in the case of Construction (NACE Rev. 2 F), and up to the 4-digit industry, such as in the case of Computer Consulting Agencies (NACE Rev. 2 J6202).

and zero otherwise. The weights used are employment and the number of firms of each industry-year observation.

Table B.1 reports results without fixed effects and finds that, across all specifications, German industries are less dynamic as measured by births, deaths, and churn. Including industry and year fixed effects with standard errors clustered at the industry level, we find that German industries do have less churn but that this appears to be primarily driven by fewer deaths. The coefficient on firm births is similar in magnitude with and without fixed effects, but is not statistically significant regardless of the weights used when fixed effects are included. Arguably though, for determining whether or not Germany employment is more distributed toward more dynamic industries, the results without fixed effects are the most relevant.

Together, we take these results as indicating that it is not the case that German employment is more distributed toward high dynamism industries. Germany has significantly lower churn rates than the rest of the sample and this appears to be distributed across sectors, reinforcing the notion that Germany has weak business dynamism compared to other countries.

# 4 Discussion

The results presented in this paper beg two questions: Firstly, why is business dynamism in Germany so low? And second, despite its low business dynamism, why does Germany observe relatively high levels of productivity growth compared to the rest of the sample?

While a comprehensive analysis identifying the causal factors driving Germany's low business dynamism is beyond the scope of this paper, we look to the literature on drivers of differences in cross-country business dynamism to find candidate explanations. While we cannot conclusively attribute Germany's relative lack of business dynamism to any particular factor and its reasons may be multifaceted, this exercise has the potential to inform policymakers intent on bolstering business dynamism in Germany.

Perhaps the most common explanation in the literature for explaining cross-country differences in business dynamism are labor market regulations. Intuitively, by increasing the cost of hiring and firing of workers, stringent labor regulations may hinder business dynamism by reducing firms' responsiveness to shocks. Moreover, stringent labor regulations discourage investing in risky technologies as firms may be prevented from shedding labor should their investment fail (Bartelsman, Haltiwanger, and Scarpetta, 2013).

Consistent with the idea that labor regulations hinder business dynamism, Haltiwanger, Scarpetta, and Schweiger (2014) find evidence that stringent labor market regulations hamper job reallocation. Bravo-Biosca, Criscuolo, and Menon (2016) find that R&D- and labor-intensive industries have more stagnant firms in countries with more stringent labor market regulations. Autor, Kerr, and Kugler (2007) find evidence that labor regulations reduce firm entry and employment flows.

A second recurring explanation in the literature is financial development. Access to finance is critical to firm growth as especially small firms are unlikely to be able to fund their growth internally. Greater financial development has been shown empirically to be related to growth in sectors likely to be financially dependent (Rajan and Zingales, 1998). Aghion, Fally, and Scarpetta (2007) find that financial development is associated with entry of small firms and greater firm growth among successful firms. Beck et al. (2008) present evidence that financial development is especially important for the growth of small firms.

Another factor that has received attention in the literature is bankruptcy regimes. The impact of stringent bankruptcy regimes are theoretically ambiguous. By penalizing unsuccessful ventures, tight bankruptcy legislation may hinder entrepreneurship. However, stronger creditor rights may make it easier for entrepreneurs to get access to finance. Accordingly, Bravo-Biosca, Criscuolo, and Menon (2016) find that the impact of bankruptcy regulations on business dynamism is sector dependent. Still, while the impact of bankruptcy regimes on business dynamism is ambiguous, it should be in both borrowers and creditors interest for bankruptcy proceedings to move efficiently. In line with this notion, Calvino, Criscuolo, and Menon (2018) find that more efficient bankruptcy proceedings is among the most important indicators predicting lower declines in job reallocation rates and entry over time.

We explore whether these factors are plausible drivers of Germany's low business dynamism relative to the rest of the sample. To this end, we present indicators on the efficiency of insolvency resolution, financial development, and labor market stringency for each country in the sample.

Our indicator on insolvency resolution comes from the World Bank and evaluates the costs and time associated with insolvency proceedings with a score based on the distance to the frontier. The indicator is presented as a distance to the frontier with the frontier being the best practices and its underlying data being based on interviews with insolvency practitioners in each country. The frontier is defined as a score of 100 with higher scores being closer to the frontier.

The indicator on financial development is domestic credit provided by the financial sector as a share of GDP and is sourced from the World Bank World Development Indicators. This is similar to approaches used by Bravo-Biosca, Criscuolo, and Menon (2016), among others.

Finally, the indicator labor market stringency comes from the Fraser Institute's Economic Freedom of the World database and is a composite of regulatory indicators collected from the World Bank and World Economic Forum. The indicator seeks to capture the burden of regulations surrounding hiring and firing workers, minimum wage laws, and the prevalence of collective bargaining, among other components.[8]

Figures A.2a through A.2c present values for each indicator separately averaged over 2014 to 2017. With respect to labor market regulations and the efficiency of insolvency proceedings, Germany ranks toward the middle of the sample. This provides suggestive evidence that these policy areas are not driving Germany's relatively low business dynamism.

[8]Each indicator mentioned in this section thus far is downloaded from the OECD's Structural Policy Structural Policy Indicators Database for Economic Research.

However, looking to domestic credit as a share of GDP, Germany ranks last with an average domestic credit to GDP ratio of 135.95%. This could be gaken to suggest that an important driver of lower business dynamism in Germany is weaker financial development.

Still, the idea that German firms have less access to finance than their southern European counterparts in the years following the Eurocrisis defies belief. Lower aggregate use of external credit may reflect, for example, higher use of internal financing and lower homeownership rates. The notion that German firms do not have difficulty accessing capital relative to their European counterparts is supported by Survey on the Access to Finance of Enterprises (SAFE) survey results, which find that a low share of German firms report access to finance as a major problem (see Apendix A.3).

In contrast, as shown in Figure A.3, the SAFE survey reveals that German firms are considerably more likely than their European counterparts to report the availability of skilled labor as a major problem. Intuitively, it may be difficult to grow a firm rapidly in a tight labor market. Moreover, potential workers may be less likely to pursue entrepreneurship and upend established firms if it is easy to find a job. Hence, the relative lack of business dynamism observed in Germany could be the outcome of a comparably calm macroeconomic environment relative to its European peers, involving fewer negative shocks to firms and thereby lower unemployment rates.

Consistent with the idea that the explanation for Germany's low business dynamism is related to a tight labor market and reduced exposure to the Eurocrisis, we observe in Figure A.4 that German firms have the lowest average unemployment rate over this period. Moreover, the ranking of unemployment rates by country largely aligns with the ranking of business dynamism observed in the previous section. We speculate that this suggests that the combination of a calmer macroeconomic environment and tighter labor market faced by Germany over this time period may have been an important contributor to relatively low levels of rapid changes in firm-level employment and business churn.

Both the theoretical and empirical literature indicate a robust link between business dynamism and productivity growth (see for example Schumpeter (1942) and Bravo-Biosca, Criscuolo, and Menon (2016)). We document in Figure A.5 that Germany enjoyed high total factor productivity (TFP) growth relative to its European peers over the sample period.[9] Only Finland, which was among the most dynamic of countries in our sample, reported higher rates of TFP growth over this period. With the exception of Germany, the summary statistics

[9]We use real TFP based on national accounts as calculated by Feenstra, Inklaar, and Timmer (2015) as part of the Penn World Table database.

presented in this sample are consistent with a link between business and productivity growth. This suggests a puzzle with respect to the sources of productivity growth in Germany.

Changes in aggregate productivity can be decomposed into the following factors:[10] (*i*) within-firm changes, which captures changes in within-firm productivity weighted by its market share, (*ii*) between-firm changes, which capture changes in aggregate productivity due to changes in the market share of firms, (*iii*) the cross effect, which captures changes in productivity due to firms with high productivity growth expanding and low productivity growth firms shrinking, (*iv*) entry, which captures changes in productivity due to differences between the productivity of incumbents and entrants, and (*v*) exit, which captures changes in productivity due to differences in the productivity of existing firms and exiting firms. The latter four can be thought of as business dynamics.

The extent to which aggregate productivity changes are attributable to business dynamics as opposed to within-firm changes varies considerably according to the industry, country, and the time period assessed (Ahn, 2001). Still, the literature overwhelmingly finds that firm dynamics are the more important source of productivity gains (see Ahn (2001), Decker et al. (2014), Syverson (2011) for discussions of the relevant literature).

Given that compared to the rest of the sample, business dynamism in Germany is low across several metrics, but TFP growth is high suggests that at least one of the following is true: Within-firm productivity growth in Germany is particularly high compared to the rest of the sample or business dynamics is more tightly related to productivity in Germany than among other countries in the sample over the sample period.

Why might within-firm productivity growth be high in Germany? One plausible hypothesis is that firms in Germany may have better management practices. Better-managed firms may be more responsive to new technologies and growth opportunities and there is evidence in the literature suggesting German firms are on average better managed than those in most other countries. Bloom and Van Reenen (2007) present evidence that management quality is strongly correlated with productivity and also find that the quality of management practices in Germany is statistically indistinguishable from that of the United States, whereas the United Kingdom and France have significantly worse management practices. In a sample of 20 countries and over 9000 firms, Bloom et al. (2012) find that German firms have the second-best average management practices in the sample after the United States.

[10]See Bartelsman, Haltiwanger, and Scarpetta (2009) for a formal decomposition.

One factor that could plausibly explain both higher within-firm productivity growth and a tighter link between business dynamics and productivity is trade. Most famously, the Melitz Model posits that following trade liberalization, the most productive firms expand at the expense of less productive firms (Melitz, 2003). Further, exporting firms are more exposed to global competition. Greater competition can increase the incentives to innovate, resulting in within-firm gains in productivity (Syverson, 2011). For example, Bloom, Draca, and Van Reenen (2016) find that increased import competition from China induced greater innovation among European firms. Consistent with the notion that German firms are exposed to greater competition, De Loecker and Eeckhout (2018) find that firms in Germany have among the lowest average markups in Europe. Finally, as theorized by Akcigit and Melitz (2022), exporting firms experience productivity gains due to market size. Market size effects of trade should impact within firm productivity and enhance the relationship between firm growth and productivity if more productive firms export.

Among the sample, Germany is particularly active in global markets. While the country's export-to-GDP ratio is not considerably higher than that of Sweden or Portugal (Figure A.6a), it is much more active in export markets beyond the EU (Figure A.6b). Its extra-EU trade amounts to approximately 19.03% of GDP, well above, for example, France's level of 9.77%. Only Belgium is more active in export markets. This exposure to global trade may have allowed Germany to experience higher productivity growth than much of the sample despite having more limited business dynamism.

Speculatively, Belgium and Germany's orientation toward global trade may also explain their relatively lower levels of employment change and business churn. Rapid changes in firm employment and churn reflect both creative destruction and exogenous shocks. More globally-oriented firms presumably have a wider customer base and thereby may experience less volatility, resulting in more stable employment across firms. This may be especially true over the sample period, as the Eurozone experienced pronounced volatility following the onset of the European debt crisis. Firms whose output was more concentrated in the Eurozone may have faced stronger shocks over this time period, resulting in higher levels of measured business dynamism without the accompanying TFP growth.

Finally, Germany's relatively reduced exposure to the Eurocrisis may have played an important role in its TFP growth. Over the credit crunch of the Eurocrisis, if reallocation was driven by variation in credit availability rather than productivity, then this could have resulted in a weaker relationship between measures of churn and productivity. Notably, Foster, Grim, and Haltiwanger (2016) find that in the U.S. context, the Great Recession was associated with weaker reallocation toward productive firms than in previous recessions and speculate that this may be related to financial constraints.

# 5 Conclusion

In this study, we provide an updated analysis of the state of business dynamism in Europe along the lines of Bravo-Biosca (2016), paying particular attention to Germany, which is typically absent from these analyses. Our paper leverages administrative German panel data of manufacturing and mining firms, providing perspective on business dynamism in one of the world's largest economies.

Our findings reveal a comparative lack of dynamism within Germany's business landscape, characterized by a relatively low share of high-growth firms, low levels of firm births and deaths (business churn), and a persistent pattern of weak dynamism across size and industry.

Notably, Germany exhibits the lowest levels of business dynamism within our European sample. When compared to the already low dynamism observed in Europe overall (Bravo-Biosca, Criscuolo, and Menon, 2016), it can reasonably be inferred that Germany ranks among the least dynamic economies in the developed world. Given the well-established link between business dynamism and productivity growth, as well as its association with entrepreneurial innovation and economic development, it is of vital interest to identify the factors that are hindering business dynamism in Germany.

We find little evidence that the relative lack of business dynamism in Germany is attributable to oft-investigated barriers to business dynamism such as labor market regulations, insolvency regimes, and financial development. We speculate that Germany's comparably calm macroeconomic environment resulted in low business dynamism. Moreover, we suppose that Germany's relatively high TFP growth without comparably high levels of business dynamism may have been the outcome of better management practices and possibly greater exposure to global export markets. However, future research could more thoroughly investigate how Germany is able to achieve relatively high TFP growth with low levels of business dynamism.

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# Tables

**Table 1:** Summary Statistics

Panel A describes the employment levels of manufacturing and mining firms in the sample as of 2014 by country. Panel B describes the annualized employment growth rates of firms in the sample over 2014 to 2017 by country. Data is from the German administrative panel data and, for the rest of the sample, the combined Orbis/Amadeus data.

| | Mean | Std.Dev. | p25 | Med. | p75 | n |
|---|---|---|---|---|---|---|
| Panel A:<br>Employment Levels | | | | | | |
| BE | 130.18 | 342.29 | 30 | 46 | 106 | 2766 |
| DE | 177.03 | 1522.46 | 37 | 61 | 130 | 32706 |
| FI | 172.58 | 517.93 | 32 | 55 | 124 | 1509 |
| FR | 391.63 | 4424.69 | 37 | 64 | 151 | 6725 |
| IT | 94.12 | 383.90 | 28 | 40 | 74 | 21675 |
| PT | 81.60 | 166.23 | 28 | 41 | 76 | 5273 |
| SE | 206.65 | 1572.16 | 29 | 46 | 101 | 2987 |
| Panel B:<br>Employment Growth | | | | | | |
| BE | 1.53 | 8.73 | -1.79 | 1.20 | 4.55 | 2766 |
| DE | 1.28 | 6.94 | -2.08 | 1.02 | 4.40 | 32706 |
| FI | 2.18 | 26.15 | -3.60 | 0.00 | 4.77 | 1509 |
| FR | 1.33 | 21.42 | -2.93 | 0.00 | 3.59 | 6725 |
| IT | 2.80 | 11.16 | -1.41 | 1.64 | 5.80 | 21675 |
| PT | 3.93 | 12.43 | -0.78 | 2.54 | 7.10 | 5273 |
| SE | 2.64 | 12.03 | -2.13 | 1.53 | 6.27 | 2987 |

**Table 2:** Differences in Churn

Table displays mean churn rate, death rate, and birth rate for Germany compared to the rest of sample (Belgium, Finland, France, Italy, Portugal, and Sweden). Observations are at the industry-year level for the years 2014 through 2020. Industry refers to the most dis-aggregated NACE Rev. 2 classification observation with churn data provided by Eurostat for each NACE Rev. 2 sector. Rates are as a share of the population of respective firms. Churn is simply the combination of the death rate and birth rate.

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. | Difference | t |
| Churn Rate | 0.013 | 0.011 | 0.022 | 0.030 | 0.009*** | (11.054) |
| Death Rate | 0.005 | 0.005 | 0.010 | 0.017 | 0.005*** | (12.062) |
| Birth Rate | 0.008 | 0.008 | 0.012 | 0.024 | 0.004*** | (6.088) |
| Observations | 374 | | 2550 | | 2924 | |

# Figures

**Figure 1a:** Distribution of Firm Growth - Shares

Grow rates are the average annual employment growth rates over the period 2014 to 2017 for all manufacturing and mining firms with at least 20 employees.

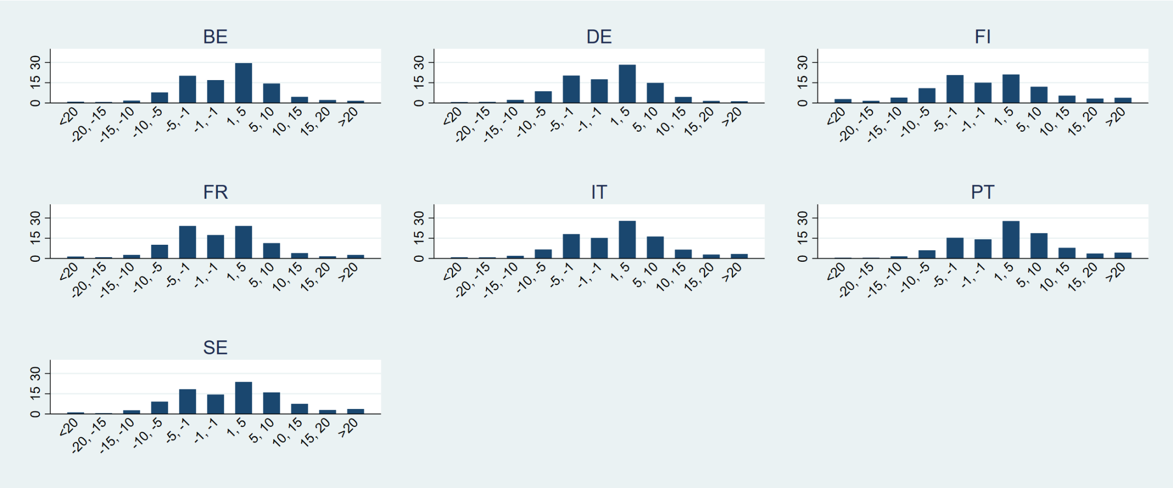



**Figure 1b:** Share of Rapidly Growing and Rapidly Shrinking Firms

Rapidly growing firms are those with average employment growth rates of 20% or more per year. Rapidly shrinking firms are those whose employement declines 20% or more per year.

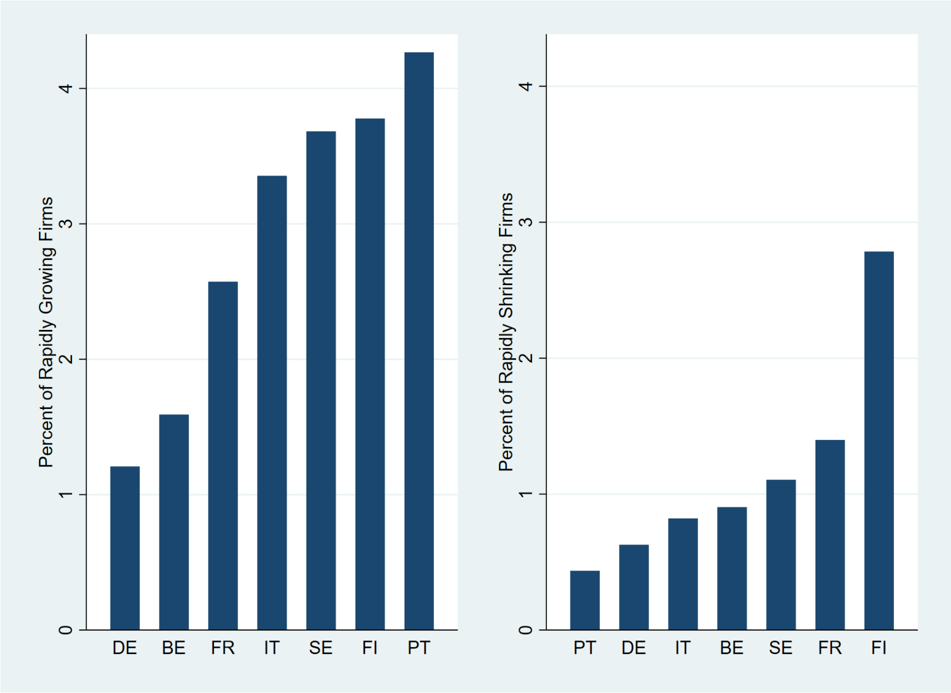


**Figure 1c:** Growth Distribution in Germany Relative to Rest of Sample

Graph is equivalent to 1a with Germany demeaned by the rest of the sample. Vertical axis reported in percentage.

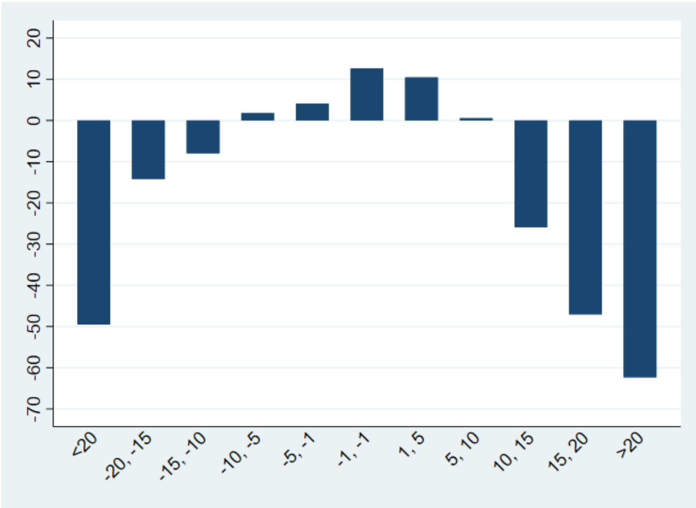



**Figure 2a:** Distribution of Firm Growth - Shares (Small Firms)

Grow rates are the average annual employment growth rates over the period 2014 to 2017 for manufacturing and mining firms with less than employees as of 2014.

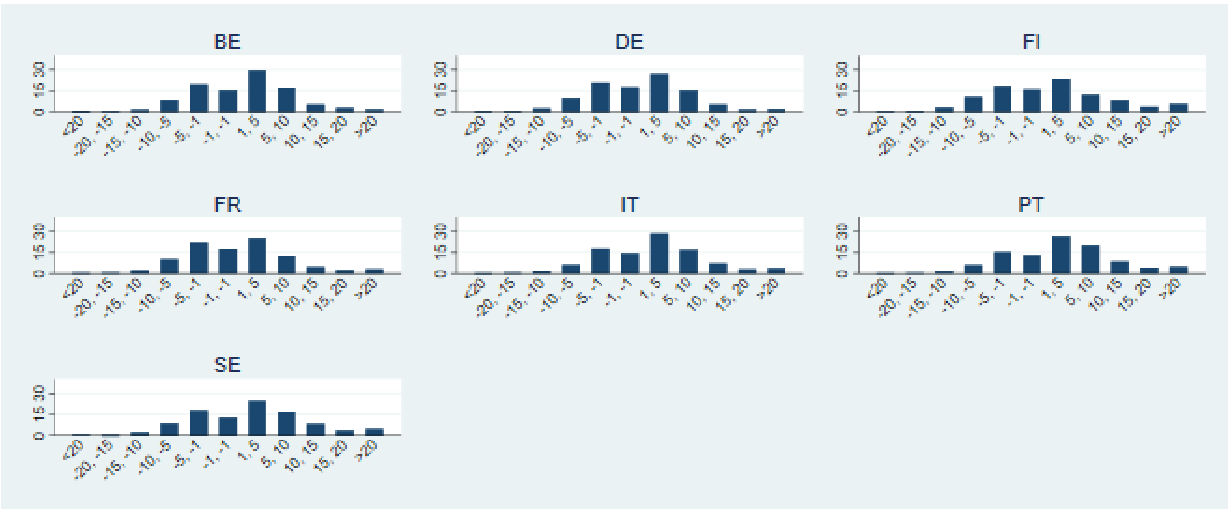


**Figure 2b:** Share of Rapidly Growing and Rapidly Shrinking Firms (Small Firms)

Rapidly growing firms are those with average employment growth rates of 20% or more per year. Rapidly shrinking firms are those with average employment declines of 20% or more.

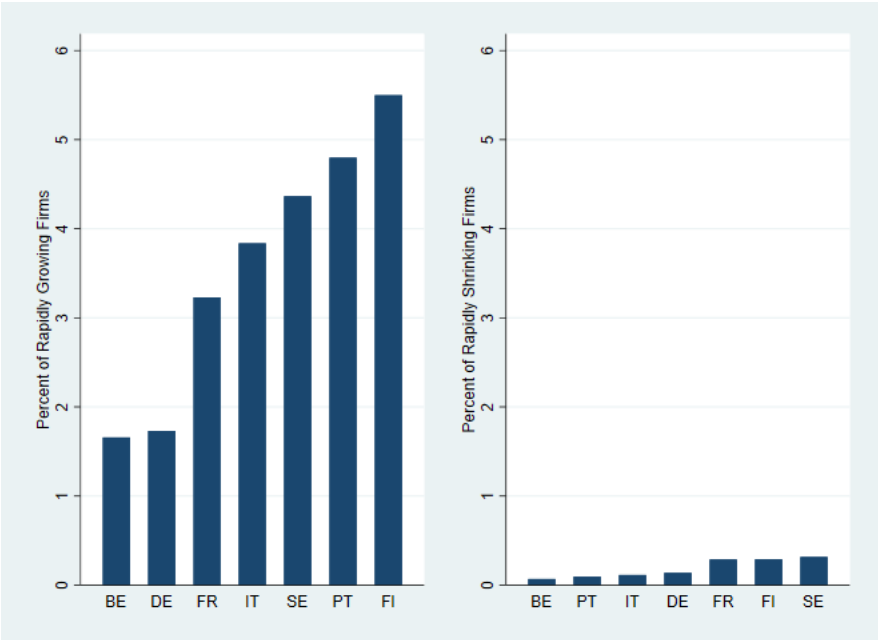



**Figure 2c:** Distribution of Firm Growth - Shares (Medium-Size Firms)

Grow rates are the average annual employment growth rates over the period 2014 to 2017 for manufacturing and mining firms with 50 to 249 employees as of 2014.

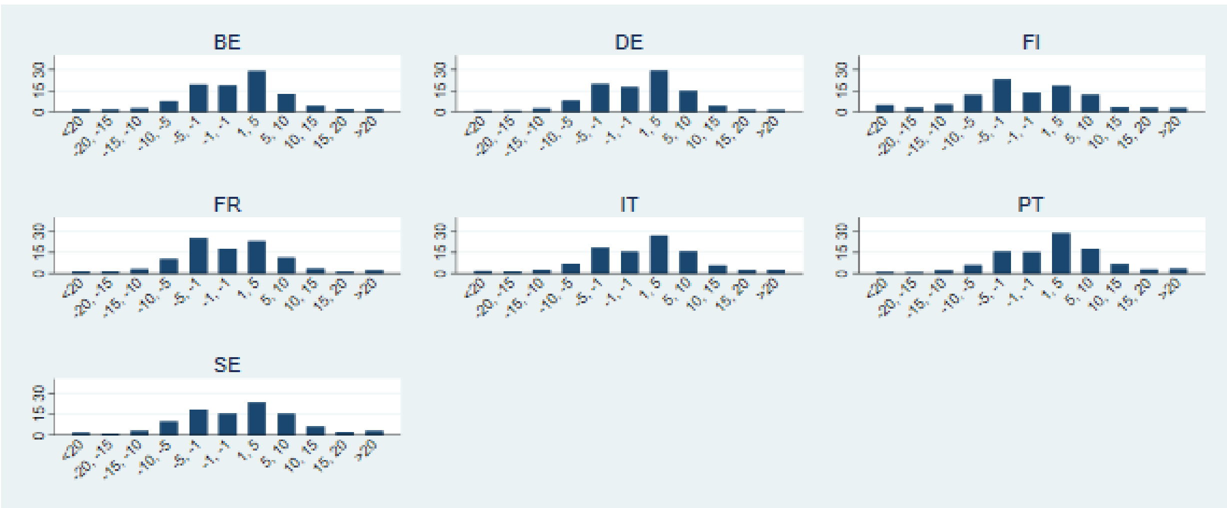


**Figure 2d:** Share of Rapidly Growing and Rapidly Shrinking Firms (Medium-Sized Firms)

Rapidly growing firms are those with average employment growth rates of 20% or more per year. Rapidly shrinking firms are those with average employment declines of 20% or more. Grow rates are the average annual employment growth rates over the period 2014 to 2017 for manufacturing and mining firms with 50 to 249 employees as of 2014.

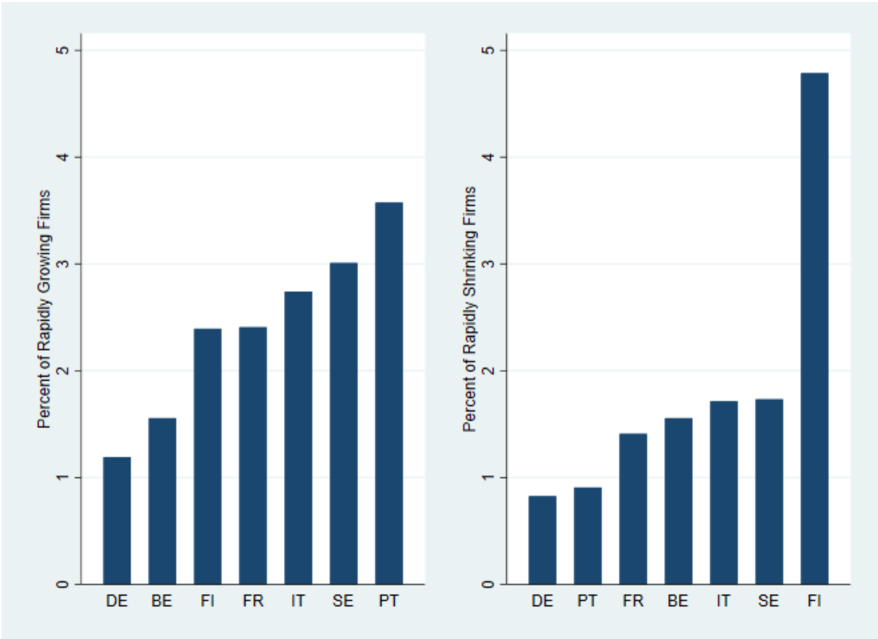



**Figure 2e:** Distribution of Firm Growth - Shares (Large Firms)

Grow rates are the average annual employment growth rates over the period 2014 to 2017 for manufacturing and mining firms with 250 employees or more as of 2014.

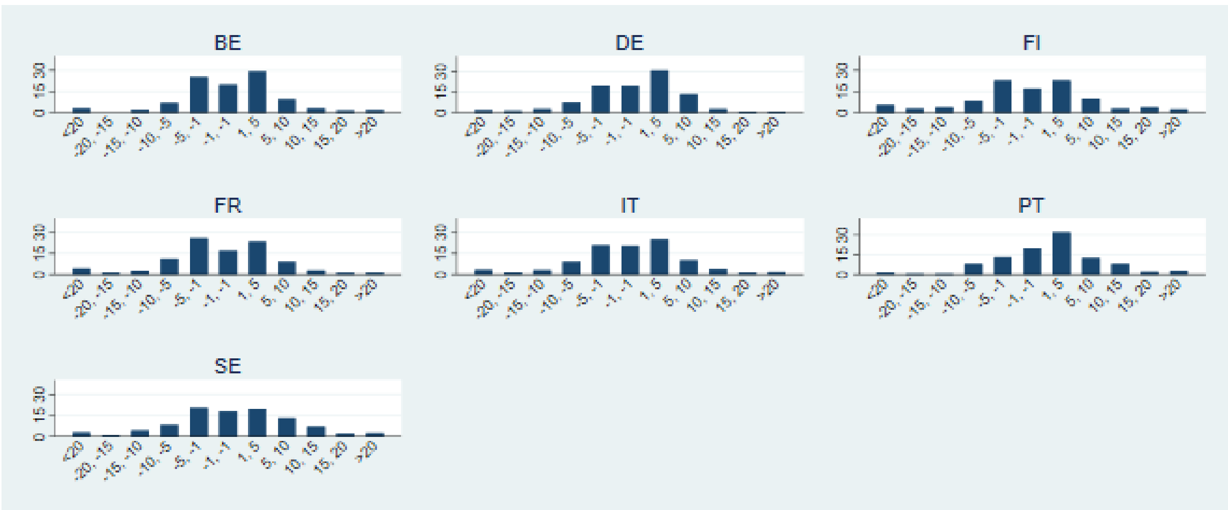


**Figure 2f:** Share of Rapidly Growing and Rapidly Shrinking Firms (Large Firms)

Rapidly growing firms are those with average employment growth rates of 20% or more per year. Rapidly shrinking firms are those with average employment declines of 20% or more. Grow rates are the average annual employment growth rates over the period 2014 to 2017 for manufacturing and mining firms with 250 employees or more as of 2014.

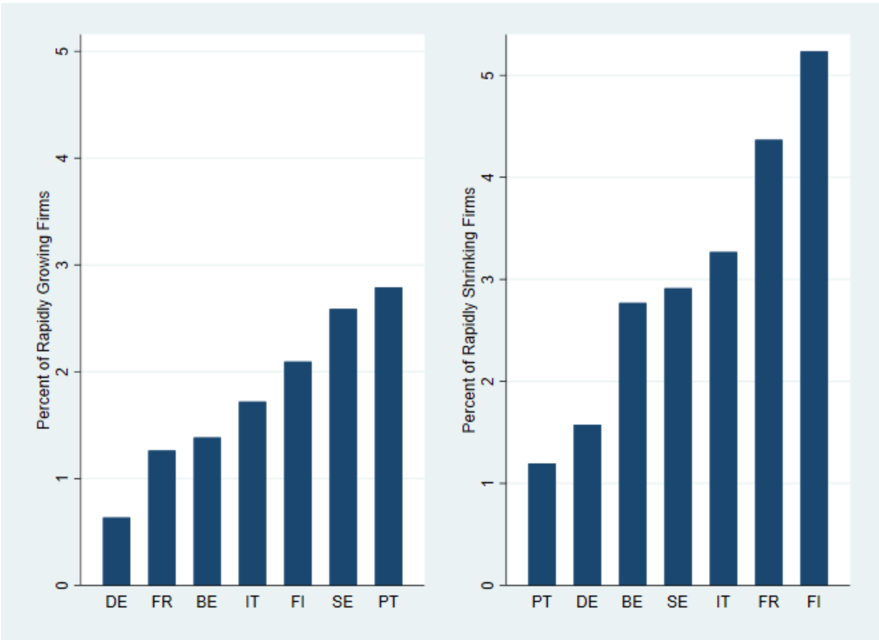


**Figure 3a:** Birth Rate, Death Rate, and Churn for the Manufacturing Sector

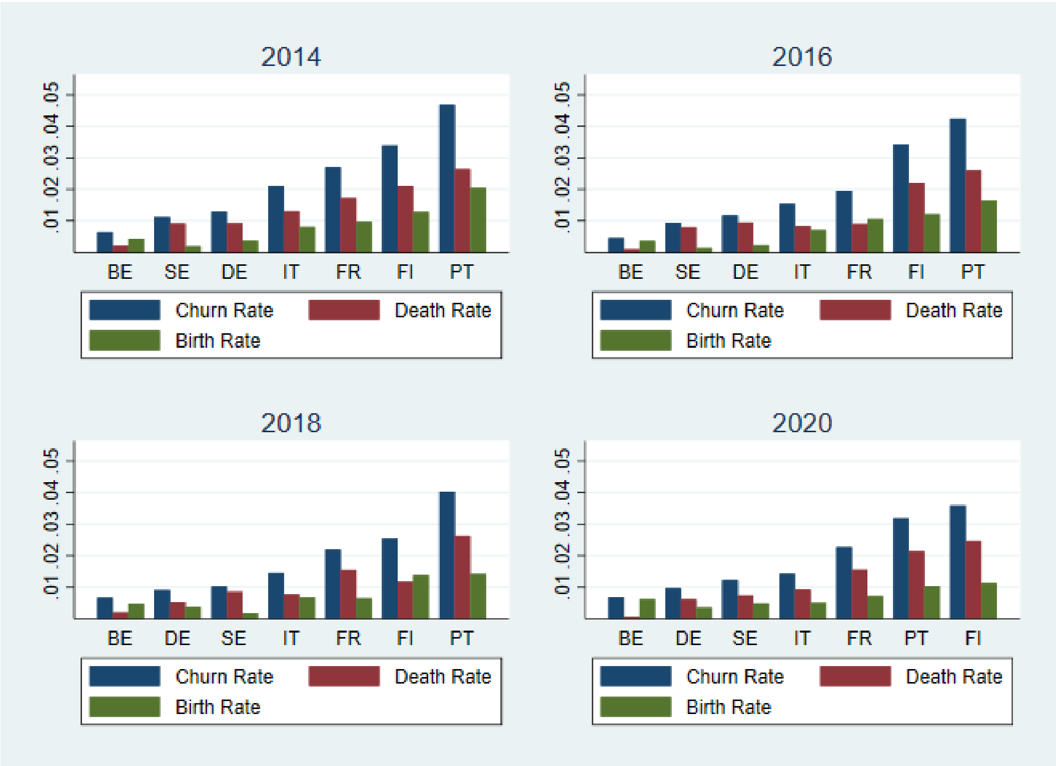
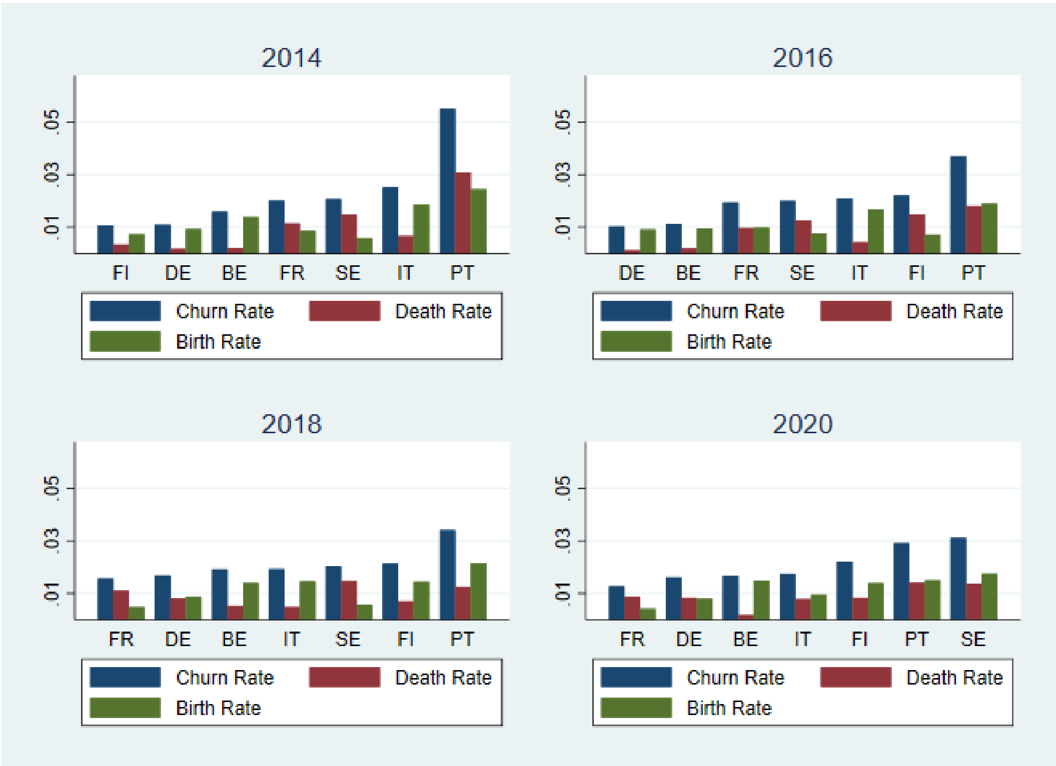


**Figure 3b:** Birth Rate, Death Rate, and Churn for other Sectors



# A Appendix Figures

**Figure A.1:** Share of Firms with Exactly Zero Growth

Figures displays the share of firms with exactly zero growth over over 2014 to 2017. Bottom two graphs displays the share among stagnant firms, defined as those in the [-1%;1) growth interval. Right two graphs display the share among large firms, defined as those with 250 employees or more as of 2014.

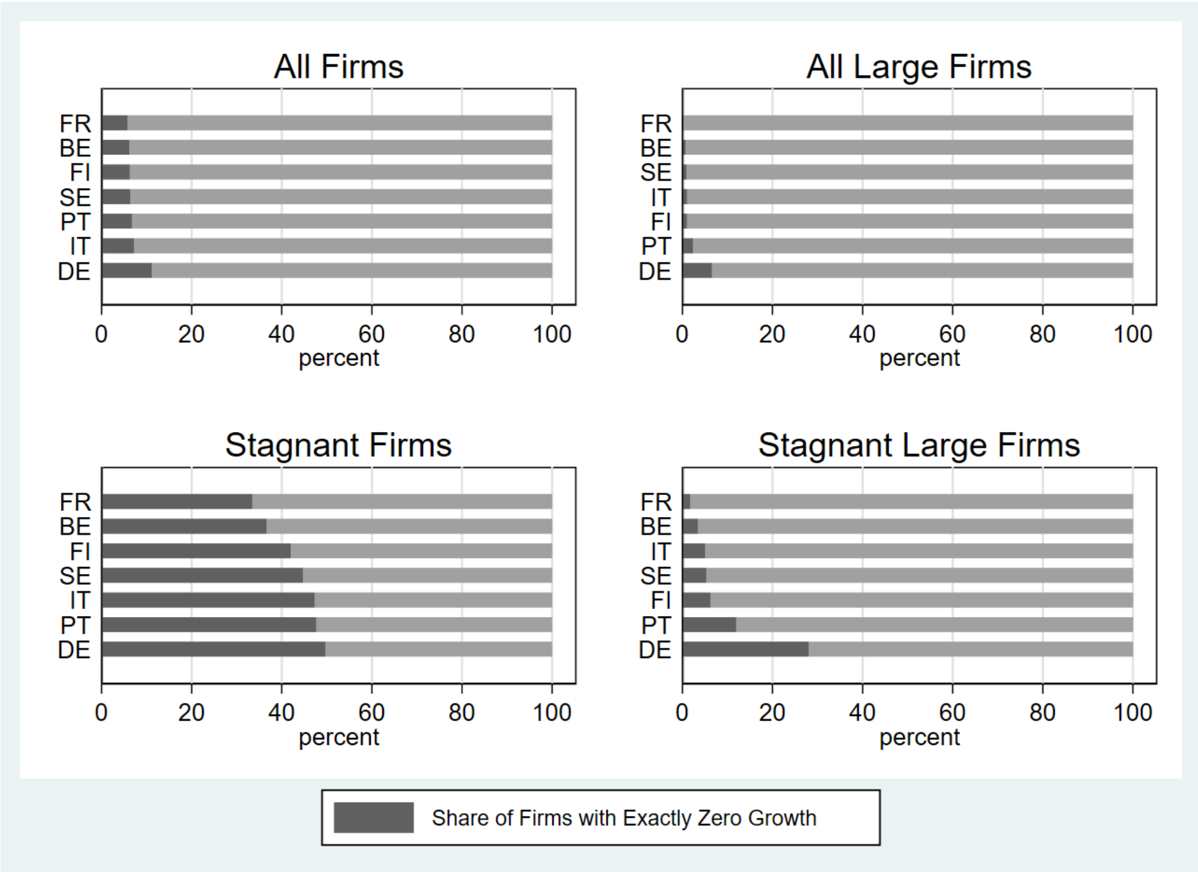


**Figure A.2a:** Labor Market Score

The figure displays average labor market stringency score over 2014 through to 2017 of each country. The index is a composite of a number of regulatory indicators collected from the World Bank and World Economic Forum by the Fraser Institute's Economic Freedom of the World database. Higher scores indicate more stringent labor market regulations. Data downloaded from the OECD SPIDER database.

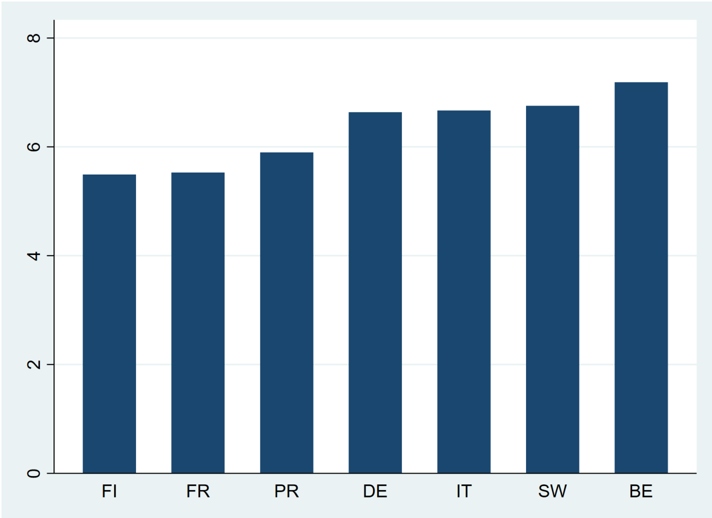



**Figure A.2b:** Insolvency Score

The figure displays average insolvency score over 2014 through to 2017 of each country. The index is based on World Bank surveys with insolvency practitioners on the time and costs associated with insolvency proceedings. The indicator is presented as a distance to frontier with the frontier defined as a score of 100 and higher scores being closer to the frontier. Data downloaded from the OECD SPIDER database.

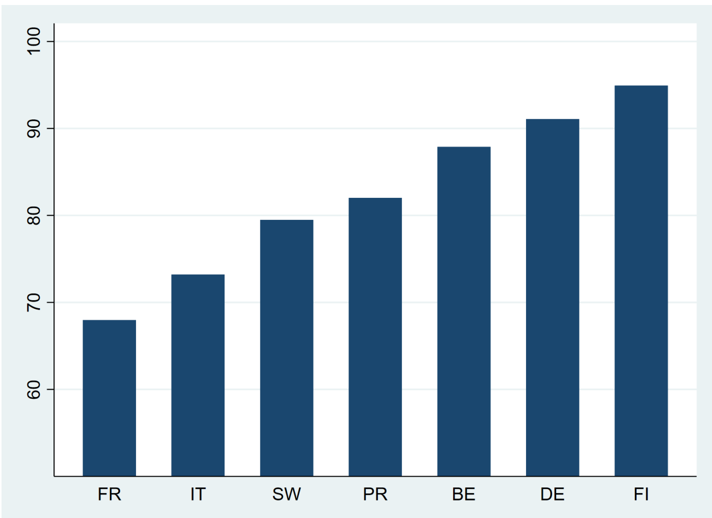


**Figure A.2c:** Domestic Credit to GDP (%)

The figure displays the average domestic bank credit to GDP and is originally sourced from the World Bank Development Indicators. Data downloaded from the OECD SPIDER database.

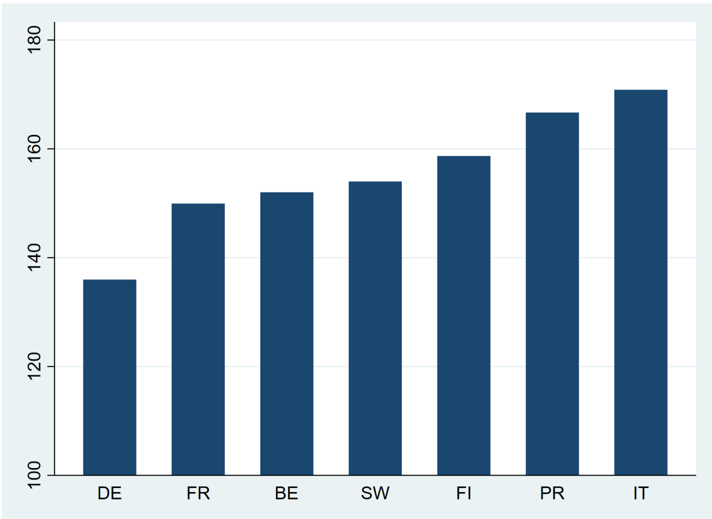


**Figure A.3:** Most Pressing Problem - Survey Results (%)

The figure displays survey results on the percentage of firms agreeing that the item is a "most pressing problem" from the Survey on the Acess to Finance of Enterprises. Individual country results are only publicly available for Germany, France, Spain, and Italy. The EU refers to all EU firms excluding Germany in the sample. All EU countries are included in the SAFE sample except Estonia, Cyprus, Latvia, Lithuania, Luxembourg, Malta and Slovenia.

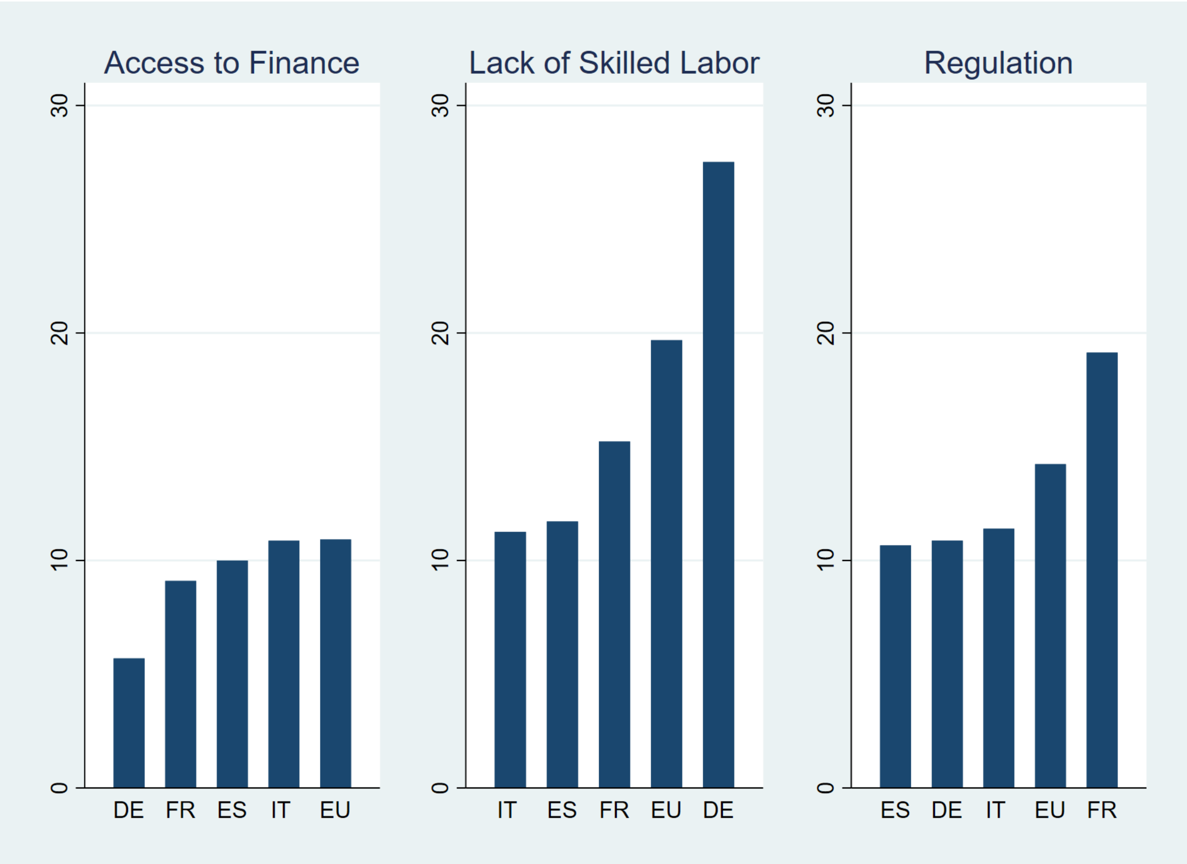


**Figure A.4:** Average Unemployment Rate (%) over 2014 to 2017

The figure displays the average unemployment rate over 2014 to 2017 for each country in the sample. Data is from Eurostat.

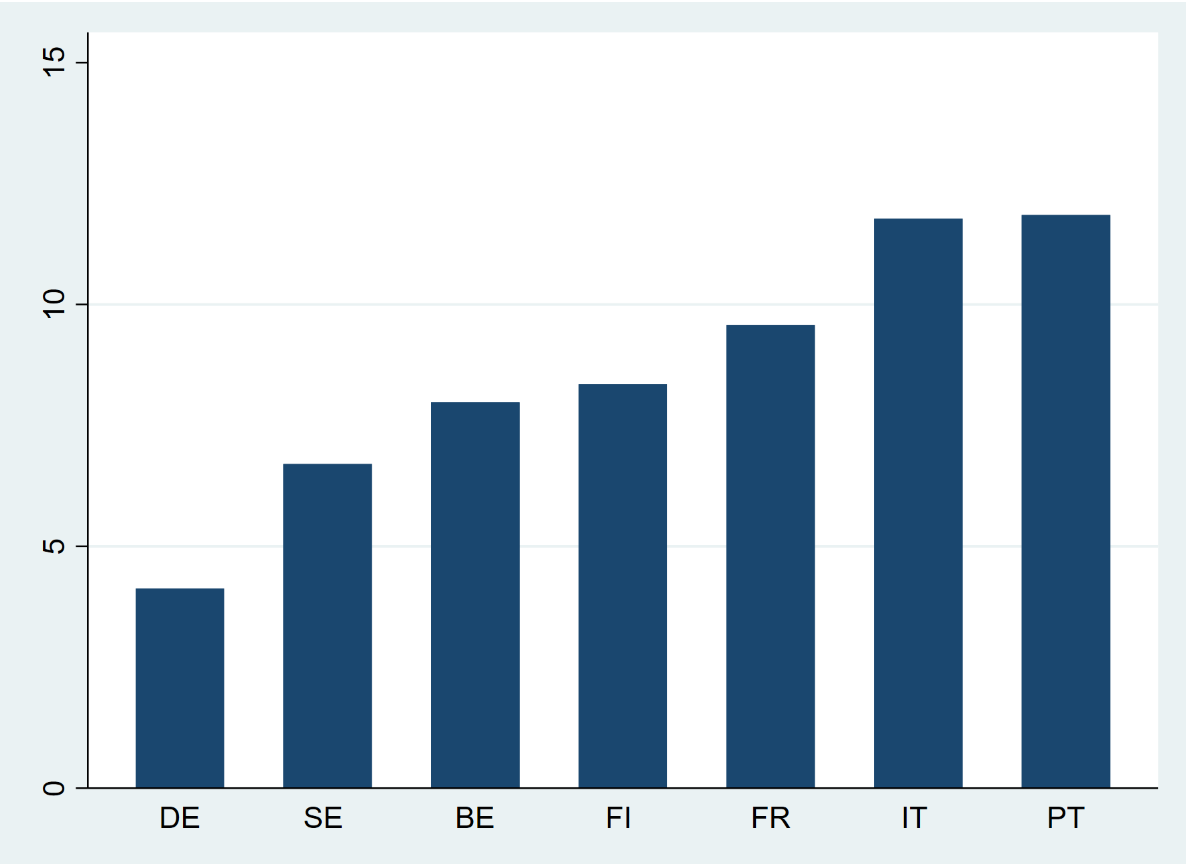


**Figure A.5:** Average Total Factor Productivity Growth (%) over 2014 to 2017

The figure displays total factor productivity growth from 2014 to 2017 for each country in the sample. Data is from the Penn World Table and used the $RTFP^{NA}$ as suggested in Feenstra, Inklaar, and Timmer (2015) for calculating changes in TFP over time.

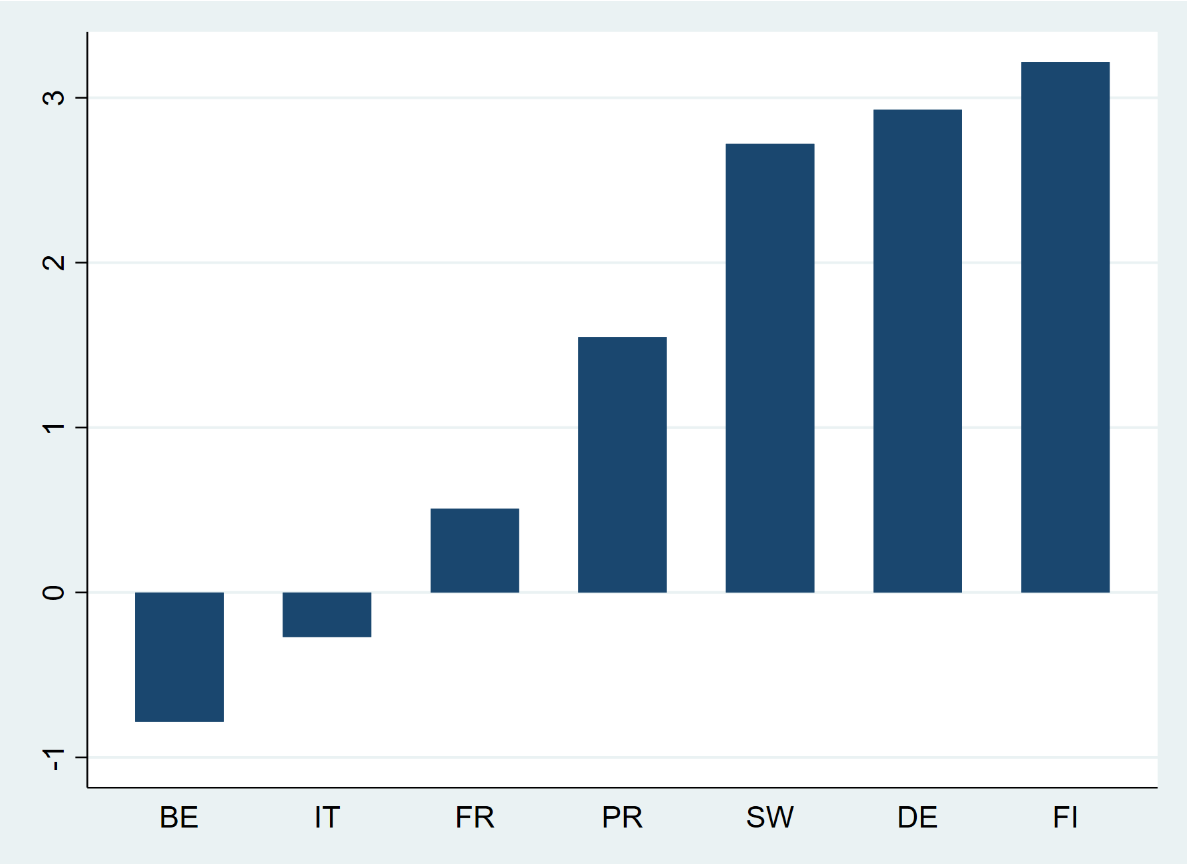


**Figure A.6:** Total Exports as a share of GDP (%)

The figure displays total exports as a share of GDP in percent using Eurostat Data.

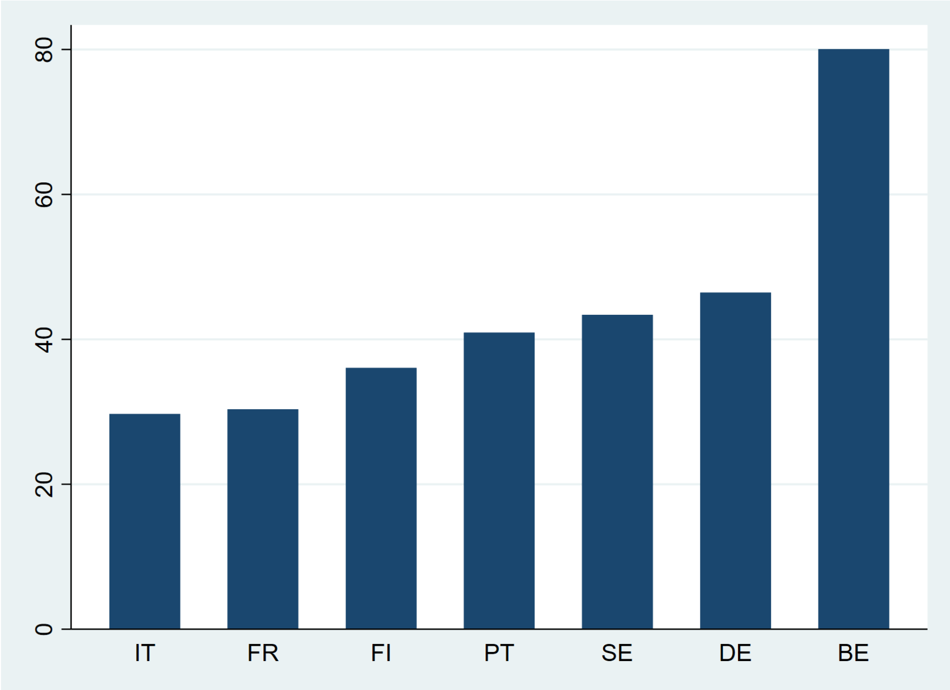
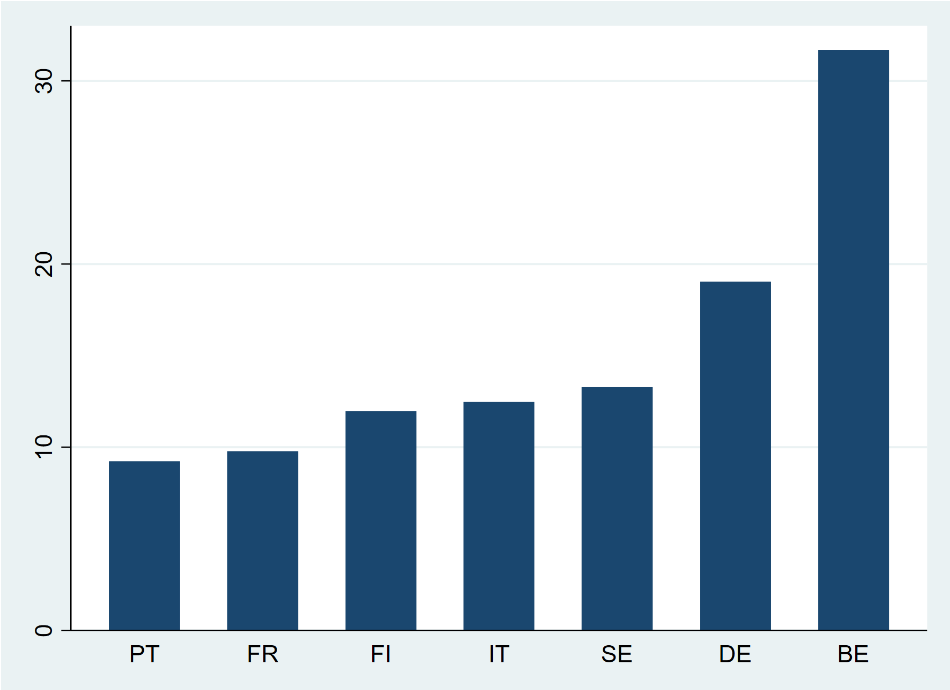



**Figure A.7:** Extra-EU Exports as a share of GDP (%)

The figure displays total exports to non-EU countries as a share of GDP in percent using Eurostat Data.



# B Appendix Tables

**Table B.1:** Weighted Churn Regressions

Table displays simple weighted OLS results with the birth rate, death rate, and churn rate as outcome variables. DE Dummy is equal to one if the country is Germany and zero otherwise. Observations are at the industry-year level. Columns (1), (3), and (5) are weighted by the employment of the respective industry-years and columns (2), (4), and (6) are weighted by the number of firms of the respective industry years. The sample period runs from 2014 to 2020. Industry refers to the most dis-aggregated NACE Rev. 2 classification observation with churn data provided by Eurostat for each NACE Rev. 2 sector. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1) Birth Rate | (2) Birth Rate | (3) Death Rate | (4) Death Rate | (5) Churn Rate | (6) Churn Rate |
|---|---|---|---|---|---|---|
| DE Dummy | -0.001** | -0.001** | -0.004*** | -0.004*** | -0.005*** | -0.005*** |
| | (0.001) | (0.001) | (0.000) | (0.000) | (0.001) | (0.001) |
| Constant | 0.008*** | 0.008*** | 0.007*** | 0.007*** | 0.016*** | 0.015*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.001) | (0.001) |
| N | 2924 | 2924 | 2914 | 2924 | 2914 | 2924 |
| $R^2$ | 0.005 | 0.005 | 0.072 | 0.086 | 0.037 | 0.037 |

**Table B.2:** Weighted Churn Regressions with Industry and Year Fixed Effects

Table displays simple weighted OLS results with the birth rate, death rate, and churn rate as outcome variables with industry and year fixed effects. DE Dummy is equal to one if the country is Germany and zero otherwise. Observations are at the industry-year level. Columns (1), (3), and (5) are weighted by the employment of the respective industry-years and columns (2), (4), and (6) are weighted by the number of firms of the respective industry years. The sample period runs from 2014 to 2020. Industry refers to the most dis-aggregated NACE Rev. 2 classification observation with churn data provided by Eurostat for each NACE Rev. 2 sector. Standard errors are clustered at the industry level. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1)<br>Birth Rate | (2)<br>Birth Rate | (3)<br>Death Rate | (4)<br>Death Rate | (5)<br>Churn Rate | (6)<br>Churn Rate |
|---|---|---|---|---|---|---|
| DE Dummy | -0.001 | -0.001 | -0.003*** | -0.004*** | -0.004*** | -0.005*** |
| | (0.001) | (0.001) | (0.000) | (0.001) | (0.001) | (0.001) |
| Constant | 0.008*** | 0.008*** | 0.007*** | 0.007*** | 0.016*** | 0.015*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| N | 2924 | 2924 | 2914 | 2924 | 2914 | 2924 |
| Adjusted $R^2$ | 0.446 | 0.446 | 0.277 | 0.318 | 0.410 | 0.426 |

# Paper 3: R&D Tax Credits and the Acquisition of Startups

# R&D Tax Credits and the Acquisition of Startups*

William McShane† Merih Sevilir‡

This Draft: August 7, 2023

**Abstract**

We propose a novel mechanism through which established firms contribute to the startup ecosystem: the allocation of R&D tax credits to startups via the M&A channel. We show that when established firms become eligible for R&D tax credits, they increase their R&D and M&A activity. In particular, they acquire more venture capital (VC)-backed startups, but not non-VC-backed firms. Moreover, the impact of R&D tax credits on firms' R&D is increasing with their acquisition of VC-backed startups. The results suggest that a key strategy for these firms to augment their R&D efforts is by acquiring startups rather than solely focusing on increasing their R&D intensity in-house. We also highlight evidence that startups do not appear to benefit from R&D tax credits directly, perhaps because they typically lack the taxable income necessary to directly benefit from the tax credits. In this context, established firms can play an intermediary role by acquiring startups and reallocating R&D tax credits, effectively relaxing the financial constraints faced by startups.

**JEL Classification: G34, O3, M13**

**Keywords:** R&D, M&A, startups, venture capital, tax credits

*A previous version of this paper has been published as IWH Discussion Paper No. 15/2023.

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# 1 Introduction

Established firms play an important role in the creation and progress of entrepreneurial startups. They provide capital to early-stage startups in the form of corporate venture capital and to later stage startups by acquiring them.

In this paper, we propose another novel role of established firms in the creation and growth of entrepreneurial startups. We present evidence that established firms may play a role in allocating R&D tax credits to startups. Existing work focuses on how R&D tax credits facilitate R&D and innovation at the firm- and geographic-level (Wu, 2005; Wilson, 2009; Moretti and Wilson, 2014; Guceri and Liu, 2019), and document important spillovers from R&D to other firms (Bloom, Schankerman, and Van Reenen, 2013; Lucking, Bloom, and Reenen, 2019; Babina and Howell, 2018). In this paper, we examine a novel mechanism through which established firms take advantage of R&D tax credits by acquiring startups.

When an established firm becomes eligible for R&D tax credits, the firm may take advantage of it by increasing its internal R&D spending. Given that a large majority of R&D expenses covers salaries and wages paid to R&D human capital (scientists, inventors, and engineers), one way for a firm to benefit from the credit is to hire R&D talent from the external labor market. Another potentially more efficient way of increasing R&D labor could be to acquire a startup. Different from mature and established firms, the most visible and predominant asset of a startup is its human capital, as in the team of people who work at the startup together. Hence, acquiring a startup might be a more efficient way of obtaining R&D labor than hiring R&D workers from the external labor market given that startups represent a team of people working together. Moreover, existing studies show that large, bureaucratic, and hierarchical firms exhibit lower R&D and innovation productivity because they are less attractive to entrepreneurial-minded employees, scientists and inventors. Small entrepreneurial startups, on the other hand, have greater innovation productivity (Bernstein, 2015; Tåg, Åstebro, and Thompson, 2016; Schnitzer and Watzinger, 2022). In short, an active M&A market for entrepreneurial startups may enhance established firms' ability to benefit from R&D tax credits by acquiring them. Hence, it is plausible to expect that the level of M&A activity by established firms should increase subsequent to a tax-induced decrease in their user cost of R&D.

In contrast to established firms, startups typically lack the taxable income necessary to benefit from R&D tax credits (Bankman and Gilson, 1999). This limitation prevents startups from directly capitalizing on these tax incentives and, as a result, may constrain their growth and innovation capabilities. Hence, it is plausible to expect that established firms may play an intermediary role in allocating R&D tax credits to small startups by acquiring them and by relaxing their financial constraints (Erel, Jang, and Weisbach, 2015). Put differently, established firms may increase the efficiency of R&D tax credits by reallocating them to R&D intensive startups that arguably need R&D capital the most while at the same time have the lowest ability to access it.

Consistent with these arguments, we find that established firms respond to plausibly exogenous tax-based R&D incentives by increasing both their R&D and M&A expenditures. In particular, when the tax-component of the user cost of R&D capital declines, established firms increase their acquisition of VC-backed startups, but not of other non-VC-backed companies. Specifically, we find that a one standard deviation increase in the user cost of R&D capital is associated with a 10.6% lower expected count in the acquisition of VC-backed targets. Importantly, we find that the user cost of R&D capital exhibits no relationship with the acquisition of non-VC-backed targets.

These results suggest that a key strategy for these firms to augment their R&D efforts is by acquiring R&D-intensive startups rather than solely focusing on increasing their R&D intensity in-house. In line with this interpretation, we find that the impact of R&D tax incentives on R&D is increasing with the number of VC-backed startups acquired by the firm.

Consistent with the idea that startups lack the income to benefit from R&D tax credits directly, a number of papers have found that state R&D tax credits have no impact on startups and, more generally, small firms. For example, Lucking (2018) and Curtis and Decker (2018) find no impact on employment at new firms in states passing R&D tax credits. Similarly, Babina and Howell (2018) find no impact on aggregate firm entry. We further substantiate these findings by examining the relationship between changes in the tax price of R&D induced by state tax credits and venture capital activity, which serves as a proxy for VC-backed startup activity. Our analysis reveals no discernible relationship between the two. The fact that startups do not appear to directly benefit from these tax credits suggests an even greater importance of the reallocative effect of the established firms R&D tax credit induced M&A activity. We present evidence that startups and the startup ecosystem benefit from R&D tax credits largely through the M&A activity of established firms.

Our paper makes several novel contributions. First, it proposes a novel re-allocative role for M&As through which R&D tax credits are reallocated from established firms to small startups. This evidence suggests a new positive role of established firms on the creation and growth of small startups. Second, the efficiency of R&D tax credits could be greater in economies with an active market for corporate control with lower barriers and frictions to conduct M&As. In economies with less developed and liquid market for corporate control, R&D tax credits could be less effective as the search costs and frictions associated with finding R&D human capital in the external labor market may reduce firms' incentives to benefit from such credits. In addition, antitrust regulations making it harder for established firms to acquire smaller startups might have a negative impact on the efficiency of R&D tax credits. Third, R&D tax credits may contribute to the creation of new firms and startups as they result in M&A capital for entrepreneurs, inventors and scientists. Prior work by Phillips and Zhdanov (2013) suggest greater M&A activity expands the incentives for small firms to engage in R&D, suggesting an indirect channel through which state tax credits may enhance small firm R&D even if the firms cannot directly benefit from the R&D tax credits.

Beyond the literature on the economic impacts of R&D tax credits, this paper also contributes to the literature on the interaction between innovation and M&A. Seru (2014) finds that merged parties produce fewer citation-weighted patents following acquisitions and interprets this as evidence that M&A stifles innovation. Phillips and Zhdanov (2013) presents evidence that large firms acquire innovation from R&D intensive targets and argues that established firms' R&D may optimally decline with M&A. Bena and Li (2014) find that low-R&D firms tend to be acquirers and R&D-intensive targets tend to be targets. A common theme among these papers is the argument that established firms acquire the target's innovation, but do not acquire to pursue further innovation. These studies share a focus on the acquisitions of publicly-listed targets.

In contrast, our results suggest that, in response to plausibly exogenous declines in the tax price of R&D, firms acquire startups to increase their innovation activity. We find that firms increase the acquisition of VC-backed startups when faced with lower R&D costs and that R&D is increasing in the number of VC-backed startups acquired. In this context, as opposed to acquiring startups either for their existing inventions or to strategically terminate the target's invention (Cunningham, Ederer, and Ma, 2021), firms facing reduced R&D costs acquire startups in order to pursue more innovation activity.

## 2 Data

**Compustat** We use Compustat to observe acquirer characteristics. We remove all finance, real estate, and utility companies and restrict the sample to US headquartered firms. Further, we drop all observations with negative asset, R&D, and capital expenditure values.

**R&D Tax Price Data** We exploit variation in the tax-price of R&D introduced by tax credits, depreciation allowances, and income taxes. Consider the adapted Wilson (2009) extension of the Hall-Jorgenson user cost of capital formula for R&D (per dollar invested):

$$\rho_{it} = \frac{1 - D_{it}}{1 - \tau_{it}}[r_t + \delta] \tag{1}$$

where $D_{it}$ is the effective value of tax credits and depreciation allowances, $\tau_{it}$ is the rate of corporate income tax, $r_t$ is the real interest rate, and $\delta$ is the depreciation rate of R&D capital. Since $r_t$ and $\delta$ are assumed to not vary across firms, the approach focuses on the tax price component of the user cost, $\rho^t_{it} = \frac{1-D_{it}}{1-\tau_{it}}$.

The tax price can be thought of as having a federal and state tax price. The federal tax price, $\rho^F_{ft}$, is firm-specific because it varies with the firm's age, previous R&D spending, sales, taxable profit, and when the firm first had qualifying R&D expenditure. Each of these components then interacts with policy changes in the federal R&D credit rate, deduction rules, and the corporate tax rate. Our calculation of $\rho^F_{ft}$ follows that of Bloom, Schankerman, and Van Reenen (2013).

The state-level R&D tax price, $\rho^S_{s,t}$, takes into account state-level R&D tax credits, depreciation allowances and corporate taxes. Estimates of the state-level R&D tax price comes from Wilson (2009). State R&D credits were introduced gradually over time and interact with changes in state corporate tax rates. Lucking (2018) extends this data through 2015, which we adjust to use the same Hall-Jorgenson user cost of R&D capital formula employed by Wilson (2009).

To bring $\rho^S_{s,t}$ to the firm level, we follow (Bloom, Schankerman, and Van Reenen, 2013) and (Lucking, Bloom, and Reenen, 2019) by using the share of the firm's patents inventors located in each state $s$ as weights. Data on firm patents comes from the NBER Patent Data Project, which includes data from the U.S. Patent and Trademark Office matched to gvkey (see Hall, Jaffe, and Trajtenberg (2001) for details).[1] We denote this firm-specific term $\rho^S_{i,t}$.

[1]The NBER Patent Data Project goes until 2006. We fix shares in 2006 through 2015. This limits the sample to firms that issued at least one patent between 1990 and 2006.

State R&D tax credits are arguably quasi-exogenous to the firm. Bloom, Schankerman, and Van Reenen (2013) find no relationship between state economic variables and R&D tax credit adoption. Miller and Richard (2010) investigate the drivers of R&D tax credits and find that more manufacturing-intensive states and one-party control of state government predict the adoption of R&D tax credits.

We then take the mean of $\rho^F_{i,t}$ and $\rho^S_{i,t}$ for each firm to get a measure of the tax price of R&D faced by the firm, which we refer to as $\rho^S_{i,t}$.

**VentureXpert** To identify M&A transactions involving VC-backed startups and measure aggregate state-level VC volumes, we use VentureXpert as provided by Refinitiv Eikon under the Private Equity Screener. Kaplan and Lerner (2017) demonstrates that of VC databases, VentureXpert has the best coverage of VC investments for our sample period. For measuring aggregate state VC activity, we define VC investments as any VC investment in a single company, regardless of whether or not there were multiple VC investors attached to the deal.

**Bureau of Economic Analysis** For state-level regressions, we include growth in GDP and log personal income from the Bureau of Economic Analysis.

**SDC Platinum:** We use SDC Platinum, also known as Eikon or Thomson Reuters Mergers and Acquisitions Data, to observe the number of acquisitions conducted by each firm. We filter transactions to completed deals in which the acquirer took at least a 50% stake in the target and owned at least 90% of the target following the transaction.[2]

That most M&A transactions do not disclose transaction values poses a challenge to measuring firms' aggregate acquisition activity. This problem is particularly acute in our setting, as we are interested in the acquisition of private VC-backed startups, which are unlikely to be subject to FTC and SDC reporting requirements (Wollmann, 2019). Beyond examining the count of acquisitions, researchers typically take one of the following approaches when measuring acquisition activity: Measure the total cash flow to acquisitions from Compustat, which has the disadvantage that it ignores stock offers or, alternatively, aggregate observable deal values in SDC for each acquirer, which ignores unreported transaction values and thereby a majority of M&A transactions (Netter, Stegemoller, and Wintoki, 2011). [3]

[2] We also exclude repurchase deals, stake purchase deals, self-tender deals, buybacks, acquisitions of partial interest, exchange offers, and recapitalizations.

[3] Barrios and Wollmann (2022) find that around 29.5% of the aggregate volumes of M&A conducted by publicly-listed firms is unreported in SDC.

To circumvent this problem, we create a measure of total acquisition expenditure following an approach similar to Barrios and Wollmann (2022). This approach relies on two components. First, publicly listed firms are required to disclose the total annual cash flow associated with M&A activities[4]. This allows us to capture both the undisclosed and disclosed cash expenditure on acquisitions. Second, because issuing stock involves considerable fixed expenses, acquisitions involving stock issuance are likely to be sufficiently large to trigger the SEC's mandatory reporting thresholds for transactions material to investors.[5] Hence, the large majority of transactions involving stock issuance should be disclosed and thereby observable in SDC Platinum. Accordingly, we collapse the stock value reported in SDC Platinum to the firm-year level and combine this with Compstat's aqc to obtain a measure of total acquisition spending of each acquirer in our sample.

[4]Reported as aqc is Compustat

[5]While thresholds vary depending on the type of transaction, the most important threshold for publicly-listed firms is whether the size of the transaction exceeds 10% of the acquirer's assets. If this threshold is reached, the acquirer must disclose the transaction value in an 8-K report.

# 3 Results

## 3.1 R&D Tax Price and M&A

Panel A of Table 1 presents summary statistics for our sample of publicly-listed firms. The sample consists of 3,587 unique firms over 25 years, amounting to 38,640 firm-year observations with coverage for all variables.[6]

In the average firm-year, 0.73 acquisitions are conducted. Approximately 16.4% of acquisitions are of VC-backed private companies. Interestingly, mean M&A expenditure is relatively similar in volume to that of R&D expenditure in the sample, at 104.5 million USD per year and 115.18 million USD per year, respectively. Average capital expenditure is larger than both, but not drastically so at 149.9 million USD per year. This suggests that inorganic growth is of similar importance to growth in tangible assets and intangible assets for established, publicly-listed firms. At the same time, only approximately 44% of firms have non-zero M&A expenditure in a given year.

We begin by estimating the impact of the firm's R&D tax price, $\rho^t$, on acquisition activity at the firm-level, with a particular eye to the acquisition of startups. As the variables explored in our firm-level regressions are either count or count-like data (both acquisition expenditure and R&D expenditure are heavily skewed with real zeros), we use Poisson regressions with firm and year fixed effects throughout.[7] We also control for the lag of various firm financials, such as firm size and cash holdings. Standard errors are two-way clustered at the state and year level.

Table 2 presents the impact of the tax price of R&D on the count of acquisitions. The first two columns indicate that the tax price of R&D capital has no statistically significant impact on the total number of acquisitions of the firm. Looking to the count of non-VC-backed companies acquired in columns 3 and 4, the coefficient on the tax price of R&D is even closer to zero and becomes positive with controls.

However, in Columns 5 and 6, we limit the outcome to the count of VC-backed companies acquired and find a negative and statistically significant coefficient on the R&D tax price.

[6]We limit the sample to firms with non-missing values for all of our main regressions. Because CAPX is only used in robustness tests, we do not limit the sample to firm-year observations with non-missing CAPX values.

[7]As a robustness check, we rerun all fully saturated firm-level Poisson regressions using log-linear models for outcome variables that are not count data in Table A2. The change in specification does not meaningfully change the results.

The economic magnitude of this coefficient is large. Column 6 indicates that a one standard deviation change in the tax-based user cost of R&D capital is associated with approximately a 10.6% lower expected count of acquisitions of VC-backed firms. At approximately 11.2% of a standard deviation in the count of VC-backed companies acquired, this suggest the R&D tax price is an economically important driver of the acquisition of VC-backed firms.

These results suggest that in response to a reduction in the tax price of R&D, firms acquire startups to increase their R&D activity. That we find no evidence of an increase in the acquisition of companies not backed by VC suggests that this is unique to the acquisition of startups.

In the appendix Table A3, we show that it makes little difference whether one examines the acquisition of in-state or out-of-state VC-backed targets. Coefficients are similar for both groups of targets. This suggests that M&A spillovers to startups from the R&D tax incentives of established firms are not geographically restricted. A likely explanation for this lack of geographical bias is that these acquirers are large, publicly-listed firms that operate nationally. Hence, these firms may have only a limited geographical bias for acquisitions in their head-quarter state. Moreover, our measured reduction in R&D tax price can come from any state in which the firm is patenting, which may not always coincide with the firm's head-quarter state.

We examine M&A expenditure as outcome variable in Columns 1 and 2 of Table 3. We find that increases in the tax-price of R&D reduce M&A expenditure. With controls, its expected value is reduced by 25.8% given a one standard deviation increase in the tax price of R&D. While seemingly very large, this is equivalent to 2.2% of a standard deviation in M&A spending.

For comparison, we look to R&D expenditure as an outcome variable in Columns 3 and 4 of Table 3. Intuitively, we find that increases in the tax-price of R&D reduce R&D. This result is in line with the literature finding that tax-based changes in the user cost of R&D drive R&D among established firms (see Babina and Howell (2018), Bloom, Schankerman, and Van Reenen (2013), and Lucking, Bloom, and Reenen (2019)) and speaks to the validity of our specification. In terms of economic magnitude, a one standard deviation increase in the tax price of R&D results in a decline in expected R&D spending of 13.0%, equivalent to 2.6% of a standard deviation in R&D expenditure. Interestingly, this is strikingly similar to the equivalent number when using M&A as an outcome variable of 2.2 percent. These results suggest that firms' acquisition activity and R&D activity respond similarly to tax-incentives for R&D.

So far, we have shown that established firms increase M&A activity in response to arguably tax-driven reductions in their user cost of R&D and that this M&A activity appears to be centered on acquisition of VC-backed startups. In Table 4, we investigate whether the impact of the tax-price of R&D, $\rho^t$, is moderated by the acquisition of VC-backed companies. First, we demonstrate in columns 1 through 3 that both the tax-price of R&D and the acquisition of VC-backed companies predict R&D spending. Interacting the tax-price of R&D with the natural log of the count of VC-backed acquisitions in columns 4 and 5, we find that the impact of the tax-price of R&D is increasing in the number of VC-backed companies acquired. This relationship is perhaps best communicated by Figures 1a and 1b, which plots the average marginal effects of each term along different values of the interaction term. One observes that the impact of acquiring VC-backed companies on R&D is highest for low values of $\rho^t$ and, vice versa, the impact of $\rho^t$ on R&D is larger the more VC-backed companies the firm acquires.

The presence of this interaction is consistent with an interpretation in which the acquisition of startups is a channel through which established firms scale up their R&D activity in response to reduce R&D costs. This speaks strongly for the idea that firms acquire startups to increase their R&D activity.

## 3.2 State R&D Tax Credits and Venture Capital

We continue by estimating the impact of within-state variation in R&D tax prices on aggregate VC volumes, as measured by both the count of VC deals and the aggregate volume of VC investments in millions of USD. Given that VC investments are heavily skewed and contain real zeros, we use poisson regressions. For a number of reasons, poisson regressions have been shown to be preferable to log-linear models for such outcomes (see, for example, Cohn, Liu, and Wardlaw (2022) and Santos Silva and Tenreyro (2006), among others).

Table 1 Panel B presents state-year level summary statistics. The average state in our sample had approximately 83.10 VC rounds in a given year and saw 678.13 million USD in VC investment. Across states, the average number of VC investments varies considerably, from 1.00 per year in Alaska to 1589.69 per year in California. Similarly, there is significant variation in R&D tax prices, with a minimum value of 1.03 and a maximum of 1.38.

We control for state macroeconomic conditions using growth in GDP and the log of personal income. Additionally, we include state and year fixed effects throughout. All standard errors are clustered at the state- and year-level.

Table 5 presents the results. Across all specifications, we observe no relationship between the R&D tax price and venture capital volume. The coefficient on the R&D tax price have the right sign for the count of VC rounds as an outcome variable in columns (1) and (2) but is not statistically distinguishable from zero with or without control variables. In columns (3) and (4), we examine the impact on total VC raised in millions of USD. Here the coefficient is positive and its confidence intervals are centered around zero.

Together, the results in Table 5 suggest that at the state-level, state R&D tax credits have no observable direct impact on venture capital activity. This is consistent with a number of papers finding no discernible impact of state R&D tax credits on small or young firms (see Lucking (2018), Curtis and Decker (2018), and Babina and Howell (2018)). That state-level changes in R&D tax credits appear to have no perceptible direct impact on startup activity can possibly be explained by the fact that, unlike in some Western countries such as Canada (Agrawal, Rosell, and Simcoe, 2020), firms are typically only able to benefit from state R&D tax credits if they have income tax to offset. The fact that startups typically are unprofitable, mitigates their ability to benefit from these tax programs.

These results underscore the importance of the M&A channel for startups to benefit from tax-based R&D incentives. More concretely, because established firms appear to respond to

R&D tax credits by increasing their acquisition of startups irrespective of targets' location, the fact that we observe no direct local impact on VC activity suggests that R&D tax credits only reach the startup ecosystem indirectly through the M&A channel. By providing M&A capital to startups and their investors, established firms enhance the effectiveness of R&D tax credits in incentivizing and financing startups' innovative activities.

## 3.3 Robustness

One concern could be that it is unclear whether Poisson regressions are suitable for R&D expenditure as an outcome variable. More than 90% of firm-years in our sample have non-zero R&D values, whereas Poisson regressions are more efficient for outcomes with long tails and many zeros. Accordingly, we run log-linear regressions with one plus R&D variable for all fully saturated models using R&D as an outcome in the paper. The results are reported in Table A2 and are economically consistent with the results reported using Poisson regressions.

Another concern could be that R&D tax credits simply alleviate firms' financial constraints. The accompanied increase in cash flow could then drive investment broadly defined to include expenditure on items including R&D, physical capital, and M&A. Under this hypothesis, the relationship between the tax-based user cost of R&D and the acquisition activity of established firms is simply driven by an increase in after-tax net income. We view this as improbable as it is incongruent with the fact that we find that declines in the tax-based user cost of R&D are associated with the acquisition of VC-backed startups, but not other firms.

Still, to directly address this concern, we show in Appendix Table A4 that a decline in the tax-based user cost of R&D capital is not associated with changes in capital expenditure. Using the log of capital expenditure as an outcome variable, we observe that the coefficient on the user cost of R&D is statistically insignificant across all specifications. Moreover, with the inclusion of R&D spending as a control variable in Column (3), the coefficient becomes positive, which could be interpreted as suggesting that any impact on capital expenditure is driven by physical capital investment scaling up with intangible capital. These results should ease any concern that the relationship between the tax price of R&D and acquisition activity is driven by changes in cash flow, as opposed to a lower R&D tax price.

## 4 Conclusion

In conclusion, this paper highlights the novel role of established firms in the allocation of government-provided R&D credits/subsidies to entrepreneurial startups. By taking advantage of R&D tax credits and acquiring startups, established firms can effectively increase their R&D labor and enhance their innovation capabilities. The research findings indicate that a plausibly-exogenous decrease in the user cost of R&D capital leads to increased acquisition of venture capital-backed startups by established firms. This suggests that acquiring R&D-intensive startups is a strategic approach for firms to augment their R&D efforts, rather than solely relying on internal R&D intensity.

Furthermore, the study reveals that startups, perhaps due to their limited taxable income, appear unable to directly benefit from R&D tax credits. Hence, the intermediary role of established firms in reallocating these credits to startups becomes crucial in alleviating their financial constraints and fostering their growth and innovation potential. The evidence supports the notion that the reallocation effect of R&D tax credit-induced M&A activity by established firms plays a significant role in supporting startups, which have a high need for capital but limited access to it.

In summary, this paper sheds light on the re-allocative role of established firms in utilizing R&D tax credits to acquire startups, facilitating the creation and growth of entrepreneurial ventures. The findings emphasize the importance of an active M&A market for startups in the efficient allocation of R&D tax credits. Potential future research could examine whether or not inactive M&A markets hinder the capacity of established firms to reallocate R&D capital to the startup ecosystem.

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# Tables

**Table 1:** Summary Statistics

The table presents summary statistics of the major variables in our analyses. For Panel A, the sample consists of firms with non-missing values for the variables of interest. Firm financials are winsorized at the 1% level. For Panel B, the sample consists of states between 1990 and 2015.

| Panel A: Firm-Year Level | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Std.Dev. | p10 | p25 | Med. | p75 | p90 | n |
| M&A | 0.75 | 7.41 | 0.00 | 0.00 | 0.00 | 1.00 | 2.00 | 37070 |
| VC M&A | 0.13 | 1.01 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 37070 |
| Non-VC M&A | 0.63 | 6.52 | 0.00 | 0.00 | 0.00 | 1.00 | 1.00 | 37070 |
| M&A Spending | 104.50 | 1200.17 | 0.00 | 0.00 | 0.00 | 3.68 | 71.06 | 37070 |
| R&D | 115.18 | 572.90 | 0.61 | 2.40 | 10.15 | 39.89 | 151.73 | 37070 |
| CAPX | 148.94 | 1087.09 | 0.12 | 0.71 | 4.89 | 32.09 | 171.07 | 36735 |
| $\rho^t$ | 1.08 | 0.03 | 1.05 | 1.06 | 1.08 | 1.10 | 1.11 | 37070 |
| Total Assets | 3193.00 | 21295.36 | 8.11 | 30.76 | 138.34 | 792.39 | 4069.60 | 37069 |
| Tobin's Q | 3.26 | 7.15 | 0.96 | 1.23 | 1.78 | 2.98 | 5.41 | 36943 |
| Cash Holdings | 0.27 | 0.26 | 0.02 | 0.05 | 0.18 | 0.42 | 0.68 | 37066 |
| Leverage | 0.14 | 0.21 | 0.00 | 0.00 | 0.05 | 0.21 | 0.36 | 36995 |
| ROA | -0.11 | 0.80 | -0.49 | -0.08 | 0.09 | 0.16 | 0.22 | 37035 |
| Panel B: State-Year Level | | | | | | | | |
| | Mean | Std.Dev. | p10 | p25 | Med. | p75 | p90 | n |
| VC Rounds | 83.10 | 253.41 | 3.00 | 6.00 | 21.00 | 72.00 | 141.00 | 1210 |
| VC Raised | 678.13 | 2702.84 | 3.75 | 17.25 | 91.48 | 445.28 | 1204.53 | 1210 |
| $\rho^S$ | 1.17 | 0.05 | 1.12 | 1.16 | 1.18 | 1.20 | 1.21 | 1210 |
| GDP Growth | 4.81 | 3.11 | 1.50 | 3.30 | 4.70 | 6.50 | 8.60 | 1210 |
| Income p.c. | 0.03 | 0.01 | 0.02 | 0.02 | 0.03 | 0.04 | 0.05 | 1210 |

**Table 2:** Acquisitions by Target Type

The table presents the results from estimating Poisson regressions on the count of acquisitions by target type. The unit of observation is at the acquirer-year level. The dependent variable in columns (1) and (2) is the number of acquisitions with no restriction on target type. In columns (3) and (4) the dependent variable is the number of acquisitions of targets that are not VC-backed. In columns (5) and (6) the dependent variable is the number of acquisitions of targets that are VC-backed. Control variables are lagged by one year and winsorized at the 1st and 99th percentiles. level. Standard errors are two-way clustered at the headquarter state-year level. Detailed variable definitions are provided in the appendix. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1) All | (2) All | (3) Non-VC | (4) Non-VC | (5) VC | (6) VC |
|---|---|---|---|---|---|---|
| $\rho^t$ | -2.149 | 0.231 | -0.437 | 1.462 | -7.872*** | -4.050*** |
| | (1.883) | (1.232) | (1.639) | (1.413) | (2.573) | (1.304) |
| Log Assets | | 0.242*** | | 0.187*** | | 0.377*** |
| | | (0.015) | | (0.016) | | (0.055) |
| ROA | | 0.231** | | 0.237** | | 0.398* |
| | | (0.090) | | (0.114) | | (0.233) |
| Cash | | 0.610*** | | 0.578*** | | 0.844*** |
| | | (0.130) | | (0.149) | | (0.116) |
| Tobin's Q | | 0.016*** | | 0.018*** | | 0.017*** |
| | | (0.002) | | (0.002) | | (0.005) |
| Leverage | | -0.662*** | | -0.610*** | | -0.970*** |
| | | (0.113) | | (0.149) | | (0.223) |
| Constant | 3.931* | -0.626 | 2.003 | -1.596 | 8.444*** | 1.166 |
| | (2.022) | (1.267) | (1.763) | (1.516) | (2.742) | (1.312) |
| N | 28705 | 28705 | 27699 | 27699 | 15218 | 15218 |
| Pseudo $R^2$ | 0.628 | 0.632 | 0.621 | 0.624 | 0.432 | 0.442 |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 3:** R&D and Acquisition Expenditure

The table presents the results from estimating Poisson regressions on total expenditure on acquisitiosn and R&D, seperately. The unit of observation is at the acquirer-year level. The dependent variable in columns (1) and (2) is total acquisition expenditure and in columns (3) and (4) it is total R&D expenditure. Control variables are lagged by one year and winsorized at the 1st and 99th percentiles. Standard errors are two-way clustered at the headquarter state-year level. Detailed variable definitions are provided in the appendix. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1)<br>M&A Spending | (2)<br>M&A Spending | (3)<br>R&D | (4)<br>R&D |
|---|---|---|---|---|
| $\rho^t$ | -14.545$^{***}$ | -10.773$^{**}$ | -9.312$^{**}$ | -5.040$^{***}$ |
| | (4.770) | (4.560) | (4.181) | (1.630) |
| Log Assets | | 0.567$^{***}$ | | 0.670$^{***}$ |
| | | (0.094) | | (0.044) |
| ROA | | 2.264$^{**}$ | | -0.048 |
| | | (1.106) | | (0.089) |
| Cash | | 1.406$^{*}$ | | -0.290$^{**}$ |
| | | (0.763) | | (0.146) |
| Tobin's Q | | 0.034$^{**}$ | | 0.003 |
| | | (0.014) | | (0.007) |
| Leverage | | -1.846$^{***}$ | | -0.160 |
| | | (0.468) | | (0.119) |
| Constant | 22.648$^{***}$ | 13.098$^{**}$ | 16.890$^{***}$ | 6.323$^{***}$ |
| | (5.096) | (5.154) | (4.477) | (1.878) |
| N | 28389 | 29296 | 35571 | 35571 |
| Pseudo $R^2$ | 0.643 | 0.660 | 0.947 | 0.967 |
| Firm Fixed Effects | Yes | | Yes | Yes |
| Year Fixed Effects | Yes | | Yes | Yes |

**Table 4:** Startup Acquisitions and the Tax Price of R&D

The table presents the results from estimating Poisson regressions on R&D. The unit of observation is at the acquirer-year level. The dependent variable in columns (1) and (2) is total acquisition expenditure and in columns (3) and (4) it is total R&D expenditure. Control variables are lagged by one year and winsorized at the 1st and 99th percentiles. Standard errors are two-way clustered at the headquarter state-year level. Detailed variable definitions are provided in the appendix. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1) R&D | (2) R&D | (3) R&D | (4) R&D | (5) R&D |
|---|---|---|---|---|---|
| Log(VC) | 0.191*** | 0.182*** | 0.118*** | 4.381*** | 2.018*** |
| | (0.058) | (0.051) | (0.031) | (1.168) | (0.602) |
| $\rho^t$ | | -8.845** | -4.824*** | -7.284* | -4.190*** |
| | | (3.845) | (1.469) | (3.970) | (1.451) |
| $\rho^t \times$ Log(VC) | | | | -3.937*** | -1.780*** |
| | | | | (1.099) | (0.567) |
| Log Assets | | | 0.664*** | | 0.661*** |
| | | | (0.046) | | (0.046) |
| ROA | | | -0.056 | | -0.058 |
| | | | (0.086) | | (0.089) |
| Cash | | | -0.270* | | -0.270 |
| | | | (0.142) | | (0.165) |
| Tobin's Q | | | 0.003 | | 0.003 |
| | | | (0.007) | | (0.007) |
| Leverage | | | -0.136 | | -0.136 |
| | | | (0.109) | | (0.109) |
| Constant | 6.817*** | 16.296*** | 6.079*** | 14.621*** | 5.425*** |
| | (0.024) | (4.114) | (1.707) | (4.252) | (1.682) |
| N | 35571 | 35571 | 35571 | 35571 | 35571 |
| Pseudo $R^2$ | 0.947 | 0.948 | 0.968 | 0.949 | 0.968 |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes |

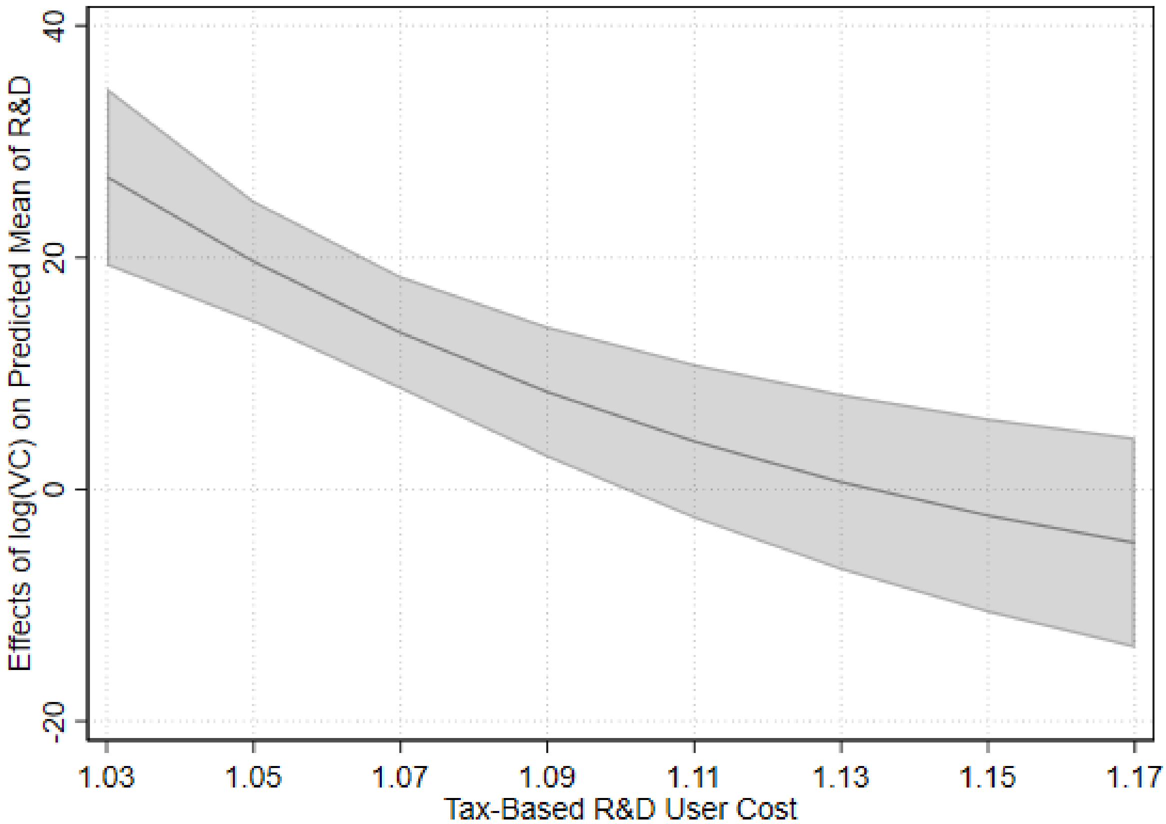
**Table 5:** State-level R&D Tax Price and Venture Capital

The table presents the results from estimating Poisson regressions on the aggregate number of venture capital rounds and volume, separately. The unit of observation is at the state-year level. The dependent variable in columns (1) and (2) is the total number of venture capital rounds conducted in the state and in columns (3) and (4) it is the total volume of venture capital raised. Standard errors are two-way clustered at the state-year level. Detailed variable definitions are provided in the appendix. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1)<br>VC Rounds | (2)<br>VC Rounds | (3)<br>VC Raised | (4)<br>VC Raised |
|---|---|---|---|---|
| $\rho^S$ | -0.208 | -0.380 | 0.112 | 0.279 |
| | (0.683) | (0.669) | (2.277) | (2.346) |
| GDP Growth | | $0.013^{**}$ | | 0.013 |
| | | (0.005) | | (0.012) |
| Log(Income p.c.) | | 0.625 | | 0.982 |
| | | (0.695) | | (1.769) |
| Constant | $5.882^{***}$ | -0.475 | $8.082^{***}$ | -2.520 |
| | (0.804) | (7.230) | (2.592) | (20.124) |
| N | 1648 | 1648 | 1210 | 1210 |
| Pseudo $R^2$ | 0.959 | 0.959 | 0.931 | 0.931 |
| State Fixed Effects | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |

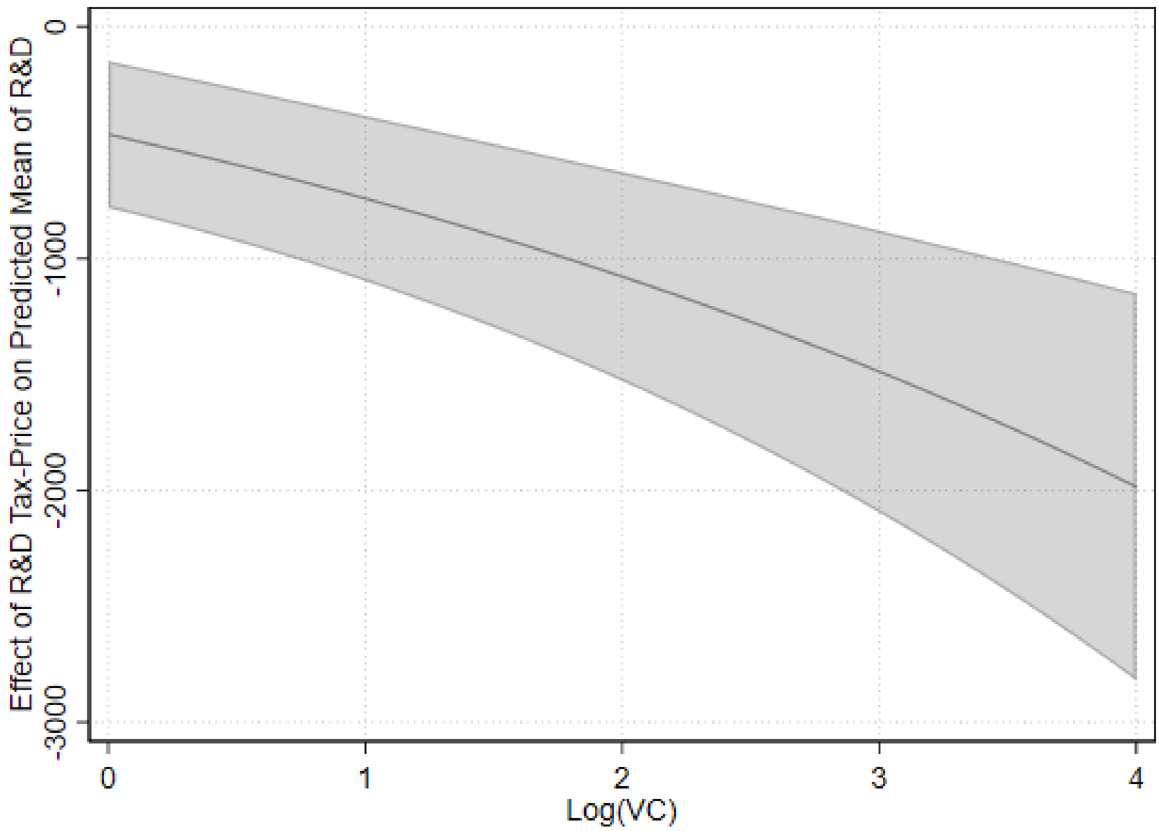
# Figures

**Figure 1a:** Average Marginal Effects of VC-Acquisitions on R&D Expenditure




The displays the average marginal effect of $Log(VC)$ on the predicted mean of R&D given different values of the tax price of R&D from the interaction term in Column 5 of Table 4. The shaded region represents 95% confidence intervals.

**Figure 1b:** Average Marginal Effects of R&D Tax Price on R&D Expenditure




The graph displays the average marginal effect of the tax price of R&D given different values of $Log(VC)$ from the interaction term in Column 5 of Table 4. The shaded region represents 95% confidence intervals.

# 5 Appendix Tables

**Table A1:** Variable Definitions

| Variable Name | Definition |
|---|---|
| VC Rounds | Aggregate number of venture capital rounds conducted in the state |
| VC Raised | Aggregate volume of venture capital invested in the state in mil. USD |
| $\rho^S$ | State-level tax component of R&D user cost |
| GDP Growth | State GDP growth |
| Income p.c. | Average per capita income of households in the state reported in USD MIL |
| M&A | Count of number of companies acquired by the firm |
| VC M&A | Count ofn umber of private companies acquired by the firm with venture capital backing |
| Non-VC M&A | Count of number of companies acquired by the firm without venture capital backing |
| M&A Spending | Total expenditure on M&A of the firm in mil. USD |
| R&D | Total expenditure on on R&D (xrd) in mil. USD |
| $\rho^t$ | Average of tax component of R&D user cost from state and federal R&D credits and incomes taxes |
| Total Asset | Firm's total assets in mil. USD (at) |
| Tobin's Q | Total assets (at) minus total common equity (ceq) plus common shares outstanding (csho) times the price at close (prcc_c), all divided by total assets (at) |
| Cash Holdings | Cash holdings (che) divided by total assets (at) |
| Leverage | Total long-term debt (dltt) divided by total assets (at) |
| ROA | Operating income before depreciation (oibdp) divided by total assets (at) |
| CAPX | Capital expenditure in mil. USD |

**Table A2:** Log-Plus-One Models

The table presents the results from ordinary least squares regressions. The unit of observation is at the acquirer-year level. The dependent variable is the natural log of R&D expenditure plus one. Standard errors are two-way clustered at the headquarter state-year level. Control variables are lagged by one year and winsorized at the 1st and 99th percentiles. Detailed variable definitions are provided in the appendix. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1)<br>Log(R&D) | (2)<br>Log(R&D) | (3)<br>Log(R&D) |
|---|---|---|---|
| $\rho^t$ | -2.423*** | -2.328*** | -2.286*** |
| | (0.492) | (0.480) | (0.483) |
| Log(VC) | | 0.237*** | 0.913* |
| | | (0.020) | (0.455) |
| $\rho^t \times$ Log(VC) | | | -0.634 |
| | | | (0.429) |
| Log Assets | 0.573*** | 0.566*** | 0.566*** |
| | (0.022) | (0.021) | (0.021) |
| ROA | -0.116*** | -0.115*** | -0.115*** |
| | (0.012) | (0.012) | (0.012) |
| Cash | -0.038 | -0.046 | -0.046 |
| | (0.037) | (0.036) | (0.036) |
| Tobin's Q | 0.010*** | 0.010*** | 0.010*** |
| | (0.001) | (0.001) | (0.001) |
| Leverage | -0.057*** | -0.051*** | -0.051*** |
| | (0.018) | (0.018) | (0.018) |
| Constant | 2.357*** | 2.278*** | 2.234*** |
| | (0.527) | (0.515) | (0.520) |
| N | 35571 | 35571 | 35571 |
| Adjusted $R^2$ | 0.955 | 0.956 | 0.956 |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |

**Table A3:** Instate versus Out-of-State VC-Backed Targets

The table presents the results from Poissoin regressions. The dependent variable in Column 1 is the count of VC-backed targets headquartered in the same state as the headquarters of the firm. The dependent variable in Column 2 is the count of VC-backed targets headquartered outside the state of the headquarters of the firm. The unit of observation is at the acquirer-year level. Standard errors are two-way clustered at the headquarter state-year level. Detailed variable definitions are provided in the Appendix Table A1. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1) In-State VC | (2) Out-of-State VC |
|---|---|---|
| $\rho^t$ | -5.884*** | -4.999*** |
| | (2.094) | (1.929) |
| Constant | 4.977** | 4.406** |
| | (2.199) | (2.060) |
| N | 6088 | 13445 |
| Pseudo $R^2$ | 0.209 | 0.238 |
| Firm Fixed Effects | Yes | Yes |
| Year Fixed Effects | Yes | Yes |

**Table A4:** Tax Price of R&D and Capital Expenditure

The table presents the results from ordinary least squares regressions. The unit of observation is at the acquirer-year level. The dependent variable is the natural log of capital expenditure plus one. Standard errors are two-way clustered at the headquarter state-year level. Control variables are lagged by one year and winsorized at the 1st and 99th percentiles. Detailed variable definitions are provided in the appendix. $***$, $**$, and $*$, represent statistical significance at the 1%, 5% and 10% levels, respectively.

| | (1)<br>Log(CAPX) | (2)<br>Log(CAPX) | (3)<br>Log(CAPX) |
|---|---|---|---|
| $\rho^t$ | -0.578 | -0.112 | 0.817 |
| | (0.661) | (0.657) | (0.762) |
| Log(R&D) | | | 0.389*** |
| | | | (0.018) |
| Log Assets | | 0.533*** | 0.311*** |
| | | (0.017) | (0.016) |
| ROA | | -0.041*** | 0.004 |
| | | (0.014) | (0.015) |
| Cash | | -0.125*** | -0.112*** |
| | | (0.034) | (0.031) |
| Tobin's Q | | 0.019*** | 0.015*** |
| | | (0.002) | (0.002) |
| Leverage | | -0.190*** | -0.166*** |
| | | (0.036) | (0.033) |
| Constant | 2.878*** | -0.311 | -1.213 |
| | (0.711) | (0.735) | (0.828) |
| N | 35258 | 35258 | 35258 |
| Adjusted $R^2$ | 0.914 | 0.940 | 0.945 |
| Firm Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |