Basel III Capital Requirements: Effects on Firm Innovation

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1 Introduction

Regulating financial institutions is a trade-off policymakers have to face. On the one hand, bankruptcies of financial institutions pose an immense risk for economies due to spillover effects onto the real economy. (Nanda and Nicholas (2014); Contreras, Ghosh, and Kong (2021)) Therefore, reducing the default risks of banks is one major goal of policymakers. On the other hand, strict financial regulation itself poses the risk of acting as a funding shock for financial institutions, which these institutions might transmit to the real economy. Understanding the transmission of financial regulation is therefore crucial in order to make informed decisions about how to regulate financial institutions. The transmission of financial regulation from banks to firms is a versatile and complex topic. This master thesis thematizes one particular instance of this research field: The effect of announced higher capital requirements for banks on firm innovation.

The relationship between bank credit and the research and development of firms is subtle. It is generally agreed upon that bank credit is not a feasible financing option for research and development projects due to the volatile, asymmetrically distributed cash flow profile of such projects. (B. H. Hall (2002); B. Hall and Lerner (2009)) Although financing through bank loans does not play a primary role in financing research and development projects, bank credit still indirectly influences firms' research and development investment decisions. Improved access to bank credit enhances the overall financial conditions for firms. Funds that would have otherwise been utilized for non-research and development-related means can now be redirected to finance research and development-related projects. Therefore, enhanced access to credit can foster the financing environment for research and development projects. (Amore, Schneider, and Zaldokas (2013)) The strand of literature which thematizes the effects of financial regulation on firm innovation in the United States finds heterogeneous impacts based on the type of regulation and the type of firms considered. (Cornaggia et al. (2015), Amore, Schneider, and Zaldokas (2013), S. Chava et al. (2013), Hombert and Matray (2016)) These papers examine the consequences of geographic deregulation within the financial sector in similar time frames spanning from 1968 to 2006, but on different facets of banking markets and for various types of firms. For interstate deregulation, Amore, Schneider, and Zaldokas (2013) find a positive effect on innovation output for manufacturing firms, confirmed by S. Chava et al. (2013) for young and private firms. S. Chava et al. (2013) also consider intrastate branching deregulation, which enhances the local market power of banks and leads to a decrease in innovation outputs of young, private firms. Hombert and Matray (2016) find similar results for intrastate branching deregulation, driven by the bank entry, which harms existing lending relationships. They report a shift in the distribution of innovative firms from small to large. Moreover, they report an overall reduction in innovation output. Cornaggia et al. (2015) report validating results for interstate branching deregulation. The authors do also report a high heterogeneity between private and public firms. Due to enhanced credit opportunities, private firms pose a less attractive acquisition target, harming the innovation output of public, acquisition-focused firms, but fostering innovation of private firms. As stated implicitly, results are highly dependent on the effect of deregulation on the specifics of local lending competition, the acquisition interaction between firms as well as existing lending relationships. Dou and Z. Xu (2021) employ a comparable setup to this master thesis, investigating the impact of regulatory-induced banking funding shocks on firm innovation inputs and outputs. Their study exploits changes in accounting standards introduced by the Statements of Financial Accounting Standards (SFAS) 166 and 167. The authors find an adverse effect on innovation inputs, outputs, and lending of banks.

This thesis exploits the announcement of heightened capital requirements proposed with Basel III at the end of 2010 following the Global Financial Crisis. The minimum tier 1 capital-to-risk-weighted assets ratio (from now on also stated as tier 1 capital ratio) for banks was proposed to be 7% on aggregate, starting at the beginning of 2014. These newly announced capital requirements did pose a funding shock for banks, which reported tier 1 capital ratios below or near the 7% threshold. In this research, the threshold for affected banks is set to be lower or equal than 8% tier 1 capital ratio in the pre-treatment period before the end of 2011. Banks reduced their loan exposure (Gavalas (2015)), which led to firms connected to those banks experiencing a funding shock themselves. This treatment depicts an exogenous funding shock for firms that borrow from affected banks because it does not change the investment opportunities of firms and is purely based on the capital structure of banks. Therefore, the supply side of credit drives the results reported in this thesis. Valuable, long-term lending relationships cannot be recreated (Bushman, Williams, and Wittenberg-Moerman (2017)) immediately. As a result, the funding shock experienced by banks affects treated firms, which cannot switch to new lenders under similar loan terms due to information asymmetry. These circumstances make it possible to assume an exogenous treatment on the firm level. In the pre-treatment period, around one of every three firms in the sample borrowed from a bank, which falls under the set threshold. I consider a matched sample of firms in the United States between 2008 and 2016. Firms are treated if they borrow from banks reporting a tier 1 capital-to-risk-weighted assets ratio of below 8% at least once in the pre-treatment period, predating the end of 2011. To examine the effect of borrowing from a treated bank on innovation input, I use a difference-in-differences approach, including firm- and time-fixed effects on the matched sample. With this approach, I control for time-invariant factors, as well as time-varying, firm-specific factors. I also consider sector-specific trends in the baseline regression, controlling for time-varying, sector-specific differences between the two groups. As a matching algorithm, a propensity score matching without replacement, using a logistic regression as a baseline method, is implemented based on a basket of firm- and loan-specific variables. The matching process links firms one-to-one based on pre-treatment averages of the previously mentioned variables. This process eases concerns about the difference-in-differences assumptions and imbalances between treatment and control groups across numerous dimensions. I find a negative association between the Basel III capital requirements announcement and firm innovation inputs. Treated firms reduce their research and development intensity in the year following the treatment by 15.8% on average compared to their mean research and development intensity. The effect is statistically highly significant, economically relevant, and robust when introducing sector-time fixed effects. On top of that, the effect is heterogeneous across multiple firm-specific dimensions. The effect is more pronounced for firms that report high market-to-book ratios and firms that are highly dependent on external financing sources. The effect is also more pronounced for firms with lower average maturities of loans in their portfolios. A triple difference-in-differences approach is implemented to confirm the group differences. I find no statistically robust disparities when assessing the firm age as a distinguishing component. To test the robustness of results, I first address the issue of having multiple lenders with only a fraction of treated lenders. The matched sample is split into three groups, decided on by the relative share of treated lenders in the pre-treatment period. The results indicate that firms that borrow from a higher number of non-treated lenders than treated lenders are not affected by the announcement of the forthcoming regulation. Results are driven by firms that borrow from more treated than non-treated lenders. This robustness check indicates that the reported main results might be interpreted as a conservative estimation of the real effect. The threshold, the deciding factor of treated banks and firms, is another starting point of a robustness check. The 8% cut-off value was set by economic intuition and reference papers, but there is no universal true cut-off value since every bank is individual. This instance is also mentioned by Hendricks et al. (2023) and Deli and Hasan (2017). The results are being tested by reproducing the baseline specification for thresholds of 7% and 11% tier 1 capital ratio as a cut-off value for treated banks. The relative economic size of the coefficient remains fairly constant across robustness checks, but there is a loss of significance for the 11% threshold, in line with Deli and Hasan (2017). The Dodd-Frank Act and the introduction of SFAS 166/167 were considered and might serve as confounding events. To assess their influence, a dynamic difference-in-differences approach was implemented and discussed in section 5. My results generally align with existing research, especially confirming Dou and Z. Xu (2021) of how bank funding shocks are being passed to the real economy. This thesis also aligns with S. Chava et al. (2013) in their conclusion that the type of financial regulation crucially affects how banks and extension firms will be affected.

2 Related Literature

This piece of research contributes to two strands of academic literature. The first strand focuses on the factors that influence the innovation input and output of firms, particularly the role of external finance. The second strand examines the real effects of financial regulation, specifically on the innovation potential of firms.

Many factors influence firm innovation input and output. Besides others, general economic conditions (Cefis, Bartoloni, and Bonati (2020)), ownership structure (Guangzhou Hu (2001)), firm information transparency (Zhong (2018)), regulatory continuity and state intervention (Li et al. (2020)), and inter- (Liu, Ying, and Wu (2017)) as well as intra-firm (Dey and Ganesh (2017)) collaboration are among the influencing factors. This research is closely related to the strand of literature, which investigates the financial factors influencing firm innovation, focusing on the development of debt markets. The investment gap, which is unique for R&D investments, compared to fixed capital investments, can be only partially attributed to external funding constraints. (B. Hall and Lerner (2009)) Even though the financial characteristic of R&D investments make them, in theory, unattractive to debt financing (B. H. Hall (2002)) and firms do in reality finance their R&D projects through other sources thank bank debt (Knudsen and Lien (2015)), there exist a number of publications that report an association between a change in bank characteristics and research and development inputs and outputs of related firms. (Benfratello, Schiantarelli, and Sembenelli (2008), Nanda and Nicholas (2014), Hsu, Tian, and Y. Xu (2014), Hellmann, Lindsey, and Puri (2008), Deng, Mao, and Xia (2021))

Among others Benfratello, Schiantarelli, and Sembenelli (2008) showed for a sample of Italian firms that banking development affects innovative activities of firms positively, particularly for sectors more dependent on external financing sources. (see also Brown, Fazzari, and Petersen (2009)) Nanda and Nicholas (2014) do confirm these results by finding an adverse effect of banking distress on firm innovation, during the Great Depression in the United States, in particular for capital-intensive firms. Hsu, Tian, and Y. Xu (2014) find conflicting results on a cross-country level for emerging market economies. They find that external finance-dependent firms and high-tech industries benefit from more developed equity markets but are adversely affected by more developed credit markets. To what extent R&D inputs and outputs are sensitive to changes in the banking industry remains debatable and is the subject of recent research. In close proximity to this thesis and the already-stated strand of research, there is a set of papers that examines the effects of banking regulation on firm innovation in the United States of America. Almost all of these papers follow a similar approach, focusing on the impact of the staggered removal of geographic restrictions for banks across different states in the United States using a difference-in-differences design. (Cornaggia et al. (2015), Amore, Schneider, and Zaldokas (2013), S. Chava et al. (2013), Hombert and Matray (2016), Dou and Z. Xu (2021)) Even though most of the results are coherent among this strand of papers, the papers differ in detail based on the type of regulation and the type of firms assessed. The authors consider intrastate bank branch deregulation (Hombert and Matray (2016)), interstate deregulation of banks (S. Chava et al. (2013), Amore, Schneider, and Zaldokas (2013)) and branches (Cornaggia et al. (2015)) and the regulation of a share of banks, decided on by non-geographical, balance sheet dependent factors (Dou and Z. Xu (2021)).

S. Chava et al. (2013) use data between 1975 and 2005 to examine the effect of two different types of bank deregulation on firm innovation of young, private firms. The authors consider intrastate bank branching deregulation and interstate bank deregulation. The results for firm innovation differ depending on the type of deregulation. Deregulation, which enhances the local market power of banks (intrastate branch deregulation), harms firm innovation of young, private firms, while deregulation, which reduces local market power (interstate bank deregulation) of local branches, enhances the innovation potential and growth of young, private firms. Regarding economic size, the authors report for intrastate bank deregulation, that the patent numbers increased by 17% for young private firms. Considering intrastate deregulation, the authors find a 23% decrease in patent filings. One aspect to point out is that the authors find no effect on mature or public firms, regardless of which deregulation is considered.

Interstate bank deregulation is also assessed by Amore, Schneider, and Zaldokas (2013), focusing on public manufacturing firms between 1976 and 1995. The authors argue that better availability and quality of bank credit for manufacturing firms fostered innovation. They state that allowing banks to enter new states increased the expected patent counts by 13.8%. They conclude that the effect is mainly driven by external finance-dependent sectors and banks close to the states they are newly permitted to enter. The findings differ to S. Chava et al. (2013) in that the authors find an effect for public firms for the same type of financial deregulation considered.

The other main deregulation type considered by S. Chava et al. (2013), intrastate branching deregulation, is also exploited by Hombert and Matray (2016) for data between 1970 and 1990. The authors focus on the ability of banks to address soft information. They are reconfirming S. Chava et al. (2013) result, reporting that deregulation is associated with a 20% drop in innovative firms and an overall drop in patent activity. While overall funding conditions improve, more new entrant banks impair lending relationships, especially for small and innovative firms, while bigger public firms benefit from better funding terms. They also report a shift in the innovative firms' size and maturity distribution. Cornaggia et al. (2015) assess the same time frame as S. Chava et al. (2013), but exploit intrastate branching deregulation. They conclude that interstate deregulation on the branch level, which reduces the local market power of banks and enhances banking competition, fosters innovation for private, generally younger firms, and harms innovation by bigger, publicly listed firms. The aggregate effect of these two counteracting forces is negative, with a reduction of 30% in patent filing numbers in the year following the deregulation. They explain this finding by suggesting that when private firms have better access to financing, it fosters innovation within those firms. Furthermore, improved funding conditions make it less likely for larger companies to acquire these smaller private firms, harming their innovation prospects in return. While the line of reasoning is similar to Hombert and Matray (2016), the findings are different, which might be due to different types of regulation considered.

The papers presented beforehand lack direct evidence on the lending channel, which Dou and Z. Xu (2021) address. On top of that, this paper does provide evidence for innovation inputs and outputs rather than only outputs. The authors exploit the introduction of SFAS 166/167, an accounting change introduced in January 2010, which affected a set of banks adversely. The treatment differs from those in previously discussed papers, affecting banks based on balance sheet differences rather than geographic location. The authors map the treatment onto firm-level, which borrow from affected banks, similar to my thesis. A major difference from the papers beforehand is the focus on innovation output and innovation input as an outcome variable. The authors report that firms borrowing from affected banks shrink their R&D exposures on average by 16% and produce fewer citations and patents. Negative effects on innovation inputs and outputs are more pronounced for firms with more intense lending relationships with downward pressured banks, confirming Hombert and Matray (2016). Besides the main results, they confirm the results of Amore, Schneider, and Zaldokas (2013), that external finance-dependent firms react more heavily from deteriorated financing environments.

3 Hypotheses and Empirical Strategy

3.1 Hypotheses

Hypothesis 1: The announcement of higher capital requirements for banks leads to a reduction in innovation intensity of firms that borrow from potentially affected banks.
Hypothesis 2: The effect differs among various firm characteristics.

3.2 Empirical Model

The main effect of the Basel III capital requirements announcement is assessed through a difference-in-differences approach on a matched sample:

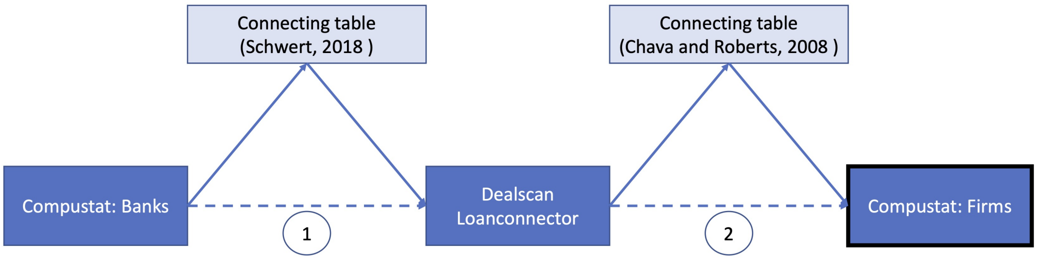
$$Y_{i,t+1} = \beta \, Treat_i * Post_t + \delta_i + \delta_t \tag{1}$$

$Y_{i,t+1}$ represents the percentage of research and development expenditures relative to the total assets of firm i at time $t+1$, depicted in equation (2). The outcome variable is projected into the future since decisions on research on development spending are typically made in advance.

$$Y_{i,t+1} = \frac{R\&D_{i,t+1}}{AT_{i,t+1}} * 100 \tag{2}$$

Samples are matched based on a basket of variables, which are discussed in section 3.6. The matching algorithm is a one-to-one propensity score matching, using a logistic regression as a baseline model. δ_i are firm fixed effects, δ_t are time fixed effects.
The treatment variable is binary, indicating firms that borrowed from affected banks in the pre-treatment period, 2008-2011. Affected banks are identified by their respective tier 1 capital ratio in the pre-treatment period. Banks that have reported a tier 1 capital ratio below 8% at least once during the pre-treatment period are categorized as treated. The post variable is either an indicator variable, which indicates the timing of the treatment, or a step-wise variable, which indicates the years since the treatment occurs. Standard errors are clustered at the matched subclass (matched firms) for all specifications reported, following Abadie and Spiess (2022). See appendix, section A.3.2 for a discussion of clustering one-to-one matched samples.

3.3 Dataset



Number	Description	Variable names
(1)	Bank characteristics of the main lender are mapped onto the firm-loan dataset. All variables besides tier 1 capital ratio are averages of the pre-treatment period	Tier 1 capital ratio, earnings per share, allowances, net income, assets total
(2)	Loan and bank characteristics of the main lenders are shared onto the firm-level dataset	Maturity per loan, number of different lenders, all variables from (1) per main lender

Figure 1: Visual representation of linking process of databases, to build the final dataset

To capture the effect of Basel III on firm innovation inputs, I connect several databases containing firm-annual information, bank-annual information, and information about loans granted from banks to firms. Figure 1 visually represents the linking procedure.

The primary data source on annual, firm-specific characteristics is taken from the widespread database from Compustat. Data entries between 2008 and 2016 are considered in the matched sample. Each year in the complete database is deemed to create the firm age variable. Only non-financial firms are included, excluding firms operating in the real estate or the financial sector, due to comparability reasons. Real estate and financial firms have distinct reporting practices and operate under different business models.(Koh and Reeb (2015)) Firms with at least one observation before the treatment at the end of 2011 and at least one observation after the treatment in 2011 are considered. This restriction on data availability is put into place to ensure a valid one-to-one matching. (See section 3.7 for more information on matching) The accounting standard is constant across observations. Only positive R&D intensities are considered, and the dataset is winsorized for the R&D intensity to reduce outlier problems. The propensity score matching process, described in section 3.7, is conducted only on complete observations for the matching variables, which is ensured beforehand.

Using the linking table from Chava and Roberts (2008), I connect the Compustat firm-level

database with the WRDS-Reuters' DealScan legacy database, which contains information about loans granted by financial institutions to firms. Using the loan-level database, the number of different lenders and the average maturity of loans are incorporated onto the firm-level dataset. To develop the treatment variable, I consider the Compustat database on banks, which includes tier 1 capital ratios to banks' risk-weighted assets in the United States, among other variables. I considered using data on banks directly from the FFIEC but ultimately decided on using the Compustat bank data due to a more easy linkage process of the datasets. For the bank-level database, I only consider observations that contain complete information on tier 1 capital ratios because this particular variable serves as the determining factor for the treatment. For syndicated loans, only the lead arranger is considered. Otherwise, the matching of lenders and borrowers would become increasingly difficult. Besides the tier 1 capital ratio, pre-treatment averages of the earnings per share, the amount of allowances, the net income, and the total assets for banks are mapped onto the firm-level dataset through the loan-level dataset. (See Figure 1)
To map observations from the bank-level dataset onto the loan-level dataset, mainly the linking table from Schwert (2018) is considered.

3.4 Treatment

At the end of 2010, the Basel Committee on Banking Supervision initially announced its agreements on global capital requirements for banks. (see Basel Committee on Banking Supervision (2010)) In the initial announcement, the committee announced, among other regulatory aspects, a forthcoming minimum capital requirement of 4.5% tier 1 capital to risk-weighted-assets ratio, with a conservation buffer of 2,5% of common equity tier 1 to risk-weighted-assets, bringing the total up to 7%. While the execution of this regulation became binding at the beginning of 2014, banks clearly anticipated a forthcoming regulation implementation. (Hendricks et al. (2023)) Starting in 2012, the variable post is set to 1. The delayed introduction of the post indicator is attributed to the annual reporting of data throughout the year, my intention to ensure the inclusion of the announcement in the decision-making process of banks.
The deciding threshold for banks affected by this regulation is set to be a tier 1 capital ratio below or equal to 8% for 2007 - 2011, before the announcement. All banks which report a tier 1 capital ratio below 8% for at least one year in the pre-treatment period are considered in the treatment group. The threshold is set 1% higher than the proposed

minimal requirement to account for banks close to the threshold, which are still likely to be affected by this new announcement. For a simple theoretical illustration of this line of reasoning, see Fraisse, Lé, and Thesmar (2020). For each observation in the firm-loan level dataset, the treatment status of the main lending bank is assigned. In the second step, for each observation in the firm-level dataset, the corresponding loans in each year are matched, and the treatment variable is assigned. Firms with at least one treated-bank connection before 2012 are assigned to the treatment group; all other firms are in the control group.

The treatment is set to be on the firm level since it is assumed that banks pass on the treatment in some way to their loan exposures and, by extension, to the firms which borrow from affected banks. (Guney, Karpuz, and Ozkan (2017)) In a second specification, a step-wise, linear treatment is implemented. (see Kerr and Nanda (2009) and Amore, Schneider, and Zaldokas (2013)) This approach captures the non-binding status of the treatment in the period between 2011 and 2014. On top of that, a dynamic difference-in-differences approach is implemented to reconfirm the results of the step-wise linear estimation. The construction of the treatment variable consolidates various firms with varying degrees of borrowing engagement with the treated banks, which in turn might lead to a conservative measure of treatment effects. In the robustness checks (see 4.3.2) part, other thresholds for banks and the relative dependence on individual, treated lenders are considered to confirm this testimony.

3.5 Dependent Variable: Innovation Measures

Innovation metrics are hard to measure and interpret. The most common metrics for innovation are patent numbers, citations (see among others S. Chava et al. (2013)), and expenditure in research and development (Dou and Z. Xu (2021)). While patents and citations refer to the innovation output, investments in research and development are generally seen as innovation input. (Ciftci and Zhou (2016))

Patent and citation data are only accessible through special data providers, which could not be accessed with the resources of the University. Research and development expenditures, while correlated with other metrics of firm innovation, do not pose an optimal measure in itself. First, it is highly skewed across firms, depending on the firm size. Secondly, it does not reflect the managerial decisions to innovate perfectly: A firm, which shrinks due to external factors, might still improve on innovative capacity by increasing the relative importance of research within the firm. Therefore, I consider firms' research and development expenditures divided by the total assets for this thesis. This constructed variable represents the relative share of research and development expenditure of a given firm of the total assets of this firm. It can be interpreted as a proxy for the trade-off decision of a given firm. A firm has to decide how much of its resources to invest in potentially innovative processes in the future. While not addressing innovation outputs directly, R&D intensity has a positive effect on innovation performance. (see among others Baysinger, Kosnik, and Turk (1991)) Since budgets for research and development are generally decided on a longer timescale, the R&D intensity one year projected into the future is the main dependent variable, but the R&D intensity two years projected into the future is also considered. As robustness checks, a different outcome variable is considered, particularly sales as a denominator instead of total assets.

3.6 Identification Strategy

A difference-in-differences approach on panel data with a binary treatment on the firm level is used for identification. Firm- and time-fixed effects are included to control for time-invariant, firm-specific factors as well as time-varying factors across all firms. Sources of omitted variable bias to control for are therefore time-varying on a subgroup. Without further processing, the parallel trends assumption is violated. (See Figure 2)
I control for time-varying, firm- and loan-specific factors to fulfill the parallel pre-trend assumption and ease concerns about omitted variable bias using a propensity score matching. The propensity score matching is performed on pre-treatment averages of variables, discussed in section 3.6.1. I match baseline pre-treatment levels of the matching variables to ease concerns about an endogeneity problem from reverse causality if covariates are included, which are correlated with the outcome and the treatment variable and dated after the treatment period. These variables might be outcome variables and thus bias the estimates. However, this approach in itself might lead to a biased estimate if matched treatment and control groups diverge from their respective baseline values after the treatment period. (See section A.3.1 for more information) The matching on baseline **averages** in the pre-treatment period eases this possible divergence problem, as using averages over the pre-treatment periods reduces random outliers being matched and increases the serial correlation of subsequent covariate values with the matched value, reducing the divergence problem further. Sector-time fixed effects are considered in the main regression to control for still omitted variables, which are time-variant and sector-specific. As a general reference point, I considered Dou and Z. Xu (2021) and Amore, Schneider, and Zaldokas (2013) but expanded on their approach. Most importantly, I omitted the change in research and development costs as a matching variable to avoid reverse causality. The selection of variables for the matching process was adjusted based on the formerly mentioned papers to match my specific setup. Across all specifications, standard errors are clustered across matched sub-classes (matched firms), following Abadie and Spiess (2022), who discuss clustering of standard errors for matched samples. See Appendix section A.3.2 for a small discussion. In the following sections, I discuss the considered variables in the matching process. Table 8 in the Appendix describes and explains the variable construction.

3.6.1 Firm Specific Covariates

- *Firms in different industries operate differently regarding products, research and development goals, financing structure, culture, and other factors and are therefore hard to compare.*

  Different sectors might be affected differently by a funding shock, as established in the literature review when comparing Amore, Schneider, and Zaldokas (2013), who only considered public manufacturing firms with S. Chava et al. (2013). The sector of a given firm is also correlated with their bank choice, as some banks preferably lend money to certain sectors. To account for imbalances in the dataset between industry sectors, dummy variables of the first 4 digits of the industry identifier are included in the matching process.

- *More successful firms face different trade-offs regarding R&D intensity than their less successful counterparts.*

  More innovative firms generally perform better than their competitors due to competitive advantages on the market and patents, which can be exploited economically. (Erdogan and Yamaltdinova (2019)) Banks also value higher innovation activities, granting more innovative firms better loan spreads and nonprice-related loan conditions, ceteris paribus. (Francis et al. (2012)) Thus, the success of a given firm is correlated with their bank choice and indirectly with the treatment. Ensuring similar levels of economic success across matched firms might therefore be a reasonable approach to ensure similar trade-off decisions. Other researchers generally use sales growth and cash flow variables to account for differences in economic success. (Dou and Z. Xu (2021))

- *Mature firms are inherently different from immature firms across many dimensions.*

  The former might benefit from less rigid structures and can innovate more radically. On top of that, younger firms tend to be more equity financed, while older firms are typically more debt-financed. (Czarnitzki and Hottenrott (2011)) Younger firms also experience higher capital costs than their older, generally larger competitors, especially in R&D intensive industries. (B. Hall and Lerner (2009)) Another aspect to point out is that longer-lasting firm-bank relationships are primarily established by more mature firms, changing the probability of being treated. (Hombert and Matray (2016)) Firm fixed effects would capture these differences if the differences in maturity did not change over time, which I argue is not the case. All other dimensions fixed, 70-year-old firms might be quite similar to 85-year-old firms, but 5-year-old firms might be quite different from 20-year-old firms. The firm age variable accounts for these differences in maturity over time, ensuring similar age distribution across matched firms.

- Even when accounted for maturity and economic success, *firms concentrated on growth are different from their competitors with a dividend-focused business approach.*
  Market-to-book ratios capture this fundamental difference between growth firms with higher market-to-book ratios (Tidd, Driver, and Saunders (1996)) and dividend-focused firms. Based on the reviewed literature on the transmission of geographical financial deregulation, a deteriorated financing environment might disproportionately hit firms with higher market-to-book ratios. (see among others Amore, Schneider, and Zaldokas (2013))
  The leverage as a matching variable is considered because highly levered firms innovate less in relative terms, given their stricter payment commitments. On top of that, the leverage is inherently related to the treatment variable. Again, the argument of the asymmetric effect of changes applies and leads to the conclusion that firm fixed- effects do not capture these changes.

- *Firms more dependent on external financing sources react differently to an external funding shock than firms that can rely on internal financing streams.*
  Basel III capital requirements can be seen as an exogenous shock to the external financing conditions of firms. Firms less dependent on financing through external sources might be less affected than their more dependent counterparts. This instance is reported in numerous studies and is, therefore one major source of endogeneity to control for. (S. Chava et al. (2013), Amore, Schneider, and Zaldokas (2013), Hombert and Matray (2016)) On top of that, firms that rely more on external finance sources are more often in the treatment group. To ease concerns of omitted variable bias and sample imbalances stemming from changes in external finance dependence and financing structure of firms, I include the net change in the capital as a matching variable to capture the funding strategy of a firm, as well as the cash and the leverage variables to ensure similar capital profiles across matched firms.

- *Firms may vary significantly in their investment strategy.*

  The other variables capture the financial structure and firm philosophy of firms and their funding demand, but they lack covering the firms' spending patterns. Changes in investment philosophies might affect the probability of being in the treatment group, as well as influence the reaction to external funding shocks. To ease imbalances in the investment strategy, I use spending in other investments as a matching variable to match firms with similar investment strategies.

3.6.2 Aggregated Loan-Level Covariates

- *Otherwise, comparable firms may vary in their usage of loans.*

  The former-mentioned leverage and net change in capital variables capture the difference in the usage of loans partially. How firms use their loans is inherently associated with their probability of being treated and their reaction to an external funding shock. To assess imbalances in the individual usage of loans, the average maturity per year of all loans granted to a given firm is incorporated as a matching criterion to ensure that matched firms use loans with similar average maturities and thus have to renegotiate loan terms similar often.

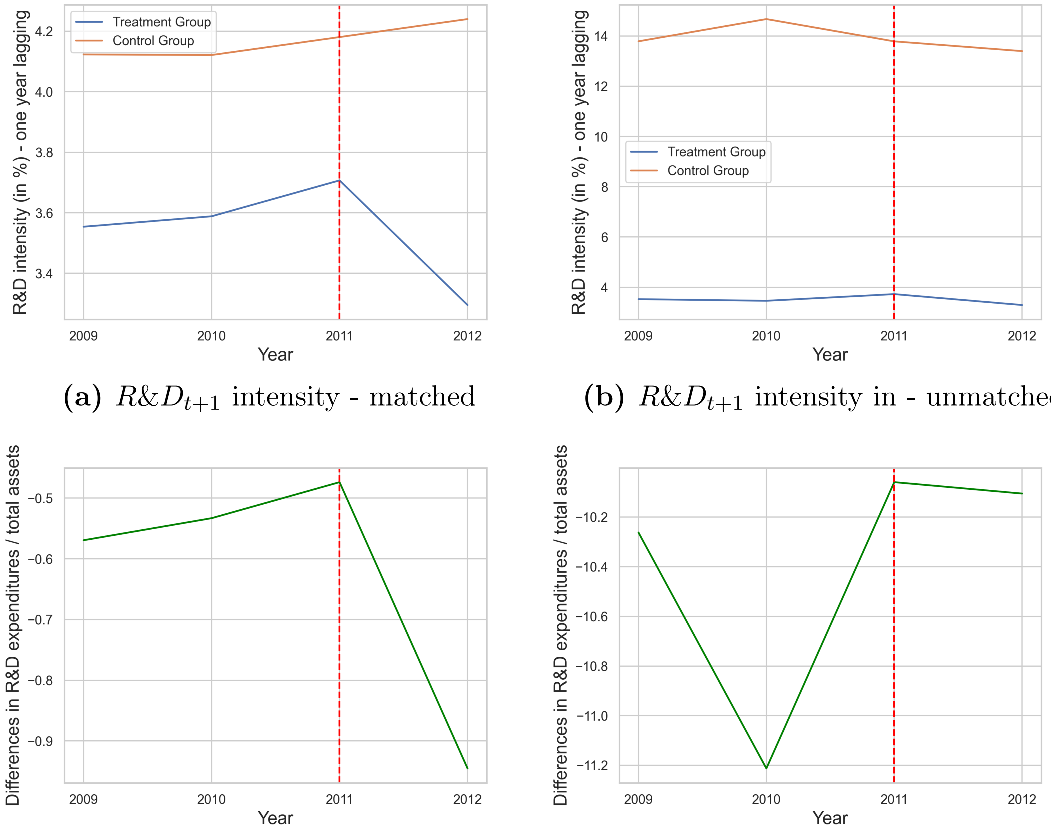
3.6.3 Aggregated Bank-Level Covariates

Bank-specific variables were considered initially but not included in the final matching and regression due to a lack of reliable data. The problem stems from the compounding effect of missing data when connecting databases. Bank-specific variables can only be matched if a firm borrows in a given year and the variable is present for this specific bank at this specific year. Each of the conditions is necessary and has a significant portion of missing values, resulting in a high number of missing values on the final firm-level dataset. Most of the variation, which can be captured via bank characteristics, is assessed by time, firm, or sector-time fixed effects, as well as general firm-specific variables like the leverage or the net change in capital.

3.7 Sample Construction: Propensity Score Matching

Without further alteration, the dataset is unbalanced, and the pre-treatment period's parallel trend assumption is violated. (See figure 2) After the initial cleaning, one-third of the observations are classified as the treatment group, while two-thirds remain in the control group. The two groups differ substantially in their mean level of research and development intensity, among other variables. (See table 9 in the Appendix)

Ryan, Burgess Jr., and Dimick (2015) argue that a propensity score matching, based on multiple possible control groups, can reduce the bias of estimates if the sample is unbalanced or the treatment is non-random and correlated with pre-intervention levels of included covariates. While not directly causally related, many firm- and loan-specific dimensions may influence the decision with which bank a given firm is building borrowing relationships, which is decisive for the treatment categorization.
The treatment and control group are matched, using a propensity score matching based on a number of variables, following Shipman, Swanquist, and Whited (2017); Dou and Z. Xu (2021); Koh and Reeb (2015). The matching algorithm is a standard one-to-one matching without replacement, using logistic regression as a scoring baseline regression with no caliper. The nearest corresponding firms are matched. For the matching, I consider the mean value of the firm- and loan-specific, time-varying variables discussed beforehand in section 3.6.1 between 2008 and 2011. I am using average values in the pre-treatment period to account for a regression to the mean problem, which may occur if the treatment and control groups are heavily different in their support. On top of that, it prevents the common problem of difference-in-differences approaches with covariates, which can be interpreted as outcome variables. (see Huntington-Klein (2021)) See Appendix section A.3.1 for a more detailed argument on the regression to the mean problem.
After matching, the differences in the key variables are reduced significantly (see table 10 in the Appendix) across all variables. On top of that, the matching improved remarkably on the issue of common pre-trends, as can be observed in figure 2. After matching, the dataset consists of 4145 observations, including 313 unique firm-to-firm matches. Visually and statistically, the common pre-trend assumption holds on the matched data sets.



(a) $R\&D_{t+1}$ intensity - matched **(b)** $R\&D_{t+1}$ intensity in - unmatched

(c) $R\&D_{t+1}$ int. change - matched **(d)** $R\&D_{t+1}$ int. change - unmatched

Figure 2: Pre-treatment trends with and without matching

4 Results

4.1 Baseline Regression: Research and Development Intensity

Table 1: Main specification with and without sector x time fixed effects
Research and development intensity and one and two-years projected into the future are the outcome variables. Samples were matched on a basket of covariates described in the sections beforehand. The two columns on the left side report results without sector x time fixed effects, and the two columns on the right include sector x time fixed effects. Standard errors are clustered across matched subclasses (firms).

	Dependent variable:			
	R&D int_{t+1}	R&D int_{t+2}	R&D int_{t+1}	R&D int_{t+2}
	(1)	(2)	(3)	(4)
Treat x Post (TxP)	-0.548^{***}	-0.625^{***}	-0.534^{***}	-0.591^{***}
	(0.195)	(0.224)	(0.193)	(0.219)
Year fixed effects	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
Sector trends			✓	✓
Additional covariates	No	No	No	No
Observations	4,145	3,928	4,145	3,928
R^2	0.008	0.010	0.026	0.029
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 1 contains the results for the baseline specification.

Column (1) illustrates that for firms, which borrow from affected banks, the announcement of Basel III capital requirements is associated with a 54.8 percentage point decrease in research and development intensity in $t + 1$. The coefficient is different from zero at a 1% significance level. The size of the estimate reflects a drop of 15.8% ($-0.548/3.47$) in research and development intensity, compared to the mean of research and development intensity of treated firms.

Column (2) shows the results for the outcome variable in $t + 2$. Borrowing from affected banks is associated with a 62.5 percentage point decrease in research and development intensity. Again, the coefficient is different from zero at the 1% significance level, and the economic magnitude of the coefficient remains largely unchanged. The 62.5 percentage points decrease in R&D intensity in $t + 2$ corresponds to an 18,1% drop from the mean R&D intensity of treated firms in t+2. The results with sector-time fixed effects are presented in columns (3) and (4). The results remain statistically robust and similar in economic magnitude. The results for $t+1$ and $t+2$ in columns (3) and (4) are statistically significant on the 1% level. The economic size of the coefficients remains similar for both considered outcome variables. Overall, table 1 indicates that the announcement of Basel

III capital requirements is associated with an average drop in R&D intensity, between 15% and 20% for treated firms, compared to the mean of R&D intensity for all periods, statistically highly significant across all specifications.

4.1.1 Linear Effects

Table 2: Linear treatment
This table reports the results for the stepwise treatment variable. In 2012, the post variable is set to 1, incrementing by 1 for each year passing. Research and development intensity one and two-year projected into the future are the outcome variables. Standard errors are clustered across matched subclasses (firms).

	Dependent variable:	
	R&D int_{t+1}	R&D int_{t+2}
	(1)	(2)
Treat x Years since	-0.186^{***}	-0.205^{**}
	(0.067)	(0.082)
Year fixed effects	✓	✓
Firm fixed effects	✓	✓
Other covariates	No	No
Observations	4,145	3,928
R^2	0.007	0.009
Note:	*p<0.1; **p<0.05; ***p<0.01	

In late 2010, when the announcement was published, banks were not entitled to act immediately, even if they were potentially affected. The implementation phase lasted until the beginning of 2014 when the capital requirements became binding. To grasp the ongoing effect of this treatment, I create a step-wise increasing measure of the number of years since the announcement. This approach is equivalent to Kerr and Nanda (2009) and Amore, Schneider, and Zaldokas (2013). Table 2 contains the results of this specification. Again, year and firm fixed effects are included, and no other covariates. For research and development intensity in $t + 1$ as an outcome variable, the linear, step-wise coefficient is statistically significant at the 1 percent level, while for the research and development intensity in $t + 2$, the coefficient is different from zero on the 5% significance level. Each year passing since the announcement in 2011 is associated with a 18.6 percentage point decrease in R&D intensity in t+1 (Column (1)) for firms that borrow money from treated banks. The 18.6 percentage point decrease is equivalent to an average yearly drop of 5.4%, compared to the mean of treated firms.

The results for the R&D intensity in t+2 indicate the same economic magnitude of results.

(Column (2)) The regulation announcement is associated with a yearly drop in R&D intensity in $t+2$ of 5.9% relative to the mean of R&D expenditures.

In the Appendix, the results for a dynamic difference-differences approach are displayed in table 11, which confirms the findings for the step-wise estimator, but depicts a more nuanced result. As table 2 indicates, the associated effect grows over time but is more heterogeneous and non-linearly. One interpretation of the two result tables might be an immediate effect of the announcement, followed by waves of action on the bank and ensuing on the firm side.

4.2 Heterogeneous Effects

4.2.1 External Finance Dependence

Table 3: Industry level external finance dependence:
Regression results for subsamples of the initial sample. Industry-level financial dependence is computed by using net change in capital. The 70% percentile of net change in capital of firms is used as the threshold value. All industries, above the threshold in their mean values before the treatment period, are considered highly external finance dependent. All sectors, with mean net changes in capital below the threshold in the pre-treatment period, are considered low external finance dependent. Since firms are distributed asymmetrically across industries, I could not ensure balanced datasets when considering a sector-based approach. The 70 % percentile ensured the most balanced dataset. Standard errors are clustered across matched subclasses (firms).

	Dependent variable:	
	R&D int_{t+1}	
	High financial dependence	Low financial dependence
	(1)	(2)
Post x Treat	-1.381^{***}	-0.221
	(0.510)	(0.190)
Year fixed effects	✓	✓
Firm fixed effects	✓	✓
Additional covariates	No	No
Observations	1,591	2,983
R^2	0.017	0.006
Note:		*p<0.1; **p<0.05; ***p<0.01

One distinguishing factor of the effect of the Basel III capital requirements announcement on firms might be the dependence on external financing (also referred to as external finance dependence or financial dependence) sources. If the announcement of the capital requirements did act as a shock to external funding, firms that are more dependent on external financing sources should be more affected.

Firms are classified by the average external finance dependence of the sector they operate in. All classifications into groups are decided on, based on pre-treatment averages, using the net change of capital as a proxy variable for the usage of external financing sources. The sector averages are being compared with a threshold, the 70% percentile of the net change in capital across all sectors in the pre-treatment period, following Amore, Schneider, and Zaldokas (2013). Sectors, which are on average more external finance dependent than the threshold, are considered in the highly external finance dependent group. All other sectors are considered in the low external finance dependent group. In the Appendix, section A.1 contains a description of how net change in capital is computed.

Table 3 shows the results for the respective sub-samples of the initial, matched sample. Firstly, the difference in point estimates is noticeable. While the Basel III capital requirements announcement is associated with a drop in R&D intensity of 138.1 percentage points for highly external finance-dependent sectors, it is associated with a 22.1 percentage point drop for sectors that are less dependent on external financing sources. The drop of 138.1 percentage points represents a shrinkage of 25.1%, compared to the mean of treated firms in highly financially dependent sectors. The point estimate in column (1) is statistically significant on the 1% percent level. For less external finance-dependent sectors, the drop of 22.1 percentage points translates to a drop of 9.1% compared to the mean of treated firms in this sub-sample, not statistically significant on any level.

To examine the differences between the two groups, I consider a triple difference-in-differences approach (see Angrist and Pischke (2009)), confirming a difference between the two reported coefficients, reported in table 12. The triple difference-in-differences approach quantifies the difference in estimates with 115.7 percentage points, significant at the 5% level. Further details on the specification can be found in section A.5 in the Appendix. Overall, this section does provide robust evidence supporting the second hypothesis of systematic differences in the responses between firms based on their external finance dependence.

4.2.2 Firm Age and Market to Book Ratio

Table 4: Firm age & market-to-book ratios:
This table presents results for two subsets of the matched sample based on firm age and market-to-book ratio. To determine the firm age, I use the first occurrence in the Compustat dataset as the birth year and calculate the age accordingly. Firms already present in the Compustat dataset before 2006 are categorized as older firms, while firms with their first occurrence after 2006 are classified as younger. These results are displayed in the first two columns. Regarding the market-to-book ratio categorization, firms with a mean market-to-book ratio above 1.5 in the pre-treatment period are placed in the high market-to-book ratio group, while firms below the threshold are assigned to the low market-to-book ratio group. A market- to-book ratio of 1.5 is the median for the matched sample. Standard errors are clustered across matched subclasses (firms).

	Dependent variable:			
	R&D int_{t+1}			
	Older Firms	Younger Firms	High M/B	Low M/B
	(1)	(2)	(3)	(4)
Treat x Post (TxP)	-0.504^{***}	-0.708	-0.957^{***}	-0.160
	(0.179)	(0.454)	(0.351)	(0.181)
Year fixed effects	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
Additional covariates	No	No	No	No
Observations	2,617	1,957	2,555	2,019
R^2	0.010	0.011	0.013	0.003
Note:				*p<0.1; **p<0.05; ***p<0.01

Besides external finance dependence, other sources of heterogeneity are considered. In this section, I focus on the firm age and the market-to-book ratio of firms as a source of heterogeneity. As described in section 3.6.1, more mature firms tend to be different in terms of financing structure compared to younger firms. Older firms, while generally higher levered and having longer and more diversified lending relationships with banks, might be affected differently than younger firms, which rely more on equity and venture capital to finance business but are generally more research and development intense. The matched sample is split by birth year in 2006. All firms with an entry in the Compustat dataset before 2006 are considered older firms, and all other firms are considered younger firms. The point estimate for older firms is lower in absolute terms for older firms than for younger firms. For older, treated firms, Basel III capital requirements announcements are associated with a 15.9% decrease in R&D intensity, relative to the mean of the respective treated firms, statistically significant on the 1% level. For younger firms, the point estimate reflects an 18.4% decrease in R&D intensity relative to the mean of younger, treated firms. The point estimate is not statistically significant, with a p-value of 11%. Again, the difference between the two groups is assessed using a triple difference-in-difference approach. The results are reported in table 12 in the Appendix. The interaction coefficient indicates no statistically significant difference between the two groups. The two groups are quite similar in their relative reaction to the treatment. For older firms, the effect seems to be less volatile and thus more robust statistically.

Another source of heterogeneity might be the market-to-book ratio, which generally represents the relative price of a given firm and its long-term growth philosophy. Firms with different M/B ratios differ across many dimensions. The main aspect of including this metric as a source of heterogeneity is that companies with higher market-to-book ratios are more focused on innovation and more R&D intensive since they rely more on projected future cash flows for their firm valuation. Firms are split at a market-to-book ratio of 1.5, representing the median value in the dataset in the pre-treatment period. Companies with higher market-to-book ratios have a higher absolute point estimate of -95.7 percentage points, compared to -16 percentage points for the estimate of interest for lower market-to-book ratios. The point estimate for the lower market-to-book ratio group is statistically insignificant, with a p-value of 37.8%. For the high market-to-book ratio group, the coefficient of interest is statistically significant at the 1% level. One aspect to point out is that for the subset with high-market-to-book ratios, the point estimate is not only higher in absolute terms compared to the baseline results. When assessing the change in R&D intensity in comparison to the mean of the respective treatment group, the 95.7 percentage point drop translates to a drop of 21.1% compared to the mean. For this particular split of the sample, the triple difference-in-differences confirms the difference between these two treated groups, significant on the 5% significance level. (See Appendix table 12) The difference between these two groups is quantified with 79.6 percentage points by the triple difference-in-differences approach. The results in this section indicate that firms with high market-to-book ratios drive the baseline effect, while firm age is not a distinguishing factor when assessing which firms are affected.

4.2.3 Loan Maturity Dependence

How firms use their loan exposure might also be a critical source of heterogeneity for how the treatment translates to firm-level decisions about research and development investments. One approximation of the usage of loans is the average loan maturity per year.

Table 5: Loan Maturities
Regression results for the initial matched sample subsamples, based on the average loan maturity. Samples are split based on the average pre-treatment loan maturity for each firm. Firms that are on average above the median loan maturity in the pre-treatment period are placed in the high-maturity group and firms below the threshold are in the low-maturity group. Standard errors are clustered across matched subclasses (firms).

	Dependent variable:	
	R&D int_{t+1}	
	Lower Maturities Loans	Higher Maturities Loans
	(1)	(2)
Treat x Post (TxP)	-1.088^{***}	-0.100
	(0.418)	(0.155)
Year fixed effects	✓	✓
Firm fixed effects	✓	✓
Additional covariates	No	No
Observations	2,304	2,323
R^2	0.014	0.008
Note:		*p<0.1; **p<0.05; ***p<0.01

The matched sample is split, based on the average yearly loan maturities, in the time period predating the announcement. All firms which are on average above the median loan maturity are considered in the higher maturity group, while all firms, which are on average below the threshold, are considered in the lower maturity loan group. The point estimates of the effect of Basel III announcements regarding higher capital differ substantially between groups. On the one hand, for the higher maturity group, the point estimate is -10 percentage points, with a p-value of 51.8%, providing no evidence for any effect. On the other hand, the point estimate for the lower maturity group is -108.8 percentage points, significant on the 1% level. In relation to the mean R&D intensity of the treatment group for the lower maturity sub-sample, the announcement is associated with a 22.7% drop in R&D intensity compared to the already stated 15.8% at the baseline level. The substantial difference between the coefficients of the two maturity groups is reconfirmed by the triple difference-in-differences approach, indicating a group difference of 101.4 percentage points, significant on the 5% level. These results suggest that firms, which on average use loans with lower maturities, are affected more by the announcement, while firms with higher average maturities are unbothered. One possible explanation is that firms with lower maturities experience a higher number of renegotiation of loans in a small time frame around the treatment, thus more possibilities for affected banks to pass on the external funding shock to these firms. Higher maturities ensure longer timescales of funding and, therefore, more time to substitute foregone borrowing opportunities of treated firms in the higher maturities group.

4.3 Robustness of Results

4.3.1 Bank relationship composition

Table 6: Different relative shares of treated lenders
Regression results for subsets of the initial sample, based on the the relative share of treated banks of all lenders for each firm in the pre-treatment period. Treated firms and their respective match are split into three groups: The sub 50% group lends from more non-treated banks than treated banks in the pre-treatment period. The above 50% group lends from more treated banks than from non-treated banks. The 100% group only lends from treated banks in the pre-treatment period. Matched observations from the control group are classified according to this treatment match. Standard errors are clustered across matched subclasses (firms).

	Dependent variable:		
	R&D int$_{t+1}$		
	Sub 50% group	Above 50% group	100% group
	(1)	(2)	(3)
Treat x Post (TxP)	−0.066	−0.764***	−1.018**
	(0.235)	(0.267)	(0.411)
Year fixed effects	✓	✓	✓
Firm fixed effects	✓	✓	✓
Additional covariates	No	No	No
Observations	951	3,623	2,097
R^2	0.011	0.010	0.016
Note:			*p<0.1; **p<0.05; ***p<0.01

As a first robustness check, I address one peculiarity of the treatment group creation process. Firms with varying relative shares of treated banks are classified as treated if they have at least one relationship with a treated bank. Firms with multiple bank relationships and only a small fraction involving treated banks may better compensate for the loss of borrowing from treated banks by shifting to established relationships with other banks. Additionally, firms that solely borrow from one treated bank might rely more on this single funding source. These factors could potentially dilute and bias the main results. To assess these differences, I create different bins, based on the matched sample, depending on firms' relative share of exposure to treated banks in the pre-treatment period. The bins represent firms with less than 50% treated banks as lenders, banks with more than 50% treated banks as lenders, and firms with only treated banks as lenders.

For each bin, the matched sub-classes of non-treated firms are added to the sample to ensure that matched observations are in the same sub-sample.

The effect for firms with less than half their banking relationships with treated banks is nearly zero, with a point estimate of -6.6 percentage points and a p-value of 77.8%. These firms can substitute treated banks with other, already existing bank relationships or internal funds. For the above 50% group, treated firms reduce their R&D intensity after the announcement on average by 76.4 percentage points, significant on the 1% level. The economic size in relation to the mean is higher than the reported effect in the baseline regression coefficient, indicating a 20.4% drop in R&D intensity relative to the mean of treated firms. For the 100% group in column (3), the point estimate is -101.8 percentage points, statistically significant on the 5% level. In relative terms, firms who only borrow from one treated bank do, on average, shrink their R&D intensity in response to the Basel III capital requirement announcement by 24.8%, relative to their pre-treatment mean of R&D intensity. This finding suggests that firms exclusively borrowing from treated banks might not have been able to substitute treated lenders effectively. As a result, these firms reduced their relative R&D exposure. The robustness check shows the expected results, which reassures the empirical work of the previous chapters. On top of that, this robustness check indicates that the baseline regression results are diluted by firms that have less than 50% treated banks relationships. Results in the baseline regression can be interpreted as a conservative measurement of the effects of the Basel III capital requirement announcements on firm innovation inputs.

4.3.2 Threshold dependence

The decision to use 8 % tier 1 capital ratio between 2007 and 2011 as a threshold value to decide on the treatment group is based on economic intuition and comparable papers. (Hendricks et al. (2023))

Other researchers who consider the effect of Basel III on banks' lending behavior report that the effect is diminished for banks, which had an 11% tier 1 capital to risk-weighted assets ratio before Basel III. (see for example Deli and Hasan (2017)) To incorporate this research and to examine the robustness of effects for different treatment thresholds, the whole matching process, described in the previous chapters, is reapplied with two different thresholds of tier 1 capital ratio to risk-weighted assets: 7% and 11%. The 7% threshold represents the proposed minimal tier 1 capital ratio in the initial announcement, while

Table 7: Results different threshold values
This table reports results for alternative treatment thresholds. In the baseline regressions, banks with a tier 1 capital ratio below 8% at least once in the pre-treatment period are considered in the treatment group of banks and mapped onto firms. This threshold is reduced to 7%, for which the results are reported in the leftmost two columns, and increased to 11%, for which the results are reported in the two rightmost columns. For both thresholds, a new matching is performed with the same pre-treatment covariates. Standard errors are clustered across matched subclasses (firms)

	Dependent variable:			
	R&D int_{t+1} 7% threshold	R&D int_{t+2} 7% threshold	R&D int_{t+1} 11% threshold	R&D int_{t+2} 11% threshold
	(1)	(2)	(3)	(4)
Treat x Post (TxP)	-0.437^{**}	-0.416^{**}	-0.502	-0.433
	(0.207)	(0.180)	(0.318)	(0.324)
Year fixed effects	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
Additional covariates	No	No	No	No
Observations	3,405	3,405	5,551	5,551
R^2	0.006	0.006	0.003	0.002
Note:				*p<0.1; **p<0.05; ***p<0.01

11% does represent the ratio at which the effect is found to be diminished by other researchers.

Table 7 displays the results. As one might notice from the number of observations, higher thresholds for treated banks lead to more firms in the matched samples. First, I discuss columns (1) & (2), which present the results for the 7% cut-off value. The statistical significance and the economic size of results remain near the values of the baseline regression, with a relative drop in R&D intensity in t+1 of 14.3% percent, compared to the mean of treated firms. The results of the 7% threshold could suggest that opting for marginally higher thresholds in the baseline case was a justified choice, as the results are not fundamentally different. For the 11% threshold, the results reported in columns (3) and (4) are statistically less significant, with a p-value of 11.4% for the research intensity in t+1 and 18.14% for the research intensity in t+2. The findings are in line with Deli and Hasan (2017). For a threshold of 11%, I can conclude that this thesis finds no significant effect on firm innovation input of bank capital requirement announcements, even though the p-values after clustering are not far off the significance range.

4.3.3 Different outcome variables

As a last robustness check, I incorporate different outcome variables on the same matched sample used in the main specification. In particular, a lagging R&D intensity, with sales figures in the denominator, is considered.

The results can be found in table 13 in the Appendix. The point estimates for the one-year lagging research intensity and the two-year lagging research intensity, using sales figures in the denominator, even though they have the same sign as the baseline regression, are not statistically significant at any level. These results do therefore offer no additional robustness to the baseline results. Again, the lower significance does support the hypothesis, that only banks with initial tier 1 capital ratios near the proposed minimum requirement are affected by this announcement.

5 Discussion

Differentiating the results of different papers assessing the effects of bank regulation on firm innovation is complex since the type of regulation and the type of firms considered decisively influence the findings. Nonetheless, my results align with the existing literature, discussed in section 2. The most similar paper to my work from Dou and Z. Xu (2021) does report a similar, adverse effect on innovation inputs when examining the effect of an accounting-driven funding shock for banks on firms. On top of that, the result fits well within the extensive research on the geographic deregulation of financial institutions, which establishes a differentiated association between innovation outputs and financial regulation. This thesis does expand on these findings by establishing an association between financial regulation and innovation inputs rather than outputs. The results in the robustness check (section 4.3) are coherent with previous papers (Hendricks et al. (2023), Deli and Hasan (2017)), which report a diminishing effect of the Basel III capital requirements for banks that are pre-treatment above a threshold of 11% tier 1 capital ratio. Other researchers (see among others Hombert and Matray (2016), Cornaggia et al. (2015)) have already described that the intensity of the lending relationship plays a decisive factor in how funding shocks are transmitted to firms. My results show that firms with a higher relative share of treated banks and lower average maturities of loans in their portfolio suffer more than their counterparts, which confirms the general statement that lending relationships play a crucial role. External finance dependence, firm age, and Market-to-book ratio (or related concepts such as innovativeness of firms) as a source of heterogeneity are also considered by numerous other researchers (Cornaggia et al. (2015), Amore, Schneider, and Zaldokas (2013), Hombert and Matray (2016), Dou and Z. Xu (2021)). The expectation that firms with higher dependence on external financing sources are affected more by changes in their funding conditions both in the positive (Cornaggia et al. (2015), Amore, Schneider, and Zaldokas (2013)) as well as in the negative sense (Dou and Z. Xu (2021), Hombert and Matray (2016)) is also a finding of this thesis. I cannot confirm a robust divergence in effects between younger and older firms, as suggested by other researchers. (Cornaggia et al. (2015); Hombert and Matray (2016)) The results are robust from an econometric standpoint. For the baseline regression, omitted variables need to be present on the firm level and vary over time in order to be problematic for the identification strategy. On top of that, a possible omitted variable has to be different than the matching variables or be a matching variable, which

is highly variable over time. During the pre-treatment period, a number of confounding events took place, which I want to acknowledge. The start Global Financial Crisis took place in the pre-treatment period. While the real estate market plunged, policymakers did introduce a number of actions in order to stabilize financial markets and the real economy. I cannot rule out that the Global Financial Crisis did influence the matching process, even though I am being cautious in using average values of firm-specific variables in the matching process to overcome the numerous competing forces during this time span. In the middle of 2010, in response to the Global Financial Crisis, the United States implemented the Dodd-Frank Act. The Dodd-Frank Act comprises a number of regulations, capital requirements, and investment regulations, among other aspects. The timing of the regulation, the middle of 2010, might serve as a confounding event. The minimum tier 1 capital ratio requirements for all banking institutions were 4%. For banks that conducted more complex financial deals, the ratio was set to be 6%. In both cases, the lower threshold resulted in fewer affected banks, which in turn does lead to fewer affected firms. The dynamic difference-in-differences estimation in table 11 does not report an effect for a placebo treatment in 2010 or 2011. Even though I cannot dismiss the possible longer-lasting effects of the Dodd-Frank Act, I argue that even if the Dodd-Frank Act serves as a confounding event, the results are still usable, since both regulations did affect banks similarly. While the causal statements about the relationship between the Basel III capital requirements announcement and the drop in innovation intensity from affected firms might not be possible, this thesis still quantifies the real effects of banking regulation on banks' tier 1 capital ratio. In 2010, SFAS 166/167 came into effect to strengthen accounting and consolidation regulations for banks. Banks were forced to consolidate $363 billion of previously off-balance sheet securitized loan assets, which affected the bank's funding adversely. The treated banks and hence the treated firms are not necessarily the same as for the Basel III announcement, and other researchers (Dou and Z. Xu (2021)) conclude that Basel III did not confound SFAS 166/167. While I can not rule out an effect on firms completely, a similar argument as for the Dodd-Frank Act does apply.
One aspect to point out, which is oftentimes overlooked in empirical papers, which use matching algorithms to control for covariates, ensure parallel trends and create balanced samples, is the matching dependence. Many researchers in well-published journals use widely different matching variables for similar treatment matching. During the conduction of the matching, I noticed a strong dependence of the results on the matching variables.

For further research based on this work, I want to highlight the lack of innovation output variables. Patent and citation data of the firms would greatly enhance the information value of this research. On top of that, ensuring better data availability by incorporating different databases and increasing the matched sample size, expanding on other countries, and including private firms could improve the external validity of the results. One might also assess different matching samples, their implications on the parallel trends assumption, and their influence on the results. Moreover, there could be more research on the transmission channel of Basel III. Other papers, in particular, Dou and Z. Xu (2021), already provide evidence of the transmission of financial regulation onto the real economy through the lending channel, but the literature lacks extensive evidence for Basel III. While pointing out the heterogeneity of the effect, my thesis does not decompose the findings further, which might also be a starting point for further research.

6 Conclusion

In this thesis, I examine the effect of the announcement of higher tier 1 capital requirements for banks on firm innovation inputs in the United States. The hypotheses tested in this thesis are that the announcement of higher capital requirements for banks did affect firm innovation input adversely and that this effect is heterogeneous across multiple firm- and loan-level dimensions. I exploit the circumstance that some banks are potentially affected by the proposed new regulation while others are already well-enough-capitalized and, therefore, unbothered. Three different data sources are considered in order to map the treatment from a bank to a firm level. I find that the announcement of higher capital requirements for banks is associated with an average reduction of R&D intensity of 15.8% compared to the mean R&D intensity of firms that borrow from affected banks. Confounding events cannot be ruled out entirely, but I argue that they do not harm the overall finding of this piece of research. Regarding the first hypothesis, I can conclude that firms with ties to worse-capitalized banks did shrink their research and development intensity following the announcement, even though the announcement might not entirely drive the effect. The effect is only present if the deciding treatment threshold is set near the minimum tier 1 capital ratio of 7% and vanishes if thresholds of more than 11% are considered. The findings are heterogeneous across many dimensions. Firms with higher market-to-book ratios, as well as more external finance-dependent firms and firms with shorter average maturities of loans, are affected more by the transmission of this funding shock on the bank level. I can confirm hypothesis number two, that the effect is heterogeneous across firm characteristics. While this thesis does only provide first empirical evidence on how Basel III affected firm innovation potential in the United States, it leads me to the conclusion that policymakers should take real effects into consideration when regulating banks.

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A Appendix

A.1 Variable Description

Table 8: Variable Description

Variable Name	**Description**	**Source**
Research and Development Intensity (R&D Int.)	Relative value of research and development expenditures compared to total assets (%)	Compustat
Treatment	A binary indicator variable at the firm level. It takes the value 1 for firms that borrow from treated banks at least once during the pre-treatment period. Treated banks are those whose tier 1 to risk-weighted asset ratio is reported below the 8% threshold of tier 1 capital ratio at least once during this period	Compustat & Dealscan
Cash Flow	Ratio of the sum of income before extraordinary items and depreciation & amortization to total assets (%)	Compustat
Market-to-Book Ratio (MB)	Ratio of the year-end market value of a given firm minus common equity value plus the multiplication of the number of common shares outstanding and the closing price per share to total assets (%)	Compustat
Leverage	Represents the proportion of debt in current liabilities and long-term debt to total assets (%)	Compustat
Cash Holdings	Represents the ratio of cash holdings to total assets (%)	Compustat
Net Change in Capital	Sum of changes in total debt, outstanding shares multiplied by share price, and convertible debt and preferred stock changes, divided by total assets (%)	Compustat
Other Investments Sum	Aggregate of capital expenditures and acquisitions (log-transformed)	Compustat
Sales Growth	Calculated as the difference between the natural logarithms of sales at time t and $t-1$	Compustat
Firm Age	Measured in years from a firm's first appearance in the Compustat dataset	Compustat
Percentage of treated lenders	Ratio of treated lenders, divided by the number of total lenders in the pre-treatment period. (in %)	Dealscan, Compustat
General industry identifier (GIND)	Utilizes the Global Industry Classification Standard (GICS) to identify a firm's sector and sub-sector.	Compustat
Average Loan Maturity per Year	Represents the mean loan maturity for loans received within a given year	Dealscan

A.2 Descriptive Statistics

Table 9: Descriptive statistics for treatment and control group before matching
Units are provided in brackets
(1): Mean untreated observations
(2): Standard deviation untreated observations
(3): Mean treated observations
(4): Standard deviation treated observations

Variable; Measure	**(1)**	**(2)**	**(3)**	**(4)**
R&D int. (%)	14.41	17.85	3.39	5.05
Avg. matur. p.a. (years)	4.28	15.02	18.30	26.24
Firm age (years)	17.52	14.54	31.46	19.13
Market to Book ratio (%)	6.01	16.92	2.35	2.32
Cash Flow (%)	-33.19	217.68	7.20	13.30
Sales Growth (%)	0.1	0.71	0.05	0.23
Oth. inv. sum (%)	1.25	3.15	4.42	2.08
Leverage (%)	33.33	270.85	24.07	17.37
Net change capital (%)	-1.04	32.10	0.06	0.38
Cash holdings (%)	25.68	24.75	11.02	11.05

Table 10: Descriptive statistics for treatment and control group after matching
Units are provided in brackets
(1): Mean untreated observations
(2): Standard deviation untreated observations
(3): Mean treated observations
(4): Standard deviation treated observations

Variable; Measure	**(1)**	**(2)**	**(3)**	**(4)**
R&D Int. (%)	4.33	6.16	3.42	5.25
Avg. Matur. p.A. (years)	10.17	21.77	18.56	26.09
Firm Age (years)	26.36	17.77	31.93	18.91
Market-to-Book ratio (%)	2.39	2.58	2.22	2.17
Cash Flow (%)	7.20	12.57	7.34	12.43
Sales Growth (%)	0.04	0.22	0.04	0.22
Oth. Inv. Sum (%)	3.45	2.55	4.43	2.09
Leverage (%)	20.20	18.89	23.56	17.09
Net Change Capital (%)	0.04	0.36	0.05	0.36
Cash Holdings (%)	13.70	12.24	11.37	11.19

A.3 Difference-in-differences

A.3.1 Matching: Reversion to the mean

Daw and Hatfield (2018a) describe the factors which lead to a regression to the mean problem. The problem arises if one matches observations on baseline levels of variables, which have different distributions. Extreme values might be matched, which regress back to the mean after the matching period, hence biasing the estimation.

In my baseline, a one-to-one propensity score matching without replacement, a regression to the mean problem might occur if not taken into account carefully since treatment and control groups have different supports across many matching variables. A high serial correlation of the matching variable reduces the regression-to-the-mean problem since extreme values are less likely to revert back to the mean. In the matching process, I am using average values of the covariates between 2008 and 2011 to account for the described problem. This process is similar to the synthetic control group setting described by Daw and Hatfield (2018b). Using mean values reduces the problem of extreme values since outliers are unlikely to occur persistently over the averaging.

A.3.2 Clustering standard errors for matched samples

Clustering standard errors after using a propensity score matching differs from plain clustering of standard errors. The matching process introduces new forms of variability, which must be considered: The estimation of the propensity score, the matching, and the sampling variability. (Greifer (2020))

Standard errors that ignore the matching pre-processing of data are, in general, not valid if the regression is misspecified. A misspecification defines the scenario when the estimation regression does diverge from the true, underlying data-generating process. A valid assumption is that I do not fully capture the true data-generating process in this thesis. Abadie and Spiess (2022) show analytically that in this scenario, clustering standard errors across matched subclasses produces standard errors which are robust to misspecification of the outcome regression. This approach is reconfirmed in a simulation study by Austin and Small (2014).

A.4 Dynamic Difference-in-Differences

Table 11: Dynamic difference-in-differences
Dynamic difference-in-difference on the matched sample. Standard errors are clustered across subclasses (firms).

	Dependent variable:
	R&D int_{t+1}
Treat x Post (TxP)	-0.627^{***}
	(0.235)
Treat x $\text{Post}_{t-1}(TxP)$	0.116
	(0.241)
Treat x $\text{Post}_{t-2}(TxP)$	0.189
	(0.177)
Treat x $\text{Post}_{t+1}(TxP)$	0.064
	(0.148)
Treat x $\text{Post}_{t+2}(TxP)$	-0.340^{*}
	(0.192)
Treat x $\text{Post}_{t+3}(TxP)$	-0.068
	(0.212)
Year fixed effects	✓
Firm fixed effects	✓
Other covariates	No
Observations	4,574
R^2	0.009
Note:	*p<0.1; **p<0.05; ***p<0.01

A.5 Triple Difference-in-Differences

This section contains the result tables for the interaction regressions, described in section 4.2 about heterogeneous effects. The triple difference-in-differences specification is used to examine group differences, which are indicated in the respective chapters of the heterogeneous effects. All other variable aspects besides the added interactions are held constant, following the baseline specification described in section 3.2. In addition to the interaction between the post, the treatment, and the respective source of heterogeneity, an interaction between the variable post and the respective source of heterogeneity is considered in the specification. (See Angrist and Pischke (2009)) The included fixed effects absorb other possible interactions.

Table 12: Triple difference-in-differences results for heterogeneous effects regressions. Specifications are conducted in line with the baseline regression, adding the respective interactions to assess group differences. Non-informative interactions are omitted in the results. Standard errors are clustered across matched subclasses (firms).

	Dependent variable:			
	R&D int_{t+1}			
	(1)	(2)	(3)	(4)
Treat x Post (TxP)	−0.221	−0.709	−0.068	−0.162
	(0.190)	(0.453)	(0.151)	(0.182)
TxP x Fin. Dep.	-1.157^{**}			
	(0.543)			
TxP x Age		0.203		
		(0.487)		
TxP x Loan Mat.			-1.014^{**}	
			(0.444)	
TxP x MB				-0.796^{**}
				(0.395)
Year fixed effects	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
Additional covariates	No	No	No	No
Observations	4,574	4,574	4,574	4,574
R^2	0.012	0.008	0.011	0.010
Note:		*p<0.1; **p<0.05; ***p<0.01		

A.6 Robustness checks

Table 13: Alternative outcome variables
This table reports the results for alternative outcome variables on the same matched sample. The different outcome variable is based on the sales figures instead of total assets in the denominator. Standard errors are clustered across matched subclasses (firms).

	Different outcome variables	
	R&D $\text{int}_{t+1}(Sale)$	R&D $\text{int}_{t+2}(Sale)$
	(1)	(2)
Treat x Post (TxP)	−0.106	−0.220
	(0.112)	(0.155)
Year fixed effects	✓	✓
Firm fixed effects	✓	✓
Observations	4,345	4,106
R^2	0.020	0.015
Note:		*p<0.1; **p<0.05; ***p<0.01

"Ich versichere hiermit, dass ich die vorstehende Masterarbeit selbstständig verfasst und keine anderen als die angegebenen Quellen und Hilfsmittel benutzt habe, dass die vorgelegte Arbeit noch an keiner anderen Hochschule zur Prüfung vorgelegt wurde und dass sie weder ganz noch in Teilen bereits veröffentlicht wurde. Wörtliche Zitate und Stellen, die anderen Werken dem Sinn nach entnommen sind, habe ich in jedem einzelnen Fall kenntlich gemacht."

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