ESSAYS ON FINANCIAL INSTITUTIONS' RESPONSE TO CLIMATE AND TARIFF RISKS

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This dissertation consists of three essays in the areas of financial economics and climate finance, examining the response of financial institutions to environmental risks and tariff changes.

The first essay investigates if banks pay attention to the investment preference of ESG-committed mutual funds and issue loans at preferable terms to firms with higher ESG ownership. I use firm-level share fraction of US PRI (Principles for Responsible Investment) mutual funds as a proxy for ESG ownership. I find that even though PRI ownership does not improve firm ESG profile in the following year, banks on average significantly increase the amount and reduce the interest rate of new loans issued to firms with higher PRI ownership. I also show that PRI ownership is not likely to be a proxy for firm credit risk or stock return and banks' response is largely driven by banks that exhibit more environmental awareness. Overall, the results show that banks factor in borrowers' ESG ownership structure when evaluating new loans.

In the second essay, my co-authors and I study the existence and effect of local bias of US mutual funds on firms affected by hurricane landfalls. We find that local funds increase normalized shares of disaster-zone firms in the disaster quarter relative to non-local funds. With-in fund style analysis also suggests the existence of local bias. Compared to non-local funds, local funds increase the portfolio weight of firms headquartered in disaster-zone counties. The investor loyalty of local funds following natural disasters does have real economic impacts on disaster-zone firms. Preliminary analysis shows disaster-zone firms with higher proportion of shares held by local funds prior to disasters

tend to have higher excess return and make more short-term investment in the disaster quarter, thus potentially having better financial recovery from disasters.

The third essay studies the effects of industry-level tariff reductions on bank new lending and banks' existing borrowers one year after the tariff reductions. High-exposure banks decrease new lending to borrowers in the affected industries relative to borrowers in the unaffected industries. Using two measures of prior bank-firm relationship, I find that banks decrease new lending only to non-relationship borrowers and actually support relationship borrowers in the affected industries. Moreover, there is a spillover effect from firms in the affected industries to unaffected industries through the bank-firm network. Existing borrowers in the unaffected industries tend to increase cash holding and decrease R&D expenditure as a result of enhanced bank monitoring. Overall, these findings suggest that banks can act as intermediaries for shock transmission from affected to unaffected borrowers, resulting in real impacts on unaffected borrowers.

BIOGRAPHICAL SKETCH

Yimeng Tang is currently in her fifth year of study in the Department of Economics at Cornell University. Her areas of specialization include financial economics, corporate finance, and asset pricing. She is interested in understanding how financial institutions respond to risks associated climate and international trade.

Before coming to Cornell, she studied at Raffles Institution in Singapore for four years and double majored in Mathematical Economic Analysis and Statistics at Rice University.

This document is dedicated to all Cornell graduate students.

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CHAPTER 1

ESG OWNERSHIP STRUCTURE AND BANK LOANS

1.1 Introduction

Over the last decades, financial institutions, such as banks and mutual funds, are paying more and more attention to responsible financing in order to minimize the costs of environmental and social externalities. Banks put in more efforts in screening the ESG characteristics of the borrowers and funds demonstrate their commitment to responsible investing through participating in sustainable investment networks. In the context of the growing emphasis on responsible investing, I study if banks pay attention to the investment preference of ESG-committed mutual funds and issue loans at more preferable terms to firms with higher fraction of shares owned by ESG-committed mutual funds.

To study lenders' response to firms' ESG ownership structure, I first investigate if higher ESG ownership influences firms' ESG performance in the following year. This question is crucial to understand the underlying reasons behind lenders' response and shed light on the debate on "greenwashing": do green funds really care about ESG or just try to attract more capital? I then estimate the effects of firms' ESG ownership structure on bank lending amounts and loan spreads to see if banks factor in this ESG-related information when issuing new loans. Next, I show that ESG ownership structure is not a proxy for omitted variables such as firm credit risk or stock return and the effects are robust after controlling for other ESG measures such as energy intensity. Last, I propose and verify some potential mechanisms that explain banks' response to firm ESG ownership structure.

Throughout this paper, I use the share fraction of US PRI (Principles for Responsible Investment) mutual funds as the measurement of firm-level ESG ownership. The PRI is the world's leading proponent of responsible investment and the number of signatories has grown from 63 in 2005 to over 3,800 by the end of 2021. US financial institutions constitute the largest group of PRI signatories. To calculate the firm-level PRI share ownership, I hand-match PRI signatories with 701 Morningstar US mutual funds and compute the fraction of outstanding shares that are owned by PRI signatories in each quarter. In the first part of the analysis, I gather the annual firm ESG scores from Thomson Reuters Asset4 database to examine if PRI ownership has a positive effect on firm ESG profile in the following year. In the second part of the analysis, I use the detailed loan-level information from LPC Dealscan database. This syndicated loan database contains comprehensive loan characteristics which allows me to study banks' response to PRI ownership using different outcomes, as well as delve deeper into the mechanisms behind the findings. Specifically, I study the effect on bank loan amount for each syndicated loan facility using the 53,484 loan-level sample and construct the loan facility-level sample that contains 12,559 facilities for the analysis on facility outcomes such as loan spreads and maturity.

I first show that PRI ownership does not affect firm ESG performance in the following year using five different ESG measures: ESG score, environmental pillar score, social pillar score, governance pillar score, and controversy score. The estimated coefficients on PRI ownership are not statistically significant for all five outcomes and the sign is even negative for the effect on the environmental pillar score. Taking the results on five outcomes together, PRI institutions in general do not seem to actively improve the ESG practices of the firms they invest in, thus having no positive impact on firm ESG performance. These results provide additional evidence for funds "greenwashing" themselves without caring enough for improving ESG (Kim and Yoon, 2020; Brandon, Glossner, Krueger, Matos, and Steffen, 2021). Given this finding, I further investigate if banks are aware of the real impacts of PRI ownership structure on firms and pay attention to this factor when issuing new loans. I find that banks on average significantly increase lending to firms with higher PRI ownership. An increase of one standard deviation of PRI ownership (i.e. 0.031) increases the lending of new loans by around 2%. To account for the endogeneity in bank-firm network formation, I also include bank-firm fixed effects which capture any persistent time-invariant differences in bank-firm relationships (Santos, 2011; Chakraborty, Goldstein, and MacKinlay, 2018). The coefficient is less economically significant but still statistically significant with the inclusion of bank-firm fixed effects.

One concern about the above results is whether PRI ownership is a proxy for other firm characteristics, such as credit risk or stock return. Although the specification for the main results include many firm-level financial variables such as profitability and leverage, it is possible that some components of firm credit risk or stock return are not captured by these covariates thus omitted from the regression. To alleviate this concern, I conduct robustness tests by adding controls such as S&P issuer credit ratings. The positive coefficients on PRI ownership are robust even controlling for firm credit rating and stock return.

The analysis on loan amount does not indicate whether the results come from the supply side or the demand side. It is both possible that banks are willing to lend more to firms with higher PRI ownership or firms with higher PRI ownership demand more loans. Thus, to understand the supply side reactions better, I conduct facility-level analysis on loan spreads to check if banks charge lower interest rate to firms with higher PRI ownership. Both within industry and within firm comparisons suggest that banks charge a significantly lower interest to borrowers with higher PRI ownership, even after controlling for firm credit risk and stock return. An increase of one standard deviation of PRI ownership decreases the pricing of new loans by around 2%. To check if the effect is driven by firms' own ESG performance instead of ESG ownership structure, I calculate the annual industry-level energy intensity[1] from U.S. Census Bureau's Annual Survey of Manufacturers (ASM) as a measure of energy and carbon efficiency. High energy intensity is strongly associated with high loan spreads, which is consistent with the findings in

[1] Defined as "dividing the cost of purchased electricity and fuel costs of the sector by the value of the shipments of the sector".

other papers which study the higher cost of capital for less green firms (Chava, 2014; Ge and Liu, 2015; Zerbib, 2019).

Next, I examine two mechanisms that could possibly explain banks' response to PRI ownership. First, I check if banks that demonstrate more ESG awareness pay more attention to PRI ownership compared to other banks. Banks that adhere to the Equator Principles (EPs), which is the benchmark for financial institutions in determining, assessing, and managing environmental and social risks, reduce loan spreads more when firms' PRI ownership increases. Compared with non-EP banks, EP banks charge 2.75% lower loan spread for one standard deviation increase in PRI ownership. In addition, the coefficient for PRI ownership is not statistically significant, indicating that the effect on loan spread is largely driven by loans issued by EP banks. Second, banks may believe that most PRI investors should have long investment horizon and sell less after firm's poor stock performance or negative shocks (Starks, Venkat, and Zhu, 2017). If banks take investor horizon and short-term volatility into consideration when setting loan spreads, then they should charge lower spread to firms with higher PRI ownership. I find that one standard deviation increase in PRI ownership would lead to around half month increase in loan maturity, controlling for loan, firm, and bank characteristics. This provides suggestive evidence that banks may care about the potential benefits of long horizon investors, thus charging lower loan spread to firms with higher PRI ownership.

This paper is related to the studies that examine how banks and investors react to firms' environmental, social, and governance factors. El Ghoul, Guedhami, Kwok, and Mishra (2011) finds that firms with better corporate social responsibility scores have lower costs of equity financing while firms in the "sin" industries exhibit higher cost of equity. Bauer and Hann (2010) and Chava (2014) show that fewer corporate environmental concerns and better ESG performance are associated with lower cost of capital. Ivanov, Kruttli, and Watugala (2021) find that banks mitigate their exposure to climate transition risks by shortening loan maturity and increasing interest rates to firms affected by the California cap-and-trade policy. Investors also care about firms' ESG profile and are willing to forego some pecuniary benefits or be more patient for ESG considerations (Renneboog, Ter Horst, and Zhang, 2008; Benson and Humphrey, 2008; Renneboog, Ter Horst, and Zhang, 2011; Barber, Morse, and Yasuda, 2021). This paper complements these studies by showing that banks, especially those that show more environmental and social awareness, pay attention to firms' ESG ownership structure when issuing loans. This suggests that banks actually factor in more soft information about firm ESG and other investors' ESG preference when evaluating borrowers.

This paper also contributes to the ongoing discussion on "greenwashing" by studying the effect of PRI ownership on firm ESG performance. On the one hand, previous studies mainly focus on fund portfolio analysis and find that while US PRI signatory mutual funds attract more fund inflow and widely advertise their signatory status, they do not show improvement in fund-level ESG score (Kim and Yoon, 2020; Brandon, Glossner, Krueger, Matos, and Steffen, 2021). On the other hand, institutional ownership positively affect firm environmental and social practices as they actively promote good proposals and practices as shareholders (Aggarwal, Erel, Ferreira, and Matos, 2011; Dyck, Lins, Roth, and Wagner, 2019; Chen, Dong, and Lin, 2020). To examine the real impacts of PRI ownership on firms, I conduct firm-level analysis on the relationship between PRI ownership and firm ESG performance. I find no evidence of improvement in corporate ESG performance across five different ESG measures. These results are in consistency with the "greenwashing" behaviors of PRI funds.

This paper is organized as follows. Section 2 introduces the background on PRI. Section 3 describes data and empirical specifications. Section 4 presents regression results. Section 5 addresses some concerns using robustness checks and Section 6 discusses two possible mechanisms. Finally, Section 7 concludes.

1.2 Background: PRI

The PRI is the world's leading proponent of responsible investment. It's goal is to help its signatories understand the implications of ESG investing and incorporate ESG investing into their investment strategies. Since the launch of PRI in 2006, the total reported AUM has increased from 6.5 trillion to 121.3 trillion dollars in 2021. Meanwhile, the number of signatories has grown from 63 to over 3,800 by the end of 2021. The absolute majority of PRI signatories are asset managers, followed by asset owners and service providers. All signatories are required to pay an annual membership fee to PRI, which serves as the primary funding source for PRI.

The PRI adhere to the six principles for responsible investment that serve as importance guidelines for its signatories. The principles are: (1) incorporate ESG issues into investment analysis and decision-making processes, (2) be active owners, (3) seek appropriate disclosure on ESG issues, (4) promote acceptance and implementation of the Principles, (5) work together to enhance the effectiveness in implementing the Principles, and (6) report on the activities and progress towards implementing the Principles. To ensure that signatories practice responsible investing and continue to improve over time, the PRI requires signatories to report on their responsible investment activities annually. The PRI then reviews the submitted reports and publishes a public report on the PRI website for each signatory that includes all public and mandatory indicators. In 2018, the PRI implemented minimum requirements for PRI membership for investors. The three minimum requirements proposed in 2018 include ESG investment AUM, senior-level oversight of responsible investing, and internal/external staff implementing responsible investing. The PRI plans to implement more rigorous minimum requirements in the following years. Any investment manager and asset owner signatories that fail to meet the minimum requirements over a two-year period are likely to be delisted.

1.3 Empirical design

1.3.1 Data

I obtain the quarterly mutual fund holdings and fund characteristics data from Morningstar. Morningstar historical monthly holdings data contains the weighting and number of shares of every stock owned by U.S. mutual funds. I limit the fund sample to only actively managed U.S. equity funds by mostly following the steps proposed by Pástor, Stambaugh, and Taylor (2015). Specifically, I remove bond funds, international funds, sector funds, target funds, real estate funds, other non-equity funds, index funds, and funds of funds by examining keywords in the Morningstar Category variable or directly using the indicator variables in the fund characteristics data. To further filter actively managed funds, I exclude single share class funds with expense ratio below 0.1 since it is very unlikely for actively managed mutual funds to charge extremely low investment fees so that the expense ratio is below 0.1 (Pástor, Stambaugh, and Taylor, 2015). For funds with multiple share classes, I exclude those funds with lag-TNA-weighted expense ratio below 0.1. Furthermore, I keep those funds with a Morningstar Category in the 3-by-3 size/value grid (US Large Growth, US Large Value, US Large Blend, US Mid-Cap Growth, US Mid-Cap Value, US Mid-Cap Blend, US Small Growth, US Small Value, US Small Blend) following Pool, Stoffman, and Yonker (2012). After the fund filtering process, there are 3,544 unique funds remaining in the sample.

I obtain firm-level ESG ratings data from Thomson Reuters Eikon database (previously known as ASSET4). Thomson Reuters offers a comprehensive ESG database with data points starting in 2002. The scores incorporates information on 10 categories[2] of the three pillars (environmental, social, and governance). Thomson Reuters first collects over

[2]Environmental: resource use, emissions, innovation; Social: workforce, human rights, community, product responsibility; Governance: management, shareholders, CSR strategy.

400 ESG measures from publicly available sources, then standardizes the information to generate more than 70 key performance indicators to ensure comparability across firms and calculates category scores using percentile rank score method[3], lastly generates the overall ESG score as the weighted average of the category scores. To determine the weight of each category in the last step, Thomson Reuters assigns higher weight to categories that contain more indicators thus more information on firm ESG performance.

I use LPC's Dealscan database to collect loan-specific information. Dealscan provides detailed information on comprehensive loan characteristics of both sole-lender and syndicated loans, such as lender, borrower, date, amount, maturity, spreads, loan purpose, etc. I merge borrowers in the loan-level data with Compustat for firm fundamentals using the link table provided by Chava and Roberts (2008). Similarly, I link banks in the loan-level data with Call Report data using the link table provided by Chakraborty, Goldstein, and MacKinlay (2018). Call Reports are used to gather information on bank holding company (BHC) characteristics as they include quartet-level financial statistics for BHCs.

I gather the list of the PRI signatories together with their join dates and signatories type from the PRI official website [4] I manually map the signatories to the Morningstar holdings data set by name matching. For each firm and year, I calculate the sum of shares held by the PRI signatories and divide that by total shares outstanding of the firm to obtain the value for *PRI ownership*. This firm-year level data set is then merged with the Dealscan loans by borrower gvkey.

1.3.2 Methodology

I first investigate if ESG ownership structure affects a firm's ESG profile. Specifically, I check if there is any effect of a firm's PRI ownership on its ESG performance in the next

[3] $score = \frac{n.of\ companies\ with\ a\ worse\ value + \frac{n.of\ companies\ with\ the\ same\ value\ included\ the\ current\ one}{2}}{n.of\ companies\ with\ a\ value}$

[4] https://www.unpri.org/signatories/signatory-resources/signatory-directory.

year using Equation 3.1.

$$ESG\ measure_{i,t} = \beta_0 + \beta_1 PRI\ ownership_{i,t-1} + \alpha X_{i,t-1} + \mu_i + \gamma_t + \varepsilon_{i,t}, \tag{1.1}$$

where i indexes firm and t indexes year. *PRI ownership* is defined as the proportion of firm *i*'s shares that is owned by PRI US mutual funds in the previous year. I control for firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio, return on equity) in the previous year as well as firm fixed effects and year fixed effects to control for firm time-invariant characteristics and eliminate bias from unobservables that change over time but are constant across firms. Given the definition and computation method for the Thomson Reuters Eikon ESG measures, a positive β_1 indicates that PRI ownership does have a positive impact on firm's ESG performance in the following year.

To estimate the effect of firm PRI ownership on bank loan amount, I regress the logarithm of the loan amount for each lender on the main explanatory variable, PRI ownership, and various covariates and fixed effects. The basic regression I estimate is

$$\begin{aligned} log(bank\ amount)_{ijlt} = \beta_0 + \beta_1 PRI\ ownership_{jt-1} + Loan\ vars_l + Bank\ vars_{it-1} \\ + Firm\ vars_{jt-1} + [fixed\ effects] + \varepsilon_{ijlt}, \end{aligned} \tag{1.2}$$

where i indexes bank, j indexes firm, l indexes loan, and t indexes year-quarter (i.e. 201001 stands for the 1^{st} quarter in 2010). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. *PRI ownership* is defined as the proportion of firm *j*'s shares that is owned by PRI US mutual funds in the previous quarter. I control for loan characteristics (allindrawn, maturity, facility amount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter as these characteristics may affect bank lending decisions. I also add various sets of fixed effects to check the robustness of the result.

If banks take borrowers' ESG ownership structure into consideration when making loan decisions, I expect the estimate for β_1 to be positive and statistically significant since banks are willing to lend more to firms with higher PRI ownership. One caveat of this analysis is that I am unable to disentangle the supply-side effect from banks and the demand-side effect from firms. It is possible that firms with higher PRI ownership have higher demand for loans, for example due to longer term investment plans, thus rendering a positive estimate for β_1. To better understand banks' response to firms' ESG ownership structure, I conduct further facility-level analysis on loan spreads following the regression specification in Equation 3.3.

$$\begin{aligned} log(loan\ spread)_{ijft} &= \beta_0 + \beta_1 PRI\ ownership_{jt-1} + Loan\ vars_f + Bank\ vars_{it-1} \\ &+ Firm\ vars_{jt-1} + [fixed\ effects] + \varepsilon_{ijft}, \end{aligned} \tag{1.3}$$

where i indexes the sole lender or lead lenders in a syndicated loan, j indexes firm, f indexes facility, and t indexes year-quarter. There are two main differences between Equation 3.2 and 3.3. First, since loan spread is unique for each loan facility while bank loan amount is unique for each lender in a specific facility, Equation 3.2 is at the loan-level while Equation 3.3 is at the facility-level. Second, Equation 3.3 controls for characteristics of the sole lender or lead lenders of each facility while Equation 3.2 controls for each lender's characteristics. Lead lenders usually decide facility contract terms in syndicated loans (Ivashina and Scharfstein, 2010) so only the covariates for lead lenders are included for each facility in Equation 3.3. Following the rationale for Equation 3.2, if banks factor in borrowers' ESG ownership structure, then they are likely to charge a lower spread for firms with higher PRI ownership, thus β_1 in Equation 3.3 is expected to be negative.

1.3.3 Sample construction

This paper covers the years from 1986 to 2017 due to the availability of the Morningstar mutual fund holdings data set. I compute the PRI ownership for each firm in the MS database as the fraction of outstanding shares that are owned by actively managed US mutual funds. Due to different data frequency, I calculate the annual PRI ownership for the analysis on ESG measures and quarterly PRI ownership for the analysis on syndicated loans. Table 1.1 shows the summary statistics for the annual PRI ownership and various ESG measures. There are in total 11,306 firm-year observations in the annual analysis for Equation 3.1. The *PRI ownership* has a mean of 0.036 and standard deviation of 0.046. All the ESG measures except for controversy are generally widely distributed in the range from 0 to 100. Since there are limited firm negative ESG news exposed by media every year, most of the observations in the sample have a full score of 100 points for the controversy score.

Table 1.1: Summary statistics on annual data

This table reports the summary statistics on the main variables included in the annual analysis. The sample is from 1986 to 2017. *PRI ownership* is computed in the last quarter of each year and the five ESG measures are at an annual frequency.

variable	N	mean	min	p5	p25	p50	p75	p95	max	sd
PRI ownership	11,306	0.036	0.000	0.000	0.000	0.017	0.059	0.129	0.628	0.046
ESG score	11,306	39.316	0.448	13.313	24.708	35.781	51.739	75.330	95.195	19.000
Environmental pillar	11,306	25.605	0.000	0.000	0.000	17.596	45.445	79.414	98.546	27.274
Social pillar	11,306	41.300	0.599	11.986	25.475	37.921	55.264	80.482	98.119	20.738
Governance pillar	11,306	48.635	0.187	12.148	30.802	49.234	66.404	83.688	98.738	22.289
Controversy	11,306	87.898	0.617	18.571	100.000	100.000	100.000	100.000	100.000	25.785

Focusing on the analysis on lender responses to firms' ESG ownership structure in Equation 3.2, I match Dealscan loans to firm quarterly PRI ownership, bank holding company, and firm characteristics. There are 53,484 observations with non-missing covariates for 12,559 term loans and revolvers newly issued by 433 banks to 1,993 publicly firms. Term loans and revolvers are the most common types of corporate loans and contained more detailed loan information so I follow Campello and Gao (2017) to include only these two loan types for the analysis of bank new lending. Since the maturity of loans in the sample are syndicated loans, each observation is uniquely defined by five attributes: borrower, lender, facility, start year, and start quarter. Table 1.2 Panel A shows the summary statistics for the matched loans in the quarterly sample. *log*(*bankamount*) is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. *Maturity*, and *all in drawn* denote loan maturity (in months), and loan spreads (above LIBOR) respectively. Compared with the annual sample, firms in the quarterly sample tend to have lower PRI ownership which can be largely explained by the exclusion of largely small firms in the Dealscan loan data set. Table 1.2 Panel B reports the summary statistics at the facility-level. Again, the number of observations shrinks since there are loans made by multiple lenders in one syndicated loan facility. I keep only the sole lenders or the lead lenders in the facility sample so the reported bank characteristics in Panel B are for those lenders only. The summary statistics for firm characteristics, omitted in Panel B, are the same as in those in Panel A. There are in total 12,559 loan facilities issued by 284 sole or lead lenders in the facility-level sample and the distribution of loan and bank characteristics are similar to those in the loan-level data.

Table 1.2: Summary statistics on quarterly data

This table reports the summary statistics on the main variables included in the quarterly analysis. The sample is from 1986 to 2017. Panel A reports statistics on the loan-level sample for the loan amount analysis and Panel B reports statistics on the facility-level sample for the loan spread analysis. *Bank loan amount* is at the loan-level and *loanmaturity* and *allindrawn* are at the facility-level. Panel A also reports the firm and bank covariates.

variable	**N**	**mean**	**min**	**p5**	**p25**	**p50**	**p75**	**p95**	**max**	**sd**
Panel A: Loan-level Sample										
PRI ownership	8,873	0.013	0.000	0.000	0.000	0.000	0.005	0.082	0.386	0.031
Log(bank loan amount)	53,484	3.349	-11.976	1.494	2.696	3.374	4.061	5.181	8.700	1.161
Maturity	53,484	57.181	1.000	27.000	49.000	60.000	60.000	84.000	276.000	16.628
All in drawn	53,484	160.212	5.000	25.000	75.000	150.000	225.000	350.000	1,275.000	104.858
Firm size	8,873	7.570	0.306	5.152	6.467	7.472	8.585	10.358	14.588	1.579
Firm profitability	8,873	0.038	-1.543	0.006	0.025	0.035	0.049	0.081	0.422	0.033
Firm tangibility	8,873	0.335	0.000	0.040	0.134	0.271	0.498	0.830	0.979	0.243
Firm leverage	8,873	0.330	0.000	0.031	0.188	0.302	0.436	0.699	2.127	0.211
Firm market-to-book	8,873	1.806	0.437	0.938	1.204	1.511	2.026	3.531	195.771	2.280
Bank loan to asset	5,177	0.582	0.046	0.275	0.498	0.633	0.694	0.768	0.868	0.163
Bank equity to asset	5,177	0.084	0.017	0.053	0.070	0.081	0.096	0.122	0.690	0.026
Bank income to asset	5,177	0.007	-0.055	0.001	0.003	0.006	0.010	0.014	0.060	0.005
Panel B: Facility-level Sample										
Log(bank loan amount)	12,559	3.763	-11.976	1.792	3.025	3.795	4.518	5.652	8.700	1.194
Maturity	12,559	56.328	1.000	24.000	48.000	60.000	60.000	84.000	276.000	18.737
All in drawn	12,559	188.650	5.000	30.000	100.000	175.000	250.000	400.000	1,275.000	121.787
Bank loan to asset	2,176	0.564	0.095	0.296	0.434	0.612	0.685	0.765	0.868	0.160
Bank equity to asset	2,176	0.083	0.030	0.051	0.069	0.081	0.094	0.119	0.305	0.021
Bank income to asset	2,176	0.006	-0.021	0.001	0.003	0.006	0.009	0.014	0.034	0.005

1.4 Results

1.4.1 Does PRI ownership affect firm ESG?

I start the analysis by investigating if higher PRI ownership has real impacts on firms' ESG profile in the next year. On the one hand, PRI investors are likely to put more weights on ESG investing and have the potential to actively influence corporate decisions, which is implied in the second PRI principle (active owners). On the other hand, studies show convincing evidence that PRI investors do not improve fund-level ESG score after joining PRI (Kim and Yoon, 2020; Brandon, Glossner, Krueger, Matos, and Steffen, 2021). Before delving into the main results of the paper on banks' responses to PRI ownership, it is important to understand the real impact of PRI ownership on firm ESG profile. If PRI ownership does not have significant effects on firm ESG, then banks can either be "victims" of the "greenwashing" by PRI institutions or link PRI ownership with some benefits other than ESG performance. I will discuss more about this in the discussion section.

Table 1.3 presents the effect of lagged one year PRI ownership on various firm ESG measures. The ESG measures are obtained from the Thomson Reuters Asset4 database. ESG score reflects the overall ESG performance of a firm, which incorporates 10 categories in the three pillars: environmental, social, and governance. Column (2)-(4) show the separate score for each ESG category, with each of the score calculated as the weighted average of the subcategories. Column (5) reports the ESG controversy score, which captures negative ESG-related media reporting events on 23 controversy topics such as tax fraud controversies and emission controversies. It is clear from table 1.3 that PRI ownership does not significantly affect a firm's ESG performance in the following year. None of the coefficients on *PRI ownership* is statistically significant and the coefficient is even negative for environmental pillar score. Hence, the results in table 1.3 suggest that there is no positive effect of PRI ownership on firm ESG performance, even if PRI institutions are committed to active ESG investment strategies.

Table 1.3: Effect of PRI ownership on firm ESG

This table reports the effect of lagged one year firm PRI ownership on firm ESG performance. The dependent variables are overall ESG score, environmental pillar score, social pillar score, governance pillar score, and controversy score. *PRI ownership* is defined as the proportion of a firm shares that is owned by PRI US mutual funds in the previous year. I also control for firm size, profitability, tangibility, leverage, and market-to-book ratio in the previous year. The values in parentheses are the standard errors. The standard errors are clustered by firm. Firm and year fixed effects are used. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1) ESG score	(2) Environmental pillar	(3) Social pillar	(4) Governance pillar	(5) Controversy
PRI ownership	7.700	-14.057	5.158	17.251	16.569
	(8.385)	(12.905)	(8.703)	(10.588)	(10.971)
Firm size	2.141***	2.990**	2.944***	0.227	-2.969**
	(0.732)	(1.171)	(0.850)	(0.861)	(1.368)
Firm profitability	12.419*	20.223*	14.064*	10.845	23.117
	(7.062)	(11.354)	(8.463)	(9.934)	(16.365)
Firm tangibility	-2.496	-3.976	-5.178	-2.795	-0.227
	(4.905)	(7.424)	(5.517)	(5.835)	(7.724)
Firm leverage	4.079**	2.880	4.766**	5.748**	0.920
	(2.075)	(3.060)	(2.359)	(2.538)	(3.701)
Firm markettobook	0.607***	0.343	0.808***	0.282	0.542
	(0.222)	(0.267)	(0.268)	(0.243)	(0.381)
Firm ROE	0.334	-0.230	0.637	1.158	-0.870
	(1.767)	(2.440)	(1.982)	(1.334)	(3.876)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	7,826	7,826	7,826	7,826	7,826
R-Squared	0.834	0.832	0.815	0.647	0.488

1.4.2 Loan amount

I first show the effects of PRI ownership on bank loan amounts. Table 1.4 reports results for Equation (1). The dependent variable is the logarithm of bank loan amounts. The the main variable of interest is $PRI\ ownership$, which is the lagged fraction of PRI shares of the borrower. $Maturity$, $Facility\ amount$ and $All\ in\ drawn$ denote loan maturity (in months), loan amount (in millions), and loan spreads (above LIBOR) respectively. Lagged firm $size$, $profitability$, $tangibility$, $leverage$, and $market\text{-}to\text{-}book\ ratio$ are included as firm characteristics. Bank financial variables such as $loan\text{-}to\text{-}asset$, $equity\text{-}to\text{-}asset$, and $income\text{-}to\text{-}asset$ serve as controls for bank characteristics.

Table 1.4 reports the results from models with different levels of fixed effects. The estimated coefficients for *PRI ownership* are positive, statistically significant across all specifications. On average, banks significantly increase lending to firms with higher PRI ownership. In Column (1), an increase of one standard deviation of PRI ownership (i.e. 0.031) increases the lending of new loans by 2.64% (calculated as (exp(0.839*0.031) – 1) * 100). Bank and firm fixed effects are used to control for any time-invariant differences among banks and firms, respectively. Year and quarter fixed effects control for differences between time periods. In Column (3), I use bank-year-quarter and firm fixed effects to control for any confounding shocks at the bank level and time-invariant differences among firms. This specification exploits the variation in firm *PRI ownership* among firms borrowing from the same bank in the same quarter. A bank in general lends more to firms with higher PRI ownership relative to other borrowers at the same time. To mitigate the concern that the formation of the bank-firm network is endogenous, I follow Chakraborty, Goldstein, and MacKinlay (2018) and Santos (2011) to add bank-firm fixed effects in Column (4) and (5). Bank-firm fixed effects capture any persistent time-invariant differences in bank-firm relationships. Within the same bank-firm pair, the positive relationship between PRI ownership and loan amount still exists: the bank lends more to the firm when the firm has higher PRI ownership.

One caveat of the analysis on loan amount is that it does not show if banks are indeed more willing to lend to firms with higher PRI ownership (supply side). A similar set of results in table 1.4 can be obtained solely if firms with higher PRI ownership demand more loans than other firms. Thus, to answer the question about whether banks pay attention to borrower PRI ownership more clearly, I conduct further analyses on loan spreads to investigate if firms with higher PRI ownership are able to borrow at a lower cost.

Table 1.4: Effect of PRI ownership on new loan amount

This table reports the effect of lagged one quarter firm PRI ownership on new bank loan amount. The dependent variable is the logarithm of new bank loan amounts for each lender in a new loan. *PRI ownership* is defined as the proportion of a firm shares that is owned by PRI US mutual funds in the previous quarter. I also control for loan-, firm-, and bank-level characteristics in the previous quarter. The values in parentheses are the standard errors. The standard errors are clustered by bank. Bank, time, firm, bank interacted with time, and bank interacted with firm fixed effects are used. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: log(bank loan amount)									
PRI ownership	0.839***	0.785***	0.506*	0.865***	0.810***	0.544**	0.838***	0.779***	0.502*
	(0.177)	(0.181)	(0.274)	(0.178)	(0.182)	(0.274)	(0.179)	(0.183)	(0.274)
Facility amount	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Maturity	0.004***	0.005***	0.006***	0.004***	0.005***	0.006***	0.004***	0.005***	0.006***
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
All in drawn	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Firm size	0.095***	0.099***	0.116***	0.093***	0.097***	0.113***	0.094***	0.097***	0.114***
	(0.014)	(0.015)	(0.018)	(0.014)	(0.015)	(0.018)	(0.014)	(0.015)	(0.020)
Firm profitability	-0.026	0.099	0.230	0.047	0.167	0.272	-0.029	0.087	0.228
	(0.183)	(0.185)	(0.209)	(0.187)	(0.187)	(0.208)	(0.183)	(0.185)	(0.210)
Firm tangibility	0.072	0.115	0.252***	0.061	0.105	0.245***	0.071	0.110	0.247***
	(0.070)	(0.074)	(0.077)	(0.070)	(0.073)	(0.077)	(0.071)	(0.074)	(0.076)
Firm leverage	-0.068*	-0.046	-0.048	-0.066*	-0.043	-0.044	-0.066*	-0.039	-0.041
	(0.037)	(0.042)	(0.054)	(0.037)	(0.042)	(0.055)	(0.037)	(0.042)	(0.058)
Firm markettobook	0.010*	0.010*	0.022**	0.011**	0.011**	0.026***	0.010*	0.009*	0.021**
	(0.005)	(0.005)	(0.009)	(0.005)	(0.005)	(0.010)	(0.005)	(0.005)	(0.009)
Bank loan-to-asset	0.223*		0.155	0.223*		0.156	0.223*		0.154
	(0.133)		(0.182)	(0.133)		(0.181)	(0.133)		(0.182)
Bank equity-to-asset	-1.291**		-1.121	-1.272*		-1.106	-1.290**		-1.118
	(0.653)		(0.950)	(0.654)		(0.949)	(0.653)		(0.950)
Bank income-to-asset	-3.207		-3.032	-3.237		-3.122	-3.207		-3.031
	(2.568)		(3.027)	(2.565)		(3.005)	(2.569)		(3.029)
Stock return				-0.199***	-0.188***	-0.249***			
				(0.039)	(0.041)	(0.045)			
Credit rating							0.001	0.003	0.003
							(0.004)	(0.004)	(0.006)
Bank FE	Yes	No	No	Yes	No	No	Yes	No	No
Year FE	No	No	No	No	No	No	No	No	No
Quarter FE	No	No	No	No	No	No	No	No	No
Year × Quarter FE	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Firm FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Bank × Year × Quarter FE	No	Yes	No	No	Yes	No	No	Yes	No
Bank × Firm FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	53346	51455	44120	53346	51455	44120	53346	51455	44120
R-Squared	0.575	0.627	0.730	0.576	0.627	0.731	0.575	0.627	0.730

1.4.3 Loan spread

I relate firms' PRI ownership to bank loan spreads to see if lenders care about this firm-specific ownership feature. The dependent variable in table 1.5 is the log of the loan spread. Since loan spread is mainly determined by the lead lender in a syndicated loan (Ivashina and Scharfstein, 2010), I keep the observations for only the lead lenders in each loan in this regression sample. All specifications in table 1.5 include year-by-quarter fixed effects. Model (1), (3) and (5) include industry fixed effects following Chava (2014) and model (2) is the baseline result which exploits within-firm variation in PRI ownership across time by adding firm fixed effects.

The results in all models suggests that banks charge a significantly lower loan spread to borrowers with higher PRI ownership. An increase of one standard deviation of PRI ownership (i.e. 0.032) decreases the pricing of new loans by 1.95% (calculated as (exp(0.604*0.032) – 1) * 100). One concern about Model (1) and (2) is that both loan spread and PRI ownership can be affected by firm ESG performance. To ease this concern, I calculate industry-level energy intensity[5] from U.S. Census Bureau's Annual Survey of Manufacturers (ASM) as a measure of energy and carbon efficiency. High energy intensity implies using more energy to produce a product or provide a service. Model (3) present the results with the added control of energy intensity. The estimated effect of PRI ownership on loan spread is still significantly negative, although the magnitude of the coefficient drops with the added control of energy intensity. Interestingly, there seems to be a large and significantly positive relationship between energy intensity and loan spreads, which is consistent with the existing findings that less environmental-friendly firms face higher cost of capital (Chava, 2014; Ge and Liu, 2015; Zerbib, 2019). Another interesting note is how loan pricing is related to various firm characteristics: banks charge lower interest rate to firms with higher market capitalization, more profits, more tangible assets, higher market-to-book ratio, and lower leverage.

[5] Defined as "dividing the cost of purchased electricity and fuel costs of the sector by the value of the shipments of the sector".

Table 1.5: Effect of PRI ownership on new loan spread

This table reports the effect of lagged one quarter firm PRI ownership on new loan spread. The dependent variable is the logarithm of bank spread for each new facility. *PRI ownership* is defined as the proportion of a firm shares that is owned by PRI US mutual funds in the previous quarter. *Energy intensity* is the industry-level measurement of energy and carbon efficiency. High energy intensity represents using more energy to produce a product or provide a service. I also control for facility-, firm-, and bank-level characteristics in the previous quarter. The values in parentheses are the standard errors. Firm, time, and industry effects are used. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: log(loan spread)						
PRI ownership	-0.830***	-0.604***	-0.565*	-0.843***	-0.618***	-0.647*
	(0.183)	(0.202)	(0.341)	(0.184)	(0.202)	(0.353)
Facility amount	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Maturity	0.003***	0.003***	0.005***	0.003***	0.003***	0.005***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Bank loan-to-asset	0.223**	0.192*	0.644	0.226**	0.188*	0.682
	(0.093)	(0.099)	(0.443)	(0.093)	(0.099)	(0.450)
Bank equity-to-asset	0.752	0.724	-1.275	0.753	0.750	-1.450
	(0.579)	(0.604)	(1.880)	(0.578)	(0.603)	(1.924)
Bank income-to-asset	-0.053	2.987	9.942	-0.121	3.130	10.282
	(4.333)	(4.542)	(11.808)	(4.331)	(4.536)	(12.210)
Firm size	-0.155***	-0.158***	-0.174***	-0.155***	-0.156***	-0.183***
	(0.006)	(0.010)	(0.015)	(0.006)	(0.010)	(0.015)
Firm profitability	-3.613***	-2.261***	-2.577***	-3.672***	-2.318***	-2.669***
	(0.222)	(0.212)	(0.783)	(0.222)	(0.213)	(0.809)
Firm tangibility	-0.239***	-0.218***	-0.318***	-0.233***	-0.213***	-0.335***
	(0.038)	(0.063)	(0.116)	(0.038)	(0.063)	(0.119)
Firm leverage	0.669***	0.513***	0.661***	0.669***	0.511***	0.675***
	(0.025)	(0.035)	(0.059)	(0.025)	(0.035)	(0.061)
Firm markettobook	-0.037***	-0.024***	-0.129***	-0.038***	-0.025***	-0.135***
	(0.003)	(0.003)	(0.018)	(0.003)	(0.004)	(0.019)
Energy intensity			2.929***			3.156***
			(0.635)			(0.654)
Stock return				0.191***	0.146***	0.082
				(0.041)	(0.039)	(0.118)
Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	Yes	No	Yes
Firm FE	No	Yes	No	No	Yes	No
Observations	12522	12275	2162	12522	12275	2162
R-Squared	0.554	0.714	0.676	0.555	0.714	0.678

1.5 Robustness

Although the results so far suggest that banks tend to lend more and charge lower interest rate to firms with higher PRI ownership, one concern remains: whether PRI ownership is a proxy for firm credit risk? In this case, banks may actually price a firm's credit risk instead of its PRI ownership. Although the loan spread analysis in table 1.5 include many firm-level financial variable such as profitability and leverage, which are related to credit risk to some extent, it is possible that some components of firm credit risk are not captured by these covariates thus omitted from the regression. To alleviate this concern, I obtain the data set, S&P issuer credit ratings, from Compustat and include the additional control for firm credit risks in the previous models. The S&P issuer credit ratings reflect the firm's overall capacity and willingness to meet its financial obligations (Hovakimian, Kayhan, and Titman, 2012). S&P credit ratings are based on information on firm's capital structure, management quality, competitiveness, and even private meetings with firm's management team about financial difficulties and strategies. The ratings also focus on forward-looking and foreseeable credit risks (S&P Manual 2006, p.33), which aligns with lenders' interest when they evaluate borrowers and decide loan terms. In short, S&P issuer credit ratings integrate information from various sources including both public and private, historical and forward-looking information. I transform the letter ratings into ordinal numerical ratings, with "AAA" having a score of 21 and "C" having a score of 1. As the observations with ratings below "B-" are very few in the sample, I exclude these observations following Hovakimian, Kayhan, and Titman (2012).

Table 1.6 presents the results on loan pricing with the additional data on firm credit risks. Model (1)-(4) re-estimate the Equation 3.3 controlling for firm credit ratings. It is clear that the addition of credit rating does not change the effect of PRI ownership on loan pricing much, although the magnitude of the estimates slightly decreases compared to those in table 1.5. It is important to note that although the estimates on PRI ownership are much more negative than the estimates on credit rating, they do not suggest the effect of PRI ownership on loan pricing is much stronger since they have different interpretations. One standard deviation increase in PRI ownership is associated with approximately 1.6% decrease in loan spread while one standard deviation increase in credit rating (i.e. 3.1) is associated with 36% decrease in loan spread. These results show that PRI ownership is not likely to be a proxy for firm credit ratings.

Table 1.6: Effect of PRI ownership on new loan spread controlling for credit risk

This table reports the effect of lagged one quarter firm PRI ownership on new loan spread. The dependent variable is the logarithm of bank spread for each new facility. *PRI ownership* is defined as the proportion of a firm shares that is owned by PRI US mutual funds in the previous quarter. *Creditrating* is the ordinal numerical ratings obtained from S&P. I also control for facility-, firm-, and bank-level characteristics in the previous quarter. The values in parentheses are the standard errors. Firm, time, and industry effects are used. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)
Dependent Variable: log(loan spread)				
PRI ownership	-0.563***	-0.451**	-0.573***	-0.463**
	(0.167)	(0.197)	(0.168)	(0.197)
Credit rating	-0.105***	-0.096***	-0.104***	-0.096***
	(0.003)	(0.005)	(0.003)	(0.005)
Facility amount	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Maturity	0.003***	0.003***	0.003***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
Bank loan-to-asset	0.177**	0.232**	0.179**	0.228**
	(0.090)	(0.095)	(0.090)	(0.095)
Bank equity-to-asset	0.686	0.635	0.687	0.657
	(0.559)	(0.582)	(0.558)	(0.581)
Bank income-to-asset	2.778	0.600	2.717	0.729
	(4.143)	(4.337)	(4.141)	(4.332)
Firm size	-0.014**	-0.079***	-0.014**	-0.077***
	(0.006)	(0.010)	(0.006)	(0.010)
Firm profitability	-1.983***	-1.772***	-2.034***	-1.822***
	(0.180)	(0.198)	(0.180)	(0.199)
Firm tangibility	-0.170***	-0.097	-0.165***	-0.093
	(0.035)	(0.062)	(0.035)	(0.062)
Firm leverage	0.338***	0.296***	0.339***	0.295***
(0.025)	(0.035)	(0.025)	(0.035)	
Firm markettobook	-0.016***	-0.016***	-0.016***	-0.017***
	(0.002)	(0.003)	(0.002)	(0.003)
Stock return			0.151***	0.123***
			(0.039)	(0.039)
Year × Quarter FE	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No
Firm FE	No	Yes	No	Yes
Observations	12522	12275	12522	12275
R-Squared	0.618	0.730	0.618	0.730

1.6 Discussion: why lenders pay attention to firm PRI ownership?

The results in Table 1.5 suggest that firms with higher PRI ownership face lower loan spreads. One natural question is why banks price PRI ownership when lending new loans? The results presented in Table robustness should help to alleviate the concern that PRI ownership is a proxy for stock return or default risks. Besides these concerns, other possible explanations include banks' environmental awareness and preference for borrowers with longer horizon investors. In this section, I investigate these two potential explanations separately.

1.6.1 Environmental awareness

Financial institutions are demonstrating more and more environmental awareness across the globe in recent years. One example is the launch of the Equator Principles (EPs)[6] in June 2003 by the International Finance Corporation, which serve as the benchmark for financial institutions in determining, assessing, and managing environmental and social risks. By adhering to EPs, Equator Principles Financial Institutions (EPFIs) have the potential to minimize project-induced negative environmental and social impacts. There are currently 127 financial institutions in 38 countries that officially adopt the EPs as EPFIs. By the end of each year, they are required to report publicly on project transactions that have reached financial close or still on implementation processes. There are four types of transactions that are subject to public reporting: total number of project finance transactions, refinance and acquisition finance for project finance transactions, project-related corporate loans, and refinance and acquisition finance for project-related corporate loans.

I obtain the list of EPFIs from the EP website[7] and hand-match them to the lenders in the Dealscan syndicate loans data set. I am able to match 58 EPFI lenders in the loan data set. I hypothesize that EPFI lenders pay more attention to and place higher value on the environmental and social profiles of a firm's investors, thus charge even lower loan

[6] The ten EPs are: (1) Review & Categorization (2) E&S Assessment (3) Applicable E&S Standards (4) E&S Management System EP Action Plan (5) Stakeholder Engagement (6) Grievance Mechanism (7) Independent Review (8) Covenants (9) Independent Monitoring & Reporting (10) Reporting & Transparency.

[7] https://equator-principles.com/members-reporting/

spreads to firms with higher PRI ownership. This can be tested using Equation 1.4.

$$log(Loan\ spread)_{ijkt} = \beta_0 + \beta_1 PRI\ ownership_{it-1} \times Equator_j + +\beta_2 PRI\ ownership_{it-1} + \beta_3 Equator_j + firm\ controls_{i,t-1} + bank\ controls_{jt-1} + facility\ controls_k + [fixedeffects] + \varepsilon_{ijkt}, \tag{1.4}$$

Compared with Equation 3.3, Equation 1.4 includes an additional interaction term between firm's PRI ownership and lender's EPFI status. Based on the hypothesis, the coefficient for the interaction term is expected to be negative.

Table 1.7 presents the heterogeneous responses to PRI ownership from EP and non-EP banks. Since this is a facility-level analysis, I calculate the mean of lead lenders' characteristic variables separately for each facility and use them as the facility-level bank controls, since the lead lenders often have more decisive power on loan pricing (Ivashina and Scharfstein, 2010). *Equator* is a binary variable that denotes if the one of the facility's lead lenders officially adopts Equator Principles. Model 1 is the baseline model that includes the same set of facility-, firm-, and bank-level controls in Equation 3.1. To make sure that the result is not driven by the possibility that *PRI ownership* proxies for firm stock performance or credit risks, I gradually add these two variables in Model (3)-(5). All five models include year-by-quarter fixed effects while Model (1) makes the with-in industry comparison and Model (2)-(5) exploit the with-in firm variation in PRI ownership across time. The coefficient for the variable of interest, $PRI\ ownership \times Equator$, is negative and statistically significant throughout five models, which suggests that compared to other banks, banks that demonstrate more environmental awareness tend to lower loan spread more when firms' PRI ownership increases. Compared with non-EP banks, EP banks charge 2.75% lower loan spread for one standard deviation increase in *PRI ownership*. In addition, the coefficients for *PRI ownership* are not statistically significant, indicating that the effect on loan spread is largely driven by loans with EP lead lenders. Thus, these results are consistent with the hypothesis that banks that care more about environmental impacts pay more attention to firms' ownership ESG profile when determining loan spread.

Table 1.7: Effect of PRI ownership on new loan spread under Equator Principle

This table reports the effect of lagged one quarter firm PRI ownership on new loan spread. The dependent variable is the logarithm of bank spread for each new facility. *PRI ownership* is defined as the proportion of a firm shares that is owned by PRI US mutual funds in the previous quarter. *Equator* is a binary variable indicating the bank's Equator Principle status. *Creditrating* is the ordinal numerical ratings obtained from S&P. I also control for facility-, firm-, and bank-level characteristics in the previous quarter. The values in parentheses are the standard errors. Firm, time, and industry effects are used. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log(loan spread)					
PRI ownership	-0.154	0.286	0.281	0.312	0.308
	(0.348)	(0.434)	(0.434)	(0.432)	(0.433)
Equator	-0.031**	-0.016	-0.016	-0.011	-0.011
	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)
PRI ownership × Equator	-0.721**	-0.980**	-0.989**	-0.841**	-0.849**
	(0.336)	(0.410)	(0.411)	(0.408)	(0.409)
Facility amount	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Maturity	0.003***	0.003***	0.003***	0.003***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Bank loan-to-asset	0.202**	0.182*	0.178*	0.225**	0.221**
	(0.094)	(0.100)	(0.100)	(0.096)	(0.096)
Bank equity-to-asset	0.816	0.752	0.778	0.655	0.677
	(0.578)	(0.604)	(0.603)	(0.583)	(0.582)
Bank income-to-asset	0.080	3.067	3.210	0.660	0.790
	(4.328)	(4.550)	(4.543)	(4.343)	(4.339)
Firm size	-0.154***	-0.157***	-0.155***	-0.078***	-0.077***
	(0.006)	(0.010)	(0.010)	(0.010)	(0.010)
Firm profitability	-3.614***	-2.257***	-2.315***	-1.771***	-1.821***
	(0.222)	(0.212)	(0.213)	(0.198)	(0.199)
Firm tangibility	-0.241***	-0.218***	-0.213***	-0.098	-0.094
	(0.038)	(0.063)	(0.063)	(0.062)	(0.062)
Firm leverage	0.670***	0.514***	0.511***	0.297***	0.296***
	(0.025)	(0.035)	(0.035)	(0.035)	(0.035)
Firm markettobook	-0.037***	-0.024***	-0.025***	-0.016***	-0.017***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Stock return			0.146***		0.124***
			(0.039)		(0.039)
Credit rating				-0.096***	-0.096***
				(0.005)	(0.005)
Year × Quarter FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	No	No	No
Firm FE	No	Yes	Yes	Yes	Yes
Observations	12522	12275	12275	12275	12275
R-Squared	0.555	0.714	0.715	0.730	0.730

1.6.2 Investor horizon

Previous studies show that longer horizon investors are likely to show preference for high-ESG firms and allocate more portfolio weight accordingly since longer horizon investors are more patient with high-ESG firms (Starks, Venkat, and Zhu, 2017). Therefore, banks may believe that a large proportion of PRI investors should have long investment horizon hence sell less after firm's poor stock performance or negative shocks. In parallel with this rationale, firms with higher PRI ownership are likely to have more patient investors, thus less likely to face short-term volatility. If banks take investor horizon and short-term volatility into consideration when setting loan spreads, then it is reasonable for banks to charge lower spreads to firms with higher PRI ownership. To check if this rationale is evident in the data, I conduct facility-level analysis on loan maturity. I expect the maturity of loans issued to firms with higher PRI ownership to be longer controlling for loan, firm, and bank characteristics, owing to their potentially longer investor horizon and less short-term volatility.

Table 1.8 report the results on loan maturity using six models. All models include year-by-quarter fixed effects while model (1)(3)(5) and model (2)(4)(6) include additional industry and firm fixed effects, respectively. All the within industry analysis yield positive and statistically significant results on loan maturity, even when controlling for firm credit risks. One standard deviation increase in PRI ownership would lead to 0.5 ($\approx$ 0.032*17) months increase in loan maturity. Looking at the within firm analysis, the coefficient on PRI ownership is positive and similar in magnitude to the within industry analysis for model (2) and (4). However, the addition of firm credit risk in model (6) renders the coefficient much smaller and insignificant. These results suggest that banks may take the potential benefits of long horizon investors into account for high PRI ownership firms, however firm credit risk plays a larger role in the decision process.

Table 1.8: Effect of PRI ownership on loan maturity

This table reports the effect of lagged one quarter firm PRI ownership on new loan spread. The dependent variable is the maturity (in months) of each new facility. *PRI ownership* is defined as the proportion of a firm shares that is owned by PRI US mutual funds in the previous quarter. *Creditrating* is the ordinal numerical ratings obtained from S&P. I also control for facility-, firm-, and bank-level characteristics in the previous quarter. The values in parentheses are the standard errors. Firm, time, and industry effects are used. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Loan maturity						
PRI ownership	16.745***	13.132**	16.617***	13.005**	19.051***	7.726
	(4.933)	(6.469)	(4.932)	(6.476)	(6.031)	(7.777)
Facility amount	0.000	0.001**	0.000	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
All in drawn	0.021***	0.022***	0.020***	0.022***	0.029***	0.028***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Bank loan-to-asset	-16.627***	-13.901***	-16.527***	-13.910***	-20.753***	-17.945***
	(2.648)	(3.325)	(2.651)	(3.325)	(3.959)	(4.452)
Bank equity-to-asset	-21.310	-12.664	-21.659	-12.712	12.140	7.945
	(18.224)	(22.065)	(18.234)	(22.071)	(26.650)	(27.829)
Bank income-to-asset	-197.340**	-137.213	-200.839**	-136.187	-66.015	24.831
	(99.393)	(120.581)	(99.405)	(120.531)	(171.546)	(186.217)
Firm size	0.863***	1.335***	0.871***	1.353***	0.210	1.332***
	(0.136)	(0.342)	(0.136)	(0.343)	(0.219)	(0.404)
Firm profitability	30.046***	30.622***	29.427***	30.144***	25.521***	31.104***
	(4.282)	(5.909)	(4.297)	(5.935)	(5.992)	(7.314)
Firm tangibility	1.134	2.301	1.185	2.314	-1.030	0.535
	(1.011)	(2.211)	(1.011)	(2.210)	(1.305)	(2.530)
Firm leverage	0.925	-10.185***	0.944	-10.200***	0.941	-7.952***
	(0.791)	(1.239)	(0.790)	(1.239)	(1.000)	(1.407)
Firm markettobook	0.164***	0.290***	0.155***	0.285***	0.126**	0.288***
	(0.044)	(0.080)	(0.044)	(0.080)	(0.061)	(0.089)
Stock return			2.184*	1.186		
			(1.263)	(1.328)		
Credit rating					0.201**	0.245
					(0.101)	(0.175)
Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	No	Yes	No
Firm FE	No	Yes	No	Yes	No	Yes
Observations	18673	17736	18673	17736	12522	12275
R-Squared	0.257	0.478	0.258	0.478	0.231	0.417

1.7 Conclusion

In this paper, I provide evidence on the effect of firm PRI ownership by mutual fund on bank loan amount and loan spread. On the one hand, I show that there is no positive impact of PRI ownership on firm ESG scores in the following year, suggesting "greenwashing" behaviors from the funds' side. On the other hand, banks pay attention to firm ESG ownership structure, increasing lending amount and reducing interest rate to firms with higher PRI ownership. These effects cannot be explained by potential omitted variables, such as firm credit rating or stock return. I find that compared to other banks, banks that demonstrate environmental awareness by adopting the Equator Principles reduce the loan spread to a greater extent when lending to firms with higher PRI ownership. Banks may also factor in the potential benefits of long horizon investors of high PRI ownership firms, thus providing loans at a lower cost to high PRI ownership firms.

These findings are relevant for banks in evaluating the ESG aspects of potential borrowers. Banks should be aware that there is no clear relationship between PRI ownership and ESG performance and re-consider if they associate PRI ownership with firm ESG performance. However, banks may care about other potential benefit of high PRI ownership, for example longer horizon investors or investors with better ESG reputation, when making decisions about loan amount and loan spread. Thus, one interesting extension of this paper is to study whether banks make informed decisions about firm PRI ownership structure and are financially better-off by lending to high PRI ownership firms at a lower interest rate.

CHAPTER 2

IN IT FOR THE LONG HAUL: INVESTOR LOYALTY FOLLOWING NATURAL DISASTERS

2.1 Introduction

Over the past few decades, natural disasters have occurred more often and become a more important threat facing humanity.[1] Despite the fact that natural disasters are causing more damages due to their higher frequency and severity, little is known in the finance literature about how investors respond to firms that are impacted by natural disasters and whether investors still exhibit local bias in times of natural disasters. Filling this gap is important since we can better understand not only how investors perceive climate risks under local bias, but also the real impact of local bias on firms that are impacted by natural disasters.

In this paper, we study how mutual funds react to natural disasters and if mutual funds exhibit local bias towards firms that are hit by natural disasters. We identify local mutual funds by exploiting the distance between mutual fund headquarters and disaster zone counties and compare the responses of local funds and non-local funds in the disaster quarter.[2] If local bias exists during natural disasters, local mutual funds should overweight the disaster zone firms compared to their non-local counterparts. We first examine the quarterly change in normalized shares held by local and non-local funds for

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[1]United Nations news article: "Natural disasters occurring three times more often than 50 years ago: new FAO report". https://news.un.org/en/story/2021/03/1087702

[2]In the baseline results, we define local mutual funds using the within 75 miles criterion between the disaster county and mutual fund headquarter. We present the results for other distance measures in the robustness section.

each firm, then conduct within fund style comparison to see if local funds overweight disaster zone firms in the disaster quarters. Last, we further investigate if a greater extent of local fund ownership prior to the disasters affects stock returns and corporate investment decisions in the disaster quarters.

We focus on hurricanes as natural disaster events in this paper. First, hurricanes are among the most common and economically destructive disasters in the US.[3] Hurricanes impact various states along the coasts of the US, thus providing geographical dispersion in disaster zone counties and local mutual funds for our analyses. Second, National Oceanic and Atmospheric Administration (NOAA) publishes real-time data on hurricanes, which are accessible to investors such as mutual funds. This is important for us to study the short-term response of mutual funds in the disaster quarters.

We use hurricane landfall data from NOAA from 1996 to 2017. We follow Kruttli, Roth Tran, and Watugala (2021) to identify disaster zone counties with centroid inside a certain radius of the eye location of each hurricane within 24 hours before and after hurricane landfall.[4] We combine those data with the Morningstar mutual funds quarterly holdings and fund characteristics, including fund headquarter locations, to construct changes in holdings of both local and non-local mutual funds. In our empirical analysis, we first look into the quarterly changes of normalized shares held by local and non-local funds separately at the firm-level. While on average local funds do not increase their normalized holdings of disaster zone firms in disaster quarters, non-local funds statistically significantly decrease their normalized holdings and the difference between the two types of funds is robust under firm fixed effects and industry interacted with time fixed effects. We find that, on average, non-local mutual funds decrease their normalized shares holding of disaster-zone firms 0.22 percentage points more than local mutual funds in the disaster quarter.

[3]For instance, in 2017, $265 billion of .the aforementioned $300 billion in damages from extreme weather events in the US were due to hurricanes.

[4]We use a radius of 150 miles in the baseline results and present the results for other radius measures in the robustness section.

The firm-level analysis suggests that local funds, compared to non-local funds, do exhibit local bias towards disaster zone firms. We further examine mutual fund portfolio weights to see if the same pattern is present at the portfolio-level. We construct the quarterly county-level portfolio weight for each fund by aggregating the portfolio weight of firms headquartered in the county. Since different fund styles may adopt different investment strategies, we compare county portfolio weights within the same 3×3 Morningstar fund style[5] following Pool, Stoffman, and Yonker (2012). We also control for the endogenous relationship between a fund-county pair by including the fund interacted with county fixed effects. The within fund style comparison yields consistent results with the firm-level analysis. Compared to non-local funds in the same fund style, local mutual funds on average increase the portfolio weight of disaster zone counties by 4.6 basis points in the disaster quarter.

After establishing the result that local mutual funds sell relatively less and tilt their portfolios towards firms in the disaster zone in the disaster quarters, we investigate the real impacts of this local bias on disaster zone firms. We define "local gap" as the difference in the normalized percentage of shares owned by local mutual funds and non-local mutual funds for each firm in each quarter and denote the lagged one quarter "local gap" as a proxy for the ex ante local bias. We find that higher lagged "local gap" is associated with higher monthly excess return, which is in line with the local bias mechanism. Furthermore, disaster zone firms with relatively higher lagged "local gap" tend to make more short-term investment, which suggests that they may suffer less from the disasters financially and are more financially active.

Our study builds on and contributes to two different finance literature. First, our paper contributes to the fast-growing climate finance literature. Recent climate finance papers look at the effects of climate shocks on financial market outcomes (Bansal, Kiku, and Ochoa, 2016; Bansal, Ochoa, and Kiku, 2017; Meng, 2017; Hong, Li, and Xu, 2019;

[5]The 3×3 Morningstar fund styles are: US Large Blend, US Large Growth, US Large Value, US Mid-Cap Blend, US Mid-Cap Growth, US Mid-Cap Value, US Small Blend, US Small Growth, and US Small Value.

Addoum, Ng, and Ortiz-Bobea, 2021). In particular, some papers study how market and professionals responses to natural disasters. Kruttli, Roth Tran, and Watugala (2021) find that stock options of firms facing potential hurricane landfall risks exhibit large increase in implied volatility. Dessaint and Matray (2017) and Alok, Kumar, and Wermers (2020) show that corporate and fund managers that are located closer to the disaster zone over-act to the disaster risks due to salience. Our paper also finds that non-disaster zone funds exhibit greater local bias in the disaster quarters, which is consistent with the results in Alok, Kumar, and Wermers (2020). In addition, the within fund style comparison in our paper suggests that local bias plays a larger role than salience bias in fund managers' decisions in the disaster quarters.

Second, our paper is related to the home and local bias literature. Home bias is a widely documented phenomenon in the portfolio choices of professional investors. Research has shown that various types of professional investors overweight local equities (Coval and Moskowitz, 1999, 2001) and firms headquartered in managers home states (Pool, Stoffman, and Yonker, 2012) due to either familiarity or an information advantage. Despite the well-documented local bias of professional investors, little is known about how local bias plays a role in portfolio choices shortly after natural disasters. Our paper shows that fund managers exhibit local bias even in destructive events, such as hurricanes, and such local bias is useful for firms to financially recover from the disasters.

Our paper is organized as follows. We introduce our empirical research design and data in Section 2. Section 3 presents the baseline empirical results. We report the results using alternative measures of "local" funds and disaster zone in Section 4 and conclude in Section 5.

2.2 Empirical design

2.2.1 Methodology

We employ a difference-in-differences method to estimate the response of local mutual funds in the disaster quarters at the firm- and fund-level, as well as the real impacts on firms. In the baseline results, we define disaster zone firms as those firms headquartered in counties within 150 miles from the eye of each hurricane. Local mutual funds refer to mutual funds that headquartered in counties within 75 miles radius from a firm's headquarter county.[6]

Firm analysis

We collapse the Morningstar quarterly mutual fund holdings data to firm-quarter level and calculate the total number of shares held by local funds and non-local funds in each quarter for each firm. We then compute the normalized shares, the dependent variables, by dividing the local shares and non-local shares by the total outstanding shares. We examine how local and non-local mutual funds shares change in the hurricane quarters by estimating the following panel regression model

$$\Delta S_{ijt} = \beta_0 + \beta_1 disaster_{it} + firm\ controls_{it-1} + \alpha_i + \phi_{st} + \varepsilon_{ijt}, \tag{2.1}$$

where the dependent variable is the change in normalized local mutual fund holdings ($j = 1$) or non-local mutual fund holdings ($j = 0$) for firm i from quarter $t-1$ to t. $Disaster_{it}$ is a binary variable that indicates whether firm i is a disaster zone firm in quarter t. We control for firm characteristics (size, profitability, tangibility, leverage, market-to-book ra-

[6] We present the results for other distance measures in the robustness section.

tio) in the previous quarter as these characteristics may affect mutual funds' investment decisions in t. We include industry-by-time fixed effects based on firm two-digit SIC numbers and firm fixed effects. Since the disaster zone firms are defined at the county-level, we cluster standard errors by the firm's headquarter county (Abadie, Athey, Imbens, and Wooldridge, 2017).

Fund analysis

To check if the results of the firm-level analysis are consistent with fund portfolio adjustments, we look into fund-level analysis by collapsing fund holdings to fund-county-quarter level. In other words, we study how mutual funds adjust the county portfolio weights in the disaster quarter using the following regression model

$$W_{fct} = \beta_0 + \beta_1 disaster_{ct} \times local_{fc} + fund\ controls_{it-1} + \alpha_{sct} + \phi_{fc} + \varepsilon_{fct}, \tag{2.2}$$

where the dependent variable is the quarterly county portfolio weight for county c and fund f in quarter t. $Local_{fc}$ is a binary variable that indicates if mutual fund f is headquartered in a county within 75 miles of county c. We include fund style × county × time and fund × county fixed effects. The first set of fixed effects ensures that the regression variation comes from mutual funds in the same 3×3 Morningstar category while with different distance from the disaster counties. This within category comparison is important for the interpretation of the estimated coefficients, which denote the overweight or underweight of local mutual funds towards disaster counties compared to the benchmark portfolio weight in the same Morningstar category (Pool, Stoffman, and Yonker, 2012). The second set of fixed effects captures the time-invariant investment relationship between each fund and county pair. We also control various fund characteristics, such as fund size, return, expense ratio, and turnover ratio, that might affect fund portfolio re-weighting decisions.

The standard errors are clustered by the fund headquarter county since funds headquartered in the same county have the same exposure to hurricanes in local counties.

Firm outcomes

Furthermore, we delve into the real impacts of mutual fund local bias on firm outcomes in hurricane quarters by estimating the model

$$\begin{aligned} Y_{it} &= \beta_0 + \beta_1 disaster_{it} \times local\ gap_{it-1} + \beta_2 disaster_{it} + \beta_3 local\ gap_{it-1} \\ &+ firm\ controls_{it-1} + fixed\ effects + \varepsilon_{it}, \end{aligned} \tag{2.3}$$

where Y_{it} denotes the outcome variables for firm i in quarter t and $disaster_{it}$ indicates if firm i is in the disaster zone in quarter t. We define a new variable, local gap, which is the difference between the normalized share percentage owned by local mutual funds and non-local mutual funds for firm i in $t-1$. This variable serves as a proxy for the extent of local bias and we investigate the interactive effect of the lagged local gap and disaster on firm outcomes. A statistically significant estimate for β_1 suggests extent of local bias in the previous quarter does affect firm outcomes in the disaster quarter heterogeneously. We cluster the standard errors at the firm headquarter county level to account for the geographical nature of the disasters.

2.2.2 Data

Hurricane landfall regions

We use hurricane track data collected from the NOAA hurricane archives. These data show the actual location and intensity of the hurricane's eye at various points of time.

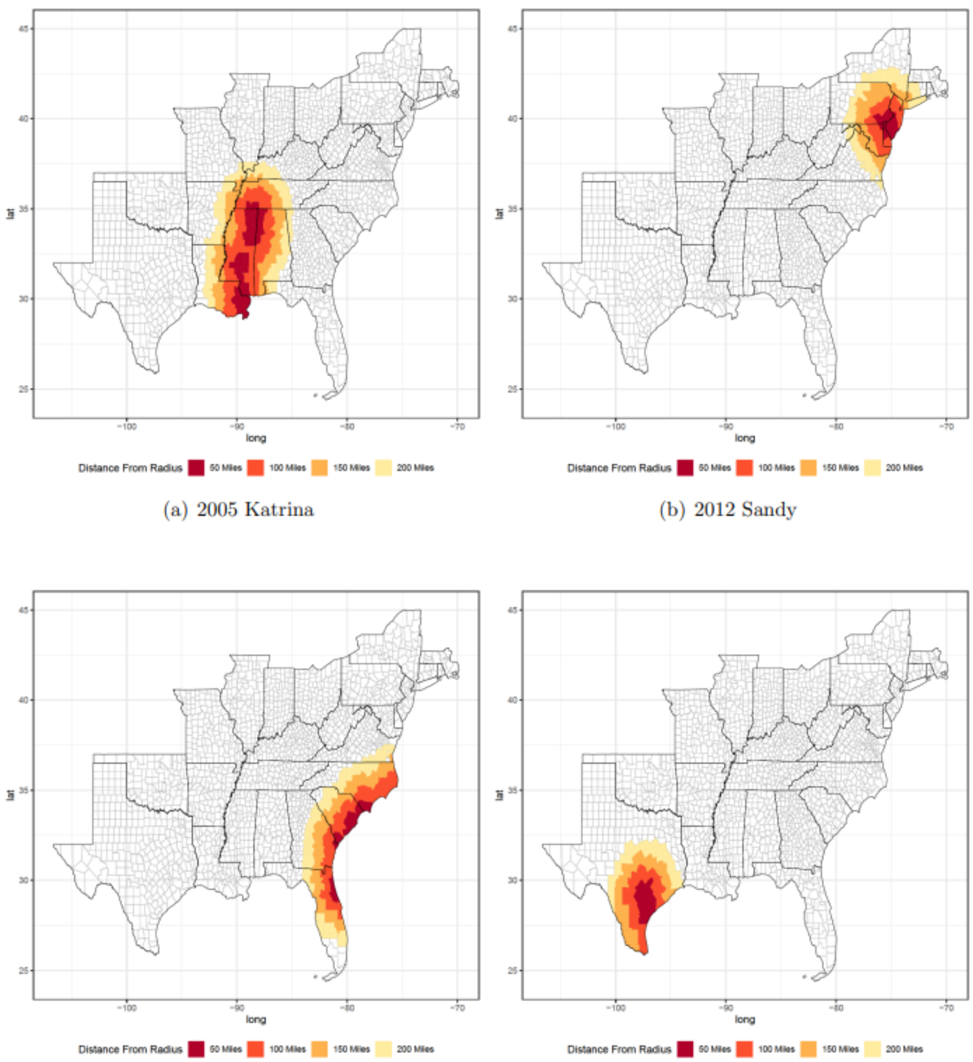
We follow the procedures outlined in Kruttli, Roth Tran, and Watugala (2021) to identify hurricane landfall regions. We first identify counties with centroid inside a certain radius of the eye location of each hurricane within 24 hours before and after hurricane landfall. In the baseline results, we consider disaster zone counties to be the set of counties that lie within 150 miles of the landfall.[7] We also generate the set of counties that lie within 100 or 200 miles of the landfall and report the results using these alternative definitions of disaster zone counties in robustness checks in Section 4. Figure 2.1 shows which counties fall within each set for the hurricanes: 2005 Katrina, 2012 Sandy, 2016 Matthew, and 2017 Harvey. We include in total 33 hurricane [8] in our sample from 1996 to 2017.

Mutual funds

We obtain the quarterly mutual fund holdings and fund characteristics data from Morningstar. Morningstar historical monthly holdings data contains the weighting and number of shares of every stock owned by U.S. mutual funds. We use the quarterly holdings data from 1996 to 2017 in this paper. We limit the fund sample to only actively managed U.S. equity funds by mostly following the steps proposed by Pástor, Stambaugh, and Taylor (2015). Specifically, we remove bond funds, international funds, sector funds, target funds, real estate funds, other non-equity funds, index funds, and funds of funds by examining keywords in the Morningstar Category variable or directly using the indicator variables in the fund characteristics data. To further filter actively managed funds, we exclude single share class funds with expense ratio below 0.1 since it is very unlikely for actively managed mutual funds to charge extremely low investment fees so that the ex-

[7]Two hurricanes in the sample, 2004 Charley and 2005 Katrina, made two landfalls in the US. To avoid double-counting with these two hurricanes, the date when the hurricane made landfall at a higher wind speed—corresponding to a higher storm category on the Saffir-Simpson scale—is considered the landfall date in our analysis.

[8]In the order of landfall time: Bertha, Fran, Danny, Bonnie, Earl, Georges, Bret, Floyd, Irene, Lili, Claudette, Isabel, Charley, Frances, Ivan, Jeanne, Dennis, Katrina, Rita, Wilma, Humberto, Dolly, Gustav, Ike, Irene, Issac, Sandy, Arthur, Hermine, Matthew, Harvey, Irma, Nate



(a) 2005 Katrina

(b) 2012 Sandy

(c) 2016 Matthew

(d) 2017 Harvey

Figure 2.1: Counties in a hurricane landfall region

This figure highlights the counties that are within 50, 100, 150, and 200 miles of the eye of the hurricane at landfall for four selected hurricanes from our sample of 33 hurricane landfalls. *Source: Kruttli, Roth Tran, and Watugala (2021)*

pense ratio is below 0.1 (Pástor, Stambaugh, and Taylor, 2015). For funds with multiple share classes, we exclude those funds with lag-TNA-weighted expense ratio below 0.1. Furthermore, we keep those funds with a Morningstar Category in the 3-by-3 size/value grid (US Large Growth, US Large Value, US Large Blend, US Mid-Cap Growth, US Mid-Cap Value, US Mid-Cap Blend, US Small Growth, US Small Value, US Small Blend) following Pool, Stoffman, and Yonker (2012). After the fund filtering process, there are 3,116 unique funds remaining in the sample. Following Pool, Stoffman, and Yonker (2012), we conduct within Morningstar fund category analysis, which means that we are comparing the overweight or underweight towards disaster counties for local mutual funds compared to the benchmark portfolio weight in the same Morningstar fund category. To do this, we fill in zeros for those missing fund-county-quarter portfolios. That is, we consider the county portfolio weight in a quarter to be zero if a fund does not hold shares of any firms headquartered in that county in a quarter. Table 2.1 reports the summary statistics for the fund-level analysis. W_{fct} is the portfolio weight of county c for mutual fund f in quarter t. Since we fill in zeros for the county portfolio weight, the majority value for W_{fct} is zero. We also report W_{fct} for non-disaster zone funds and coastal funds. We see from $No.\ firms_{ct}$ that around half of county-quarter observations are with only one firm headquarter, so we present the summary statistics for counties with at least two firm headquarters in order to make the county portfolio weights reflect funds' preference for counties instead of specific firms. We include 675 unique counties in the sample. The fund-county binary variable *Local* indicates if a fund is headquartered in a county that is within 75 miles radius of a disaster county and the county-quarter binary variable *disaster* takes the value one if a county is in the disaster zone.

Table 2.1: Summary statistics for fund-level analysis

This table reports the summary statistics on the main variables included in the fund-level analysis. The sample is from 1996 to 2017. W_{fct} is the weight of county c in fund f's portfolio in quarter t. *Local* and *Disaster* are binary variables and $No.\ firms$ indicates the number of firm headquarters in the county.

variable	N	mean	min	p25	p50	p75	p90	p95	p99	max	sd
W_{fct}	3.73e+07	0.232	0.000	0.000	0.000	0.000	0.452	1.591	4.618	68.312	0.954
W_{fct} (non-disaster zone funds)	3.67e+07	0.232	0.000	0.000	0.000	0.000	0.451	1.591	4.617	68.312	0.954
W_{fct} (non-disaster zone funds, coastal)	1.93e+07	0.227	0.000	0.000	0.000	0.000	0.428	1.573	4.607	38.992	0.904
W_{fct} (non-disaster funds, coastal, $\geq$ 2 firms)	1.35e+07	0.310	0.000	0.000	0.000	0.000	0.958	2.126	5.358	38.992	1.057
W_{fct} (non-disaster funds, coastal, $\geq$ 2 firms, disaster-prone counties)	1.25e+07	0.331	0.000	0.000	0.000	0.000	1.064	2.248	5.533	38.992	1.093
$Local_{fc}$	1.38e+06	0.030	0.000	0.000	0.000	0.000	0.000	0.000	1.000	1.000	0.169
$Disaster_{ct}$	36923	0.018	0.000	0.000	0.000	0.000	0.000	0.000	1.000	1.000	0.132
$No.\ firms_{ct}$	36923	6.662	1.000	1.000	2.000	6.000	16.000	25.000	81.000	199.000	14.704

Firm outstanding shares and characteristics

We obtain firm quarterly outstanding shares, headquarter county, as well as other financial variables from CRSP-Compustat. We exclude financial firms with SIC numbers from

6000 to 6799 from our analysis as it is hard to quantify the geographic exposure to natural disasters for financial firms. We report summary statistics on our sample of firms in table 2.2. The first two rows show the percentage of firm i's outstanding shares in quarter t that is held by local mutual funds and non-local mutual funds, respectively. Notice that the percentage is much higher for non-local mutual funds due to the fact that there are much more non-local than local mutual funds for each firm. The third and fourth rows report the quarterly changes in holding share percentages for local and non-local mutual funds, respectively. These two variables serve as the dependent variables in table 2.3 column (1)-(4). We stack ΔS_{i1t} and ΔS_{i0t} together to obtain the variable ΔS_{ijt}, with j takes the value one for local mutual funds and zero otherwise. The number of observations for ΔS_{ijt} is exactly the sum of the number of observations of ΔS_{i1t} and ΔS_{i0t}. Since we winsorize the variables at 0.5 percentile, the distribution of ΔS_{ijt} is slightly different than taking the sum of two variables. ΔS_{ijt} is the dependent variable in table 2.3 column (5)-(6). The last three rows are relevant to the firm outcome analysis in Section 3.3. On average, non-local mutual funds hold 7.78 percentage points more shares than local mutual funds. The average monthly excess return in our sample is around 1% and the median is around 0.53%. There are many observations with missing shortterm investment values so the sample size shrinks for $log(shortterminvestment)$. We take the log transformation of shortterm investment to make the distribution approximately conform to normality.

Table 2.2: Summary statistics for firm-level analysis

This table reports the summary statistics on the main variables included in the firm-level analysis. The sample is from 1996 to 2017. S_{i1t} and S_{i0t} are the normalized share percentage of firm i held by local and non-local mutual funds in quarter t, respectively. *Local gap* is the difference in the normalized share percentage held by local mutual funds and non-local mutual funds for firm i in t.

variable	**N**	**mean**	**min**	**p25**	**p50**	**p75**	**p90**	**p99**	**max**	**sd**
S_{i1t}	199,875	0.532	0.000	0.000	0.000	0.230	1.715	7.982	7.982	1.371
S_{i0t}	199,875	8.306	0.000	2.267	6.666	12.698	18.702	29.782	29.782	7.151
ΔS_{i1t}	199,864	0.002	-1.826	0.000	0.000	0.000	0.105	1.781	1.825	0.371
ΔS_{i0t}	199,864	0.038	-8.539	-0.653	0.002	0.807	2.289	7.551	7.682	2.236
ΔS_{ijt}	399,728	0.022	-6.334	-0.085	0.000	0.154	1.261	5.841	5.841	1.506
$Local\ gap_{it}$	199,875	-7.780	-32.210	-12.032	-6.031	-1.920	-0.313	2.469	4.149	7.192
$Monthly\ excess\ return_{it}$	199,101	1.001	-46.807	-6.308	0.526	7.527	16.709	51.182	66.167	14.810
$Log(shortterm\ investment)_{it}$	42,619	3.091	-6.908	1.624	3.296	4.694	6.019	8.846	8.846	2.464

2.3 Results

We first analyze the change in local and non-local mutual fund holdings in the disaster quarter at the firm level. Then, we examine the portfolio responses at the fund level. Finally, we investigate the economic impacts of the local bias on firms in the disaster zone.

2.3.1 Firm-level analysis

We begin our analysis by looking into the different changes in mutual fund holdings at the firm level. We calculate the shares of local and non-local mutual funds in each quarter (normalized by the total shares outstanding) and their changes in consecutive quarters, respectively. In table 2.3, we define local mutual funds as those mutual funds headquartered in counties within 75 miles from the firm's headquarter county. We show that the results are robust to different definitions of local funds in Section 5. The dependent variable ΔS_{i1t} refers to the change in local mutual fund shares from quarter $t-1$ to quarter t for firm i and ΔS_{i0t} refers to the change in non-local mutual fund shares from quarter $t-1$ to quarter t for firm i. *Disaster* is a binary variable that takes the value one if firm i is in the disaster zone in quarter t and zero otherwise. Column (1)-(4) present the effect of disaster on changes in local and non-local mutual fund holdings with firm and industry-by-time fixed effects and firm covariates in the previous quarter. The estimated coefficient on *disaster* is positive for local mutual fund and negative and statistically significant for non-local mutual fund, indicating that non-local mutual fund reduce their holdings of the disaster-zone firms in the disaster quarter. Compared with non-disaster quarters, non-local mutual fund on average decrease their shares of disaster-zone firms by 0.11 percentage points. Column (5)-(6) combine the two data samples with *local* equal to one if ΔS_{i1t} is for local mutual fund and zero if ΔS_{i1t} is for non-local mutual fund. The results show that compared with the levels in non-disaster quarters, non-local mutual fund decrease their shares of disaster-zone firms 0.22 percentage points more than local mutual fund in the disaster quarter. In other words, local mutual fund tend to sell less disaster-zone firms in the disaster quarter, demonstrating local bias towards nearby firms in the events of natural disasters.

Table 2.3: Normalized share holding response to disaster zone firms

This table reports the coefficient estimates from the following regression model.

$$\Delta S_{ijt} = \beta_0 + \beta_1 disaster_{it} + firm\ controls_{it-1} + \alpha_i + \phi_{st} + \varepsilon_{ijt},$$

where the dependent variable is the change in normalized local mutual fund holdings ($j = 1$) or non-local mutual fund holdings ($j = 0$) for firm i from quarter $t-1$ to t. $Disaster_{it}$ is a binary variable that indicates whether firm i is a disaster zone firm in quarter t. We control for firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter as these characteristics may affect mutual funds' investment decisions in t. We include industry-by-time fixed effects based on firm two-digit SIC numbers and firm fixed effects. The standard errors are clustered by the firm's headquarter county. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
	Within 75 miles		Outside 75 miles		All	
Dependent Variable:	ΔS_{i1t}	ΔS_{i1t}	ΔS_{i0t}	ΔS_{i0t}	ΔS_{ijt}	ΔS_{ijt}
Local × disaster					0.211***	0.221***
					(0.057)	(0.060)
Disaster	0.009	0.009	-0.114**	-0.115***	-0.153***	-0.158***
	(0.010)	(0.011)	(0.044)	(0.044)	(0.046)	(0.046)
Local					-0.046***	-0.011*
					(0.003)	(0.007)
Firm size		0.003		-0.203***		-0.089***
		(0.003)		(0.014)		(0.007)
Firm profitability		0.038*		0.764***		0.390***
		(0.020)		(0.135)		(0.074)
Firm tangibility		-0.017		-0.290***		-0.164***
		(0.017)		(0.074)		(0.038)
Firm leverage		-0.006		0.033		-0.001
		(0.009)		(0.042)		(0.022)
Firm markettobook		0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)
S_{i1t-1}		-0.071***		0.004		-0.033***
		(0.011)		(0.003)		(0.008)
S_{i0t-1}		-0.000		-0.000***		-0.000***
		(0.000)		(0.000)		(0.000)
S_{i1t-1} × Local						-0.062***
						(0.008)
S_{i0t-1} × Local						0.000***
						(0.000)
Industry × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	198,816	198,816	198,816	198,816	398,784	398,784
R-Squared	0.052	0.104	0.147	0.149	0.076	0.080

2.3.2 Fund-level analysis

Next, we look into fund portfolios to see if the same pattern is present at the portfolio-level. For each mutual fund f, we construct the quarterly county-level portfolio weight W_{fct} by aggregating the portfolio weight of firms headquartered in the county. We adopt the same definition of "local" mutual fund and disaster zone as the firm-level analysis. Local mutual funds refer to those mutual funds headquartered within 75 miles of each county and a county is considered as in the disaster zone if it is within 150 miles of the eye of hurricane landfall. Table 2.4 shows the results of the portfolio-level analysis. All columns in table 2.4 include fund style-by-county-by-time and fund-by-county fixed effects. The first set of fixed effects ensures that the regression variation comes from mutual funds in the same 3X3 Morningstar fund style while with different distance from the disaster counties. This within fund style comparison is important for the interpretation of the estimated coefficients, which denote the overweight or underweight of local mutual funds towards disaster counties compared to the benchmark portfolio weight in the same Morningstar fund style. The second set of fixed effects captures the time-invariant investment relationship between each fund and county. All columns in table 2.4 control for fund characteristics, such as fund size, return, expense ratio, and turnover ratio. Column (1) reports the estimated coefficient of interest using all mutual funds in the sample while we exclude those mutual funds headquartered in disaster zones in column (2). Alok, Kumar, and Wermers (2020) find that fund managers in the disaster region tend to overreact due to the salience bias, thus underweighting the disaster zone stocks. Consistent with this argument, we find that estimated coefficient in column (2) is much larger that in column (1). Compared to other funds in the same MS fund style, local mutual funds increase the portfolio weight of disaster zone stocks by 4.6 basis points in the disaster quarter. Next, we show that the result holds for funds headquartered in the coastal states (states that ever hit by hurricanes) in column (3). In this county-level portfolio weight analysis, counties with none corporate headquarters always get a portfolio weight of zero. Therefore, we examine the sub-group of counties which are headquarters for at least two firms in column (4) and (5), with column (5) further excluding counties that never experience hurricanes. Unsurprisingly, the estimates in column (4) and (5) are greater in magnitude after excluding the zero headquarter counties. In short, we do find that mutual funds exhibit local bias in disaster quarters from the portfolio analysis.

Table 2.4: Portfolio weight response to disaster zone firms

This table reports the coefficient estimates from the following regression model.

$$W_{fct} = \beta_0 + \beta_1 disaster_{ct} \times local_{fc} + fund\ controls_{it-1} + \alpha_{sct} + \phi_{fc} + \varepsilon_{fct},$$

where the dependent variable is the quarterly county portfolio weight for county c and fund f in quarter t. *Local* is a binary variable that indicates if mutual fund f is headquartered in a county within 75 miles of county c. We include fund style × county × time and fund × county fixed effects. All mutual funds are included in column (1) while only those funds headquartered in non-disaster counties in the disaster quarters are included in column (2). In column (3), we exclude mutual funds in non-coastal states. We further restrict the sample to counties with at least two firm headquarters in column (4). In the last column, we only include counties that are ever hit by hurricanes. The standard errors are clustered by fund headquarter county. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

Dependent Variable: W_{fct}	(1) All funds	(2) Non-disaster zone funds	(3) Non-disaster zone funds Coastal states	(4) Non-disaster zone funds Coastal states At least 2 firms	(5) Non-disaster zone funds Coastal states At least 2 firms Only disaster-prone counties
Local × disaster	0.015*	0.046**	0.049***	0.064***	0.064***
	(0.008)	(0.018)	(0.018)	(0.024)	(0.024)
Fund control	Yes	Yes	Yes	Yes	Yes
Fund category × County × Time FE	Yes	Yes	Yes	Yes	Yes
Fund × County FE	Yes	Yes	Yes	Yes	Yes
Observations	23,052,404	22,623,932	11,844,861	8,216,198	7,533,786
R-Squared	0.653	0.653	0.624	0.623	0.624

2.3.3 Firm outcomes

In the previous sections, we show the existence of mutual fund local bias towards nearby disaster zone firms in the disaster quarter. One follow-up question that we are interested in is whether local bias has any real impacts on disaster zone firms. To further study this question, we define a new variable, local gap, which denotes the difference in the normalized share percentage owned by local mutual funds and non-local mutual funds for each firm in each quarter. This variable serves as a proxy for the extent of local bias and we investigate the interactive effect of the lagged local gap and disaster on firm stock return and investment decisions. Table 2.3 shows that non-local mutual funds sell relatively more disaster zone firm shares in the disaster quarter, thus we examine if lagged local gap affects firm stock return in the disaster quarter. Table 2.5 column (1)-(3) present the effects on excess return. We include industry fixed effects in column (1) and firm fixed effects in column (2)-(3). The coefficient for the interaction term, $disaster \times localgap$, is the coefficient of interest, which is positive and statistically significant under both the the five-factor model (Fama and French, 2015) and the five-factor model with momentum. Controlling for lagged firm characteristics does not alter the coefficient much and the monthly excess return increases by around 0.0012 percentage points with one percentage point increase in lagged local gap.

Given that firms with higher lagged local gap experience higher return in the disaster quarters, it is likely that firms with higher lagged local gap suffer less from the disasters financially, thus being able to make more investments in the disaster quarters. We present the effects on corporate short-term investment in columns (4)-(6). Starting with the least stringent fixed effects in column (4), we include additional industry-by-state fixed effects to control for specific state-level industry shocks and industry-by-time fixed effects to control for specific industry-level shocks over time. We find consistent results that disaster zone firms with higher lagged local gap make relatively more short-time investments in the disaster quarter. Taking the within firm results with industry-by-time fixed effects in column (6) as an example, one percentage point increase in lagged local gap is associated with 0.017% increase in short-term investment for disaster-zone firms in disaster quarters.

Table 2.5: Effect of local bias on excess return and firm short-term investment

This table reports the coefficient estimates from the following regression model.

$$Y_{it} = \beta_0 + \beta_1 disaster_{it} \times local\ gap_{it-1} + \beta_2 disaster_{it} + \beta_3 local\ gap_{it-1} \\ + \text{firm controls}_{it-1} + fixed\ effects + \varepsilon_{it},$$

where Y_{it} denotes the outcome variables for firm i in quarter t and $disaster_{it}$ indicates if firm i is in the disaster zone in quarter t. We define a new variable, local gap, which is the difference in the normalized share percentage owned by local mutual funds and non-local mutual funds for firm i in t. The standard errors are clustered by firm headquarter county. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	Excess return			log(short-term investment)		
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster × Local gap	0.144***	0.138***	0.119**	0.024**	0.024**	0.017**
	(0.055)	(0.053)	(0.052)	(0.012)	(0.010)	(0.008)
Disaster	0.019	0.160	-0.105	0.058	0.011	0.002
	(0.383)	(0.379)	(0.419)	(0.077)	(0.071)	(0.062)
Local gap	-0.050***	0.054***	0.006	-0.000	0.000	-0.006*
	(0.004)	(0.007)	(0.008)	(0.005)	(0.005)	(0.004)
Rm-Rf	1.115***	1.128***	1.048***			
	(0.022)	(0.021)	(0.020)			
SMB	0.675***	0.649***	0.727***			
	(0.022)	(0.022)	(0.025)			
HML	0.125***	0.120***	-0.048			
	(0.041)	(0.040)	(0.044)			
RMW	0.173***	0.178***	0.217***			
	(0.040)	(0.039)	(0.039)			
CMA	-0.079**	-0.092**	-0.007			
	(0.035)	(0.036)	(0.034)			
MOM			-0.206***			
			(0.013)			
Firm size			-1.639***	0.811***	0.836***	0.744***
			(0.089)	(0.031)	(0.036)	(0.063)
Firm profitability			3.687**	-1.066**	-1.007**	-0.240
			(1.497)	(0.520)	(0.455)	(0.295)
Firm tangibility			-2.443***	-1.991***	-2.492***	-2.401***
			(0.606)	(0.372)	(0.418)	(0.341)
Firm leverage			0.083	-0.874***	-0.941***	-0.462***
			(0.350)	(0.263)	(0.197)	(0.131)
Firm markettobook			-0.006*	0.161***	0.128***	0.022***
			(0.004)	(0.018)	(0.016)	(0.008)
Time FE	No	No	No	Yes	Yes	No
Industry FE	Yes	No	No	Yes	No	No
State FE	No	No	No	Yes	No	No
Industry × State FE	No	No	No	No	Yes	No
Industry × Time FE	No	No	No	No	No	Yes
Firm FE	No	Yes	Yes	No	No	Yes
Observations	197715	197401	197401	42,504	42,469	41,944
R-Squared	0.111	0.166	0.171	0.428	0.534	0.812

2.4 Robustness

We conduct various robustness checks on both firm-level and fund-level analyses. In our main results, we define disaster zone firms as those firms headquartered in counties that lie within 150 miles of the eye of each hurricane. Local mutual funds refer to mutual funds that headquartered in counties that lie within 75 miles of a firm's headquarter county. Since these choices are somewhat arbitrary in nature, we re-do the main results using alternative definitions of disaster zone and local mutual fund, respectively. First, we adopt different distance measures of disaster zone, 100 miles and 200 miles instead of 150 miles and present the firm-level results in table 2.6. Compared to table 2.3, the estimates are similar in magnitude. Although the we find statistically insignificant result for non-local funds selling disaster zone firms in disaster quarters in column (2), the coefficients for the interaction term $disaster \times local$ are highly significant and close to the estimates in the main results. Therefore, non-local funds sell more disaster zone firms than local funds in the disaster quarters.

Next, we investigate if different definitions of local mutual fund affects the firm-level results. Table 2.7 presents the results using two alternative distance measures of local mutual funds: 100 and 150 miles instead of 75 miles. We estimate the model in Equation 2.1 again and results still hold for either definition of local mutual funds. One interesting comparison is that the coefficient for the interaction term under the local distance measure of 150 miles is smaller than those under the distance measures of 75 and 100 miles. This is consistent with the local bias mechanism but to examine this comparison further, we look into fund-level analysis to see if different distance measures of local mutual funds affect the coefficient in a monotonic way.

Table 2.8 shows the fund-level analysis results using different distance measures for disaster zone and local mutual fund. Column (1)-(2) replicate column (2) and (4) in the baseline result table 2.4. Column (3)-(4) keep the same definition of local mutual fund as the baseline result, while changing the distance measure for the disaster zone from 150 miles to 100 miles. Since all the models exclude mutual funds in the disaster zone, a different definition of disaster zone leads to a slightly different sample size for column (3)-(4), compared to other columns. Column (5)-(8) keep the same definition of disaster zone as the baseline result, while changing the distance measure for the local mutual fund. First, changing the distance measure for disaster zone does not affect the conclusion of the baseline results, although the magnitude of the effect is reduced (smaller number of local funds?). Interestingly, the estimated effect decreases as we increase the distance measure for local mutual funds, from 0.064 in column (2) to 0.047 in column (6) and 0.029 in column (8), suggesting that local bias towards disaster zone firms at the fund-level declines as the fund is located further from the disaster zone.

Table 2.6: Normalized share holding response to disaster zone firms: alternative definitions of disaster zone
This table reports the coefficient estimates from the following regression model.

$$\Delta S_{ijt} = \beta_0 + \beta_1 disaster_{it} + firm\ controls_{it-1} + \alpha_i + \phi_{st} + \varepsilon_{ijt},$$

where the dependent variable is the change in normalized local mutual fund holdings ($j = 1$) or non-local mutual fund holdings ($j = 0$) for firm i from quarter $t-1$ to t. $Disaster_{it}$ is a binary variable that indicates whether firm i is a disaster zone firm in quarter t. We control for firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter as these characteristics may affect mutual funds' investment decisions in t. We include industry-by-time fixed effects based on firm two-digit SIC numbers and firm fixed effects. The distance measures for disaster zone counties in column (1)-(3) and column (4)-(6) are 100 and 200 miles from the eye of hurricane landfalls, respectively. The standard errors are clustered by the firm's headquarter county. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	Disaster zone: within 100 miles of landfall			Disaster zone: within 200 miles of landfall		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	ΔS_{i1t}	ΔS_{i0t}	ΔS_{ijt}	ΔS_{i1t}	ΔS_{i0t}	ΔS_{ijt}
Local × disaster			0.225***			0.222***
			(0.082)			(0.054)
Local			-0.010			-0.013*
			(0.007)			(0.007)
Disaster	0.007	-0.090	-0.153**	0.014	-0.067*	-0.132***
	(0.010)	(0.060)	(0.064)	(0.010)	(0.036)	(0.039)
Firm control	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	198,816	198,816	398,784	198,816	198,816	398,784
R-Squared	0.104	0.149	0.080	0.104	0.149	0.080

Table 2.7: Normalized share holding response to disaster zone firms: alternative definitions of local MF

This table reports the coefficient estimates from the following regression model.

$$\Delta S_{ijt} = \beta_0 + \beta_1 disaster_{it} + firm\ controls_{it-1} + \alpha_i + \phi_{st} + \varepsilon_{ijt},$$

where the dependent variable is the change in normalized local mutual fund holdings ($j = 1$) or non-local mutual fund holdings ($j = 0$) for firm i from quarter $t-1$ to t. $Disaster_{it}$ is a binary variable that indicates whether firm i is a disaster zone firm in quarter t. We control for firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter as these characteristics may affect mutual funds' investment decisions in t. We include industry-by-time fixed effects based on firm two-digit SIC numbers and firm fixed effects. The distance measures for local mutual funds in column (1)-(3) and column (4)-(6) are 100 and 150 miles from a disaster zone county, respectively. The standard errors are clustered by the firm's headquarter county. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	Local mutual funds: within 100 miles of disaster county			Local mutual funds: within 150 miles of disaster county		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	ΔS_{i1t}	ΔS_{i0t}	ΔS_{ijt}	ΔS_{i1t}	ΔS_{i0t}	ΔS_{ijt}
Local × disaster			0.222***			0.206***
			(0.059)			(0.059)
Local			-0.008			0.000
			(0.006)			(0.007)
Disaster	0.009	-0.115***	-0.159***	0.009	-0.112***	-0.152***
	(0.011)	(0.043)	(0.046)	(0.014)	(0.040)	(0.044)
Firm control	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	198,816	198,816	398,784	198,816	198,816	398,784
R-Squared	0.105	0.148	0.081	0.108	0.146	0.083

Table 2.8: Portfolio weight response to disaster zone firms: alternative definitions of disaster zone and local MF

This table reports the coefficient estimates from the following regression model.

$$W_{fct} = \beta_0 + \beta_1 disaster_{ct} \times local_{fc} + fund\ controls_{it-1} + \alpha_{sct} + \phi_{fc} + \varepsilon_{fct},$$

where the dependent variable is the quarterly county portfolio weight for county c and fund f in quarter t. *Local* is a binary variable that indicates if mutual fund f is headquartered in a county within 75 miles of county c. We include fund style × county × time and fund × county fixed effects. Column (1)-(2) present the baseline results. Column (3)-(4) use 100 miles as the distance measure for disaster zone. Column (5)-(8) change the distance measure for local mutual funds to 100 and 150 miles, respectively. The standard errors are clustered by fund headquarter county. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

Disaster zone:	within 150 miles of landfall		within 100 miles of landfall		within 150 miles of landfall		within 150 miles of landfall	
Local mutual funds:	within 75 miles of disaster county		within 75 miles of disaster county		within 100 miles of disaster county		within 150 miles of disaster county	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: W_{fct}	Non-disaster zone funds	Non-disaster zone funds Coastal states At least 2 firms	Non-disaster zone funds	Non-disaster zone funds Coastal states At least 2 firms	Non-disaster zone funds	Non-disaster zone funds Coastal states At least 2 firms	Non-disaster zone funds	Non-disaster zone funds Coastal states At least 2 firms
Local × disaster	0.046**	0.064***	0.029***	0.033***	0.034**	0.047***	0.024*	0.029*
	(0.018)	(0.024)	(0.007)	(0.008)	(0.014)	(0.018)	(0.012)	(0.016)
Fund control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fund category × County × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fund × County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,623,932	8,216,198	22,893,119	8,312,307	22,623,932	8,216,198	22,623,932	8,216,198
R-Squared	0.653	0.623	0.653	0.624	0.653	0.623	0.653	0.623

2.5 Conclusion

In this paper, we examine whether professional investors exhibit local bias in times of natural disasters and how local bias affects firms in the disaster quarters. Although the disasters can be more salient to mutual funds located closer to disaster zone counties, we find that relative to distant funds, local mutual funds sell less disaster zone firms in the disaster quarters. Our within fund style analysis confirms that local mutual funds overweight disaster zone firms in the disaster counties and the overweighting is greater for the mutual funds not in the disaster zone. We further show that firms with higher ownership gap between local and non-local mutual funds prior to disasters have higher excess return and make more short-term investment in the disaster quarters.

Our results suggest that local bias has real impacts on firms in times of disasters. One potential method for firms in the disaster-prone areas to better recover from natural disasters financially is to attract more investment from local funds. Future research could examine the response of local and non-local mutual funds to other types of natural disasters and shed more light on the long-term effect of local bias on disaster zone firms.

CHAPTER 3

DIRECT AND SPILLOVER EFFECTS OF BANK LENDING FOLLOWING TARIFF REDUCTIONS

3.1 Introduction

Banks have always been the critical players in modern financial markets. It is commonly accepted that banks' lending ability is crucial to money creation and circulation in the whole economy. The 2007-2008 financial crisis, however, sheds light on another ability of banks that can result in profound impacts on the wider economy: the ability to transmit liquidity shocks. While recent evidence supports that large liquidity shocks on banks do have real impacts on borrowers (Khwaja and Mian, 2008; Santos, 2011; Chodorow-Reich, 2014; Cingano, Manaresi, and Sette, 2016), little has been shown about banks' ability as intermediaries which transmit negative shocks from a group of affected borrowers to other unaffected borrowers. This lack of evidence can potentially undermine the stability of financial sector as specific shocks at the industry- or firm-level, instead of bank-level, can have spillover effects on a wide range of unaffected firms through the bank-firm network. This paper aims to fill this gap and present empirical results for the spillover effects in the bank-firm network.

To do this, I consider significant industry-level tariff reductions as negative shocks for domestic U.S. firms in the affected industries. Clearly, domestic firms have to operate in much more competitive and riskier environments due to the lower cost of entry for foreign rivals to the U.S. market (Valta, 2012; Frésard and Valta, 2016). From the point of view of banks, firms in those affected industries (hereafter treated firms) face higher entry threat and have a higher probability of loan defaults than other firms (hereafter control firms). Therefore, liquidity risk should be higher for banks with a higher proportion of existing loans issued to treated borrowers. There are two main parts in this paper. First, I show that banks directly respond to higher liquidity risks by cutting new lending to treated borrowers compared to control borrowers. Second, I present some evidence suggesting that banks respond to higher liquidity risks by enhancing monitoring of large existing borrowers, thus creating spillover effects on control firms.

I use the detailed loan-level information from LPC Dealscan database. This comprehensive loan database not only allows me to study the effects of tariff reductions on new and existing borrowers separately, but also provides necessary bank-firm network information for calculating banks' exposure to the shocks and existing control borrowers' exposure to the shocks through bank monitoring. Using 9,271 term loans and revolvers issued by 452 banks to 2,495 publicly traded firms from 1988 to 2004, I empirically examine the impact of tariff reductions on bank new lending by comparing the amount of new loans issued to treated and control borrowers. I also investigate if prior bank-firm lending relationship affects banks' new lending to treated borrowers. Besides, I show some evidence suggesting the spillover effect of tariff reductions on existing control borrowers through the bank monitoring channel. The results are new in revealing the spillover effect in bank-firm networks and the role of lending relationship when borrowers suddenly face significantly higher market competition from foreign counterparts.

The baseline results can be summarized as follows. High-exposure banks decrease new lending to treated borrowers compared to control borrowers to a greater extent than low-exposure banks. Controlling for bank, firm, and time fixed effects and various characteristics, an increase of one standard deviation of bank's exposure to tariff reductions (i.e. 0.034) decreases the lending of new loans to treated firms compared to control firms by 4.5% . This finding is even more economically significant with the inclusion of bank-firm fixed effects, which capture any persistent time-invariant differences in bank-firm relationships and potentially account for endogeneities in bank-firm relationship (Chakraborty, Goldstein, and MacKinlay, 2018; Santos, 2011). To ensure that this effect is due to lower supply from the exposed banks (bank supply channel) rather than lower demand from treated borrowers (firm demand channel), I use the "within firm comparison" strategy in Khwaja and Mian (2008) and compare the amount of new lending issued by banks with different levels of exposure to the same firm in the same year and quarter. This strategy disentangles the two channels and suggests that the main results is still valid from the bank supply channel alone. A one standard deviation increase in bank's exposure reduces new lending to treated borrowers compared to control borrowers by 2.3% and this bank supply effect is largely driven by more-affected banks.

I also examine how prior lending relationship affects bank new lending to treated borrowers. I construct two relationship measures based on prior lending activities (Ferreira and Matos, 2012; Bharath, Dahiya, Saunders, and Srinivasan, 2007; Schenone, 2004; Dahiya, Saunders, and Srinivasan, 2003; Hombert and Matray, 2017). The first measure is derived from the how many times the firm has borrowed from the bank before new lending. The second measure hinges on if the firm has existing outstanding loans with the bank at the time of new lending. A one standard deviation increase in bank exposure reduces the new lending to first-time treated borrower by 5.2% compared to first-time control borrowers. Interestingly, banks do not significantly lend less new loans to treated borrowers with strong prior relationship (i.e. fourth-time or more borrower) than control borrowers with strong prior relationship. Similarly, there is no statistically significant difference between new lending to treated borrowers with existing loans and control borrowers with existing loans. In other words, banks are more supportive of treated borrowers who have borrowed at least three times from the banks or have existing loans with the banks.

In the next step, I examine if more exposed banks make more effort in screening and monitoring treated borrowers when they issue new loans. If more exposed banks increase screening intensity of treated borrowers compared to control borrowers, then they are able to acquire relatively more information about treated borrowers and differentiate treated borrowers more than less exposed banks. As banks generally exert some effort on monitoring borrowers after lending (Datta, Iskandar-Datta, and Patel, 1999; Wang and Xia, 2014; Cerqueiro, Ongena, and Roszbach, 2016; Ma, Stice, and Williams, 2019), this ability to differentiate loans helps them choose certain borrowers as their main monitoring targets after loan issuance to minimize their monitoring cost and maximize their monitoring potential. Empirically, I find that a one standard deviation increase in bank exposure increases the standard deviation of loan spreads of new loans issued to treated borrowers by 3.84 compared to control borrowers, which is approximately 6% of the mean of the standard deviation of loan spreads.

As the last step my investigation, I proceed to check if tariff reductions have spillover effects on control firms through bank monitoring. Exposed banks have the the incentive to enhance the monitoring of existing large borrowers, who can have detrimental effects on bank liquidity if not repaying debts on time. One important finding of this paper is that control firms more exposed to tariff reductions through bank monitoring increase cash holdings and short-term investments, and reduce R&D expenditure. This result is consistent with the goals of bank monitoring, which are to make large borrowers hold more cash and invest less in risky projects to reduce potential liquidity risks. Thus, banks can act as intermediaries for shock transmission from treated to control borrowers, resulting in real impacts on control borrowers.

This paper is related to several strands of literature. First, the paper provides evidence for the spillover effects through the bank-firm network. While previous papers study how bank-level shocks can be transmitted to borrowers through the bank-firm network (Khwaja and Mian, 2008; Ivashina and Scharfstein, 2010; Santos, 2011; Cingano, Manaresi, and Sette, 2016; Chakraborty, Goldstein, and MacKinlay, 2018), little has been shown about banks as intermediaries for shock transmission from affected to unaffected borrowers. More importantly, the spillover effect causes unaffected borrowers to hold more cash and invest less in R&D. Taken as whole, this spillover effect through bank-firm network has substantial real impacts on unaffected borrowers and economic growth.

Second, this study contributes to the vast literature on the effect of relationship lending on firms' access to credit. Although previous studies shed light on the fact that relationship borrowers get access to more credits from banks at lower interest rates (Boot, 2000; Jiangli, Unal, and Yom, 2008; Degryse, Kim, and Ongena, 2009; Sette and Gobbi, 2015), this paper provides new evidence that relationship borrowers benefit from more credits even if they are affected by tariff reductions and threatened by the entry of capable competitors. This is an important dimension of relationship lending since it suggests the supportive role banks play when relationship firms are under threats. One key difference between this paper and the previous papers lies in the role of banks and firms. In this paper, borrowers can be separated into treated and control borrowers based on industry classification since tariff reduction shocks primarily target at firms rather than banks. When banks directly experience liquidity shocks as in the previous papers, it is hard to identify which firms are more "treated" and draw conclusions about the effect of relationship lending on firms in trouble.

Finally, this paper is related to the literature on bank monitoring. It has been shown that banks play an important role in external monitoring (Shleifer and Vishny, 1997). Bank monitoring helps lower the cost of financing (Datta, Iskandar-Datta, and Patel, 1999; Ma, Stice, and Williams, 2019) and improve corporate governance (Johnson, 1997; Chen, Guo, and Mande, 2006; Shepherd, Tung, and Yoon, 2007; Dass and Massa, 2011) for borrowers. However, other studies show the costs associated with bank monitoring in terms of less firm disclosure (Vashishtha, 2014) and reduced liquidity in borrowers' shares due to information asymmetry (Dass and Massa, 2011). This paper provides further evidence for the cost and externality of bank monitoring as it can affect corporate cash holding and investment policies of firms not directly affected by tariff reductions.

This paper is organized as follows. Section 2 introduces the background knowledge on tariff reductions. Section 3 describes data and testable hypotheses. Section 4 presents regression results. Section 5 addresses some concerns using robustness checks. Finally, Section 6 concludes.

3.2 Background information

As globalization becomes more prominent in the last few decades, import tariffs in the United States have been gradually falling to facilitate international trade. Since import tariffs continue to be a large source of trade costs despite of various trade programs (Anderson and Van Wincoop, 2004; Hoekman and Nicita, 2008), reduced tariffs inevitably lower trade restrictions for foreign firms, hence enable them to enter the U.S. market at lower costs. At the same time, domestic U.S. firms have to cope with the presence of more overseas competitors who can sell goods and services at cheaper prices owing to tariff reductions. Since more fierce product market competition can both increase default risks and lower liquidation value of assets for U.S. manufacturing firms (Valta, 2012), there are higher chances of loan underperformance for firms affected by tariff reductions. Previous studies have shown that banks impose higher interest rates for firms facing higher product market competition (Valta, 2012).

3.3 Empirical design

3.3.1 Data

The data for this paper come from several sources. I use the yearly four-digit SIC-level US import tariff data for manufacturing industries shared by Frésard and Valta (2016).

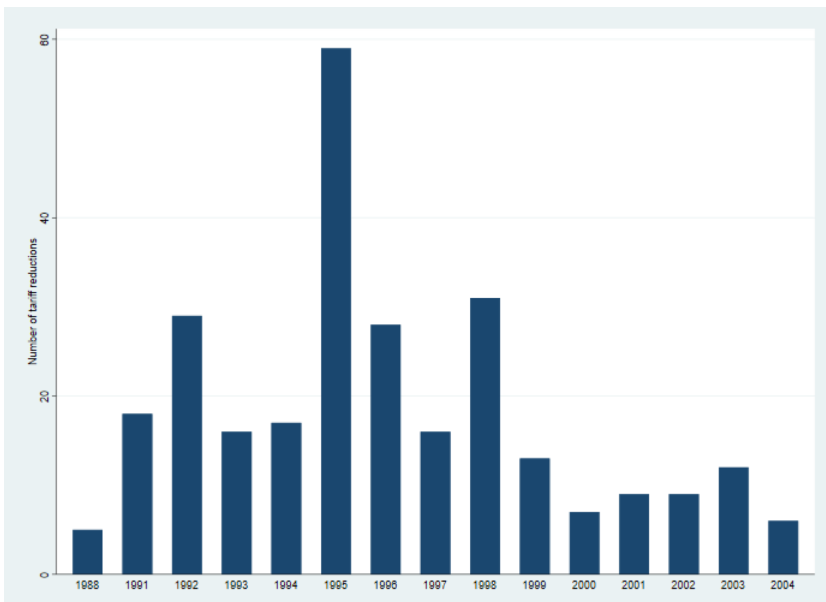
They identify significant import tariff reductions that accentuate the threat of entry of foreign competitors. They measure reductions of import tariffs at the industry level by using product-level import data compiled Feenstra, Romalis, and Schott. The data only includes manufacturing industries with SIC code 2000-3999, so I focus on firms in these industries only in this paper. They convert product import tariffs at Harmonized System (HS) level to four-digit SIC-level and calculate industry-year import tariff as the duties collected by U.S. custom divided by the Free-on-Board value of imports.

I use LPC's Dealscan database to collect loan-specific information. Dealscan provides detailed information on comprehensive loan characteristics of both sole-lender and syndicated loans, such as lender, borrower, date, amount, maturity, spreads, loan purpose, etc. I merge borrowers in the loan-level data with Compustat for firm fundamentals using the link table provided by Chava and Roberts (2008). Similarly, I link banks in the loan-level data with Call Report data using the link table provided by Chakraborty, Goldstein, and MacKinlay (2018). Call Reports are used to gather information on bank holding company (BHC) characteristics as they include quartet-level financial statistics for BHCs.

3.3.2 Sample construction

Due to data availability of tariff reductions and Dealscan loan-level data, the sample in this paper covers 1988-2004. Since the coding of imports changed in 1989, I exclude tariff reductions and loans issued in 1989 and 1990. Following the strategy used in Frésard and Valta (2016), I compute the tariff change between consecutive years for each industry and compare the change with industry-level average change over the entire sample period. Then, I define significant industry-year tariff reductions if the negative change is three times larger than the industry's average. On top of their strategy, I ensure that the tariff reductions are not transitory or fluctuating by excluding those reductions that follow or are followed by large tariff increases in consecutive years. Moreover, since I study both bank new lending or spillover effects on control borrowers through bank monitoring one year after the tariff reductions, I include only shocks that last one year to simplify the identification strategy and the interpretation of regression estimates. Conveniently, I define all firms in the industries that experience significant tariff reductions one year before bank new lending as treated borrowers and other firms with SIC code 2000-3999 as control borrowers. Figure 3.1 shows the distribution of tariff reductions over the sample period. In total, there are 730 significant tariff reductions in 374 out of 495 manufacturing industries (SIC code 2000-3999) from 1988 to 2004.

Figure 3.1: Tariff reductions over the years



This figure shows the number of tariff reductions by year from 1988 to 2004 in manufacturing industries (SIC code 2000-3999).

Then, I compute bank exposure to tariff reduction shocks $Banksexposure_{iy}$ as the proportion of all existing loans of bank i that are issued to treated firms in year y. After matching loan-level data with bank holding company and firm characteristics, there are 33,437 observations left for 9,271 term loans and revolvers newly issued by 452 banks to 2,495 publicly traded firms. Term loans and revolvers are the most common types of corporate loans and contained more detailed loan information so I follow Campello and Gao (2017) to include only these two loan types for the analysis of bank new lending. Since the maturity of loans in the sample are syndicated loans, each observation is uniquely defined by five attributes: borrower, lender, loan, start year, and start quarter. Among the 33,437 observations, 2,087 are for treated firms and 31,350 are for control firms. Table 3.1 shows the summary statistics for bank exposure and some loan characteristics. $log(bankamount)$ is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. $facility\ amount$, $maturity$, and $allindrawn$ denote total loan amount (in millions), loan maturity (in months), and loan spreads (above LIBOR) respectively.

Table 3.1: Summary statistics for bank exposure and loan characteristics

This table reports the summary statistics on the main variables included in the loan-level analysis. The sample is from 1989 to 2005. *Bankexposure* is at an annual frequency and the remaining variables are loan-specific characteristics.

variable	**N**	**mean**	**min**	**p50**	**max**	**sd**
Bankexposure	33437	.0208413	0	.0098913	1	.0343751
Log(bank amount)	33429	2.715166	-13.43427	2.739192	7.600903	1.155663
Facility amount	33437	331.575	.114	150	8500	638.5661
Maturity	33437	55.85055	1	60	252	19.88782
Allindrawn	33437	169.5367	6.8	150	1400	113.0539

Furthermore, I construct another sample for the analysis of spillover effects on control firms. Specifically, using the original data set containing all loan types, I compute a loan-size weighted average of the bank exposure for the banks that the firm has borrowed loans from (see Equation (4)) and I match control borrowers with their annual financial information in Compustat. There are in total 7,896 firm-year level observations for 1,831 control firms. Table 3.2 shows the summary statistics for control firm outcomes one year after tariff reductions. All variables use the year before tariff reductions as the base year as firm outcomes in the base year are not influenced by tariff shocks.

Table 3.2: Summary statistics for control firm outcomes

This table reports the summary statistics on the outcome variables for the control firms. The sample is from 1989 to 2005. All variables at an annual frequency.

variable	N	mean	min	p50	max	sd
Cash holding change	7451	.7828878	-21.10659	.1172938	25.86405	6.661754
Cash holding growth	7389	190.0932	-99.24109	18.11212	4807.75	638.3668
Short-term investment growth	6942	18.98343	-100	0	1337.333	162.6489
R&D growth	4734	24.41131	-80.44787	10.75245	402.1266	69.62647
Sale growth	7552	17.8725	-68.65361	10.46155	236.0602	45.20244
Operating margin growth	7550	-25.14053	-2414.082	-21.41051	2830.928	536.9421

I am aware that tariff reductions are not perfectly random events. First, I argue that it is unlikely for U.S. manufacturing firms to lobby for tariff reductions as they may face tremendous competition from overseas firms. Second, to account for the possibility that some firms can anticipate tariff reductions and respond accordingly, I argue that early-actions of these firms are against finding significant results for new bank loans controlling for firm characteristics and firm fixed effects. The statistically significant results suggest that banks still consider tariff reductions as potential risks to treated firms.

3.3.3 Testable hypotheses

- **Hypothesis 1: Banks more exposed to tariff reduction shocks lend less new loans to treated firms relative to control firms.**

If banks have outstanding loans with firms in the treated industries in the years of tariff reduction, then they are exposed to the shocks to some degree. Since treated firms are more likely to default or postpone loan payment due to higher competition pressure from overseas firms, banks more exposed to the shocks would need to take higher risks of defaulted debt. To mitigate the risk, exposed banks are likely to prefer control firms over treated firms when they are deciding which firm to lend new loans to. Also, banks' exposure should accentuate their preference in issuing new loans to control borrowers rather than treated borrowers.

- **Hypothesis 2: Banks lend support to treated borrowers with strong prior relationship through new loans.**

Bank-firm relationship should play a role in banks' new lending decisions. It has be shown in the previous studies that firms do benefit from closer bank-firm relationship by obtaining more credits at lower costs (Jiangli, Unal, and Yom, 2008; Sette and Gobbi, 2015). The natural question to ask is would bank-firm relationship still benefit firms if they are negatively treated by tariff reduction shock? I hypothesize that when lending new loans, banks would not prefer control borrowers over treated borrowers among strong relationship borrowers.

- **Hypothesis 3: Banks more exposed to tariff reduction shocks increase the screening and monitoring of treated firms more relative to control firms.**

With higher default risks from the treated borrowers, more exposed banks have incentives to acquire more information about treated borrowers before they issue new loans to reduce the probability of loan default. Moreover, enhanced monitoring helps banks select target treated borrowers more precisely after they issue new loans to minimize their monitoring cost and maximize their monitoring potential.

- **Hypothesis 4: Banks more exposed to tariff reduction shocks increase the monitoring of existing large borrowers. Control borrowers more exposed to tariff reductions shocks through bank monitoring increase cash holdings and decrease risky investments.**

Exposed banks have the the incentive to enhance the monitoring of existing large borrowers due to the huge stress on bank liquidity if they are unable to repay debts on time.

Large control borrowers, though not affected by tariff reductions directly, may experience a spillover effect through bank monitoring. Since exposed banks want to ensure large borrowers' capability of repaying loans, large control borrowers may need to hold more cash and invest less in risky projects as a result of bank monitoring.

3.3.4 Methodology

First, I examine the effect of tariff reduction shocks on bank new lending to treated and control borrowers. The basic regression I estimate is

$$\begin{aligned} log(bank\ amount)_{ijlyq} = {} & \beta_1 Bankexposure_{iy-1} + \beta_2 Treatedborrower_{jy-1} \\ & + \beta_3 Bankexposure_{iy-1} \times Treatedborrower_{jy-1} \\ & + Loan\ vars._l + Bank\ vars._{iq-1} + Firm\ vars._{jq-1} \\ & + [fixed\ effects] + \varepsilon_{ijlyq}, \end{aligned} \tag{3.1}$$

where i indexes bank, j indexes firm, l indexes loan, y indexes year, and q indexes quarter (1 to 4). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter as these characteristics may affect bank lending decisions. I also add various sets of fixed effects to check the robustness of the results in Section 4.1.

Under **Hypothesis 1** and **Hypothesis 2**, banks more exposed to tariff reduction shocks lend less new loans to treated firms relative to control firms and this effect disappears if there is a strong prior relationship between banks and treated borrowers. Empirically, β_3 in Equation (1) should be negative and statistically significant in most cases. In the case of strong bank-firm relationship, β_3 should no longer be statistically significant.

Second, I examine the effect of tariff reduction shocks on bank screening and monitoring of treated and control borrowers. Due to data constraint, I am unable to observe the level of bank screening and monitoring directly. However, I am able to test **Hypothesis 3** indirectly by examining the standard deviation of loan spreads. The regression I estimate is

$$sd(loan\ spreads)_{ikyq} = \beta_1 Bankexposure_{iy-1} + \beta_2 Treated_k + \beta_3 Bankexposure_{iy-1} \times Treated_k + Loan\ vars._{ikyq} + Bank\ vars._{iq-1} + [fixed\ effects] + \varepsilon_{ikyq}, \tag{3.2}$$

where i indexes bank, k indexes type, y indexes year, and q indexes quarter. The dependent variable is the standard deviation of loan spreads of new loans made by bank i to borrowers of type k in year y and quarter q. Type k can be either treated borrower or control borrower. $Treated$ is a binary variable that takes the value one if the type of borrowers is treated borrower. Under **Hypothesis 3**, more exposed banks can acquire more information about treated borrowers and differentiate treated borrowers more through loan pricing. Loan spread differentiation can also help exposed banks identify monitoring targets after the issuance of new loans and lower monitoring costs. Therefore, β_3 in Equation (2) is expected to be positive and statistically significant.

Last, I examine the spillover effect of tariff reduction shocks from existing treated borrowers to existing control borrowers through enhanced bank monitoring. The regression

I estimate is

$$\Delta Firm\ outcome_{jy} = \beta_1 Higher\ Firmexposure_{jy-1} + Firm\ vars._{jy-1} + \alpha_j + \theta_y + \varepsilon_{jy}, \quad (3.3)$$

where j indexes existing control borrower and y indexes year. An existing control borrower is defined as a firm that has some existing loans with sample banks and is not treated in years $y-1$ or y. The dependent variable is the change in various firm outcomes from year $y-2$ to year y as tariff reduction shocks in year $y-1$. I choose $y-2$ as the reference year since it is the year not influenced by tariff shocks happened in year $y-1$. To calculate the exposure to tariff reduction shocks for existing control borrowers, I follow Khwaja and Mian (2008) to construct a loan-size weighted average of the bank exposure for the banks that the firm has borrowed loans from. Mathematically, for a control firm j in year t,

$$firmexposure_{j,t} = \sum_i bankexposure_{i,t} \times \frac{existing\ loan_{i,j,t}}{existing\ loan_{i,t}} \quad (3.4)$$

Intuitively, the more exposed the bank is and the larger the control borrower is to the bank, the more enhanced the bank monitoring would be. Thus, a control firm's exposure to tariff reduction shocks is a function of bank exposure for the banks it has existing loans with and how large it is to those banks. Then, I separate control firms each year into halves based on their $firmexposure$. Control firms in the top half take the value one for $Higher\ firmexposure$. As control firms in the bottom half are the omitted group in this regression, the coefficient for $Higher\ firmexposure$ is relative to control firms in the bottom half. Under **Hypothesis 4**, control borrowers more exposed to tariff reductions shocks through bank monitoring increase cash holdings and decrease risky investments. Thus, I expect β_1 to be positive and significant when the dependent variable is change in cash holdings and negative and significant when the dependent variable is change in R&D investment as R&D investment is used as a proxy for firm risk-taking in the previous studies (Bhagat and Welch, 1995; Kothari, Laguerre, and Leone, 2002; Coles, Daniel, and

Naveen, 2006; Kim and Lu, 2011).

3.4 Results

3.4.1 Main results

Firstly, I examine the effects of tariff reduction on bank loans by exploiting the differences in the amount of new loans issued to treated borrowers and control borrowers. Table 3.3 reports results for Equation (1). *Bankexposure* measures the level of bank exposure to tariff reduction shocks in the previous year and *treatedborrower* takes the value one if the borrower firm is in the treated industries in the previous year and zero otherwise. *maturity*, *facilityamt* and *allindrawn* denote loan maturity (in months), loan amount (in millions), and loan spreads (above LIBOR) respectively. *Size*, *profitability*, *tangibility*, *leverage*, and *markettobook* are firm characteristics in the previous quarter. *loanoverasset*, *equityoverasset*, and *incomeoverasset* are bank holding company characteristics in the previous quarter.

On average, banks significantly decrease new lending to treated borrowers compared to control borrowers. In Column (1), the key variable of interest is the interaction between bank exposure to tariff reductions and whether the borrower is treated or not. As expected in **Hypothesis 1**, the estimated coefficient is negative and statistically significant. Controlling for loan, firm, and bank holding company characteristics, an increase of one standard deviation of *bankexposure* (i.e. 0.034) decreases the lending of new loans to treated firms compared to control firms by 4.5% (calculated as (exp(-1.3409*0.034) – 1) * 100). Bank and firm fixed effects are used to control for any time-invariant differences among banks and firms respectively. Year-quarter fixed effects control for differences between time periods. In addition, the coefficient for *bankexposure* itself is positive and statistically significant, meaning that a one standard deviation increase of *bankexposure* increases the lending of new loans to control firms by 2.1%. Interestingly, *treatedborrower* enters positively in the regression. Banks unexposed to the tariff shocks actually increase new lending to the treated borrowers. This can be due to either demand-side or supply-side factors as treated borrowers may change their demand for new loans in face of higher competition from the overseas firms. This result prompts me to conduct with-in firm analysis in Section 4.2 to disentangle the supply-side effects from the demand-side effects.

In Column (3), I use bank-year-quarter and firm fixed effects to control for any confounding shocks at the bank level and time-invariant differences among firms. The point estimates are similar to the results in Column (1). To mitigate the concern that the formation of the bank-firm network is endogenous, I follow Chakraborty, Goldstein, and MacKinlay (2018) and Santos (2011) to add bank-firm fixed effects in Column (4) - Column (5). Bank-firm fixed effects capture any persistent time-invariant differences in bank-firm relationships. The effects on new lending are greater with the bank-firm fixed effects. A one standard deviation increase of *bankexposure* decreases the lending of new loans to treated firms compared to control firms by 7.5% controlling for bank-firm and year-quarter fixed effects in Column (5), which slightly more than doubles the effect in Column (1).

Table 3.3: New bank loans to treated and control borrowers

This table reports the coefficient estimates from the following regression model.

$$
\begin{aligned}
log(bank\ amount)_{ijlyq} = \beta_1 Bankexposure_{iy-1} + \beta_2 Treatedborrower_{jy-1} \\
+ \beta_3 Bankexposure_{iy-1} \times Treatedborrower_{jy-1} \\
+ \text{Loan vars.}_l + Bank\ vars._{iq-1} + Firm\ vars._{jq-1} \\
+ [\text{fixed effects}] + \varepsilon_{ijlyq},
\end{aligned}
$$

where i indexes bank, j indexes firm, l indexes loan, y indexes year, and q indexes quarter (1 to 4). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log(bank loan amount)					
Bankexposure × treatedborrower	-1.4218**	-1.1590*	-1.8139**	-2.2381**	-2.1438**
	(0.6515)	(0.6143)	(0.7565)	(0.9306)	(0.8810)
Bankexposure	0.6485**	0.6513**		1.1151***	0.9736**
	(0.2951)	(0.2969)		(0.3649)	(0.4806)
Treatedborrower	0.2429***	0.2451***	0.2869***	0.2159**	0.2399***
	(0.0576)	(0.0595)	(0.0611)	(0.0943)	(0.0882)
Facilityamt	0.0006***	0.0006***	0.0006***	0.0007***	0.0007***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Maturity	-0.0000	-0.0004	-0.0004	0.0010*	0.0004
	(0.0007)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Allindrawn	-0.0018***	-0.0018***	-0.0019***	-0.0016***	-0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Size	0.1094*	0.0861	0.0392	0.2288***	0.0965*
	(0.0600)	(0.0620)	(0.0710)	(0.0325)	(0.0492)
Profitability	-1.1764**	-1.0183*	-0.9963*	0.2232	-0.1536
	(0.5852)	(0.5636)	(0.5565)	(0.6829)	(0.6365)
Tangibility	-0.9924***	-1.2107***	-1.4535***	-1.1185***	-1.3978***
	(0.2038)	(0.2099)	(0.2166)	(0.3080)	(0.3291)
Leverage	-0.1306	-0.0648	-0.0398	-0.1117	-0.0019
	(0.1235)	(0.1273)	(0.1111)	(0.0978)	(0.1102)
Markettobook	0.0358	0.0382	0.0205	0.0333	0.0162
	(0.0282)	(0.0288)	(0.0276)	(0.0425)	(0.0409)
Loanoverasset	0.0629	0.1211		-0.0130	0.0471
	(0.2352)	(0.2197)		(0.3324)	(0.3343)
Equityoverasset	-1.4578	-0.9506		-3.9122***	-1.0763
	(1.0402)	(1.0070)		(1.4355)	(2.0311)
Incomeoverasset	-4.3259	-1.5717		9.6488***	1.1870
	(2.6570)	(3.3232)		(2.8754)	(6.5678)
Bank FE	Yes	Yes	No	No	No
Year FE	Yes	No	No	No	No
Quarter FE	Yes	No	No	No	No
Year × Quarter FE	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	No	No
Bank × Year × Quarter FE	No	No	Yes	No	No
Bank × Firm FE	No	No	No	Yes	Yes
Observations	12003	12003	11283	8882	8882
R-Squared	0.621	0.627	0.682	0.744	0.751

3.4.2 Bank supply channel

Although Table 3.3 shows that banks more exposed to the tariff reduction shocks issue less amount of new lending to treated borrowers, I have not demonstrated whether this effect is due to lower demand from treated borrowers (firm demand channel) or lower supply from the exposed banks (bank supply channel). Ideally, I would like to show the effect still exists with only the bank supply channel. Thus, I use the "within firm comparison" strategy in Khwaja and Mian (2008) and exploit the fact that firms borrow new loans from multiple banks at the same time. The idea is to compare new lending from banks with different levels of exposure to the same firm in the same year and quarter.

Table 3.4 demonstrates the supply-side evidence by controlling for firm-year-quarter fixed effects in all columns. The variables of interest are *bankexposure* and *bankexposure* × *treatedborrower*. The coefficient for *bankexposure* is statistically insignificant in Column (1) so I cannot draw any conclusion on the bank supply channel using the full sample in Column (1). However, banks more exposed to the tariff reduction shocks decrease their new lending to the treated borrowers, as shown in Column (2). A one standard deviation increase in *bankexposure* reduces new lending to treated borrowers compared to control borrowers by 2.3%. To further understand which banks mainly drive this result, I separate banks with positive *bankexposure* into two groups every year. The less-affected banks are those banks with *bankexposure* below the median cutoff, while the more-affected banks are those banks with *bankexposure* above the median cutoff. Column (5) and Column (6) show that the result in Column (2) is largely driven by more-affected banks, as expected.

Column (3) examines the bank supply channel using the sample of control borrowers. Again, *bankexposure* has no effect on within firm lending to control firms. More importantly, the coefficient for the interaction term in Column (4) is negative and statistically significant. Thus, s one standard deviation increase of *bankexposure* decreases the lending of new loans to treated firms compared to control firms by 2.8% from the bank supply channel alone.

Table 3.4: Bank supply to treated and control borrowers

This table reports the amount of new loans banks supply to the treated and control borrowers. The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount) and bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
					Treated borrowers	
Dependent Variable: log(bank loan amount)	All US borrowers	Treated borrowers	Non-treated borrowers	All US borrowers	less-affected banks	more-affected banks
Bankexposure	-0.0664	-0.6989**	0.0800	0.0778	3.2775	-1.5751**
	(0.2140)	(0.3523)	(0.2336)	(0.2348)	(2.7061)	(0.6095)
Allindrawn	0.0006	-0.0002	0.0007	0.0006	0.0050	-0.0027
	(0.0005)	(0.0027)	(0.0006)	(0.0005)	(0.0038)	(0.0038)
Maturity	0.0041***	-0.0091	0.0046***	0.0041***	-0.0131*	-0.0070
	(0.0011)	(0.0059)	(0.0009)	(0.0011)	(0.0070)	(0.0096)
Facilityamt	0.0017***	0.0035***	0.0016***	0.0017***	0.0038***	0.0038***
	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0004)	(0.0004)
Loanoverasset	-0.2474**	-0.3009	-0.2439**	-0.2461**	-0.4985***	-0.2010
	(0.1070)	(0.2045)	(0.1059)	(0.1065)	(0.1598)	(0.3018)
Equityoverasset	-4.5712***	-5.2780***	-4.5242***	-4.5747***	-0.5672	-7.3286***
	(1.2788)	(1.8033)	(1.2839)	(1.2774)	(2.1038)	(2.4732)
Incomeoverasset	-4.3699	-3.4269	-4.3523	-4.3523	-15.2233	-4.0988
	(3.4830)	(9.1372)	(3.3647)	(3.4789)	(13.4437)	(14.2727)
Bankexposure × treatedborrower				-0.7709**		
				(0.3857)		
Firm × Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17128	1555	15573	17128	584	708
R-Squared	0.768	0.769	0.771	0.768	0.858	0.736

3.4.3 Does prior relationship with banks matter?

Having established that banks exposed to the tariff shocks do cut new lending to treated borrowers, I would like explore more about the role of bank-firm relationship in lending decisions. Would banks support relationship borrowers more in terms of new lending? Past literature has extensively used prior lending activities as a proxy for bank-firm relationships (see Ferreira and Matos, 2012; Bharath, Dahiya, Saunders, and Srinivasan, 2007; Schenone, 2004; Dahiya, Saunders, and Srinivasan, 2003; Hombert and Matray, 2017). Therefore, I construct two relationship measures based on prior lending. The first measure is derived from the how many times the firm has borrowed from the bank before new lending. As such, I separate new lending into three groups: first-time, second or third-time, and fourth-time or more. The second measure hinges on if the firm has existing outstanding loans with the bank at the time of new lending. As such, I separate new lending into two groups: with and without existing loans.

Another factor that may influence banks' new lending to relationship borrowers is loan purpose. A large portion of loans are for corporate restructuring (for example, LBOs, M&A, and stock repurchases) (Ivashina and Scharfstein, 2010), while some other loans are for real investment and debt repayment. To exploit the differences in new lending among various loan purposes, I group loans that are used as working capitals or debt repayment together as these loans are generally more essential for the basic operation and survival of firms.

Borrowing times

Table 3.5 illustrates how the prior borrowing times can affect bank new lending to treated borrowers. Column (1) - Column (3) report the results using all loans in the regression while Column (4) - Column (6) report the results using a sub-sample of loans that are used as working capital or debt repay. The regression specification used in all 6 columns is the same as that in Table 3.3 Column (2), where I control for bank, year-quarter, and firm fixed effects, as well as loan, firm, and bank holding company characteristics. Thus, the variable of interest is still $bankexposure \times treatedborrower$.

Comparing the coefficients for the interaction term across Column (1) to Column (3), the term enters negatively and statistically significantly when firms have borrowed less than three times before from the same bank. For example, a one standard deviation increase in bank exposure reduces the new lending to first-time treated borrower by 5.2% compared to first-time control borrowers. Interestingly, Column (3) shows that banks do not significantly lend less new loans to treated borrowers with strong prior relationship (i.e. fourth-time or more borrower) than control borrowers with strong prior relationship. In other words, banks are more supportive of treated borrowers in the case that treated borrowers have borrowed at least three times from the banks before the new lending. This result is consistent with the prediction of **Hypothesis 2**

I further study the results using a sub-sample of loans based on loan purposes because banks can be even more supportive of treated borrowers with strong prior relationship if the purpose of the new loans are for working capital or debt repayment. Banks still lend significantly less amount of new loans to weak-relationship treated borrowers than weak-relationship control borrowers as shown in Column (4) and (5). However, banks are willing to lend significantly more to treated strong-relationship borrowers if they are lack of working capital or they have difficulty in repaying their debts.

Table 3.5: New bank loans: borrowing times and loan purposes

This table reports the coefficient estimates from the following regression model.

$$\begin{aligned} log(bank\ amount)_{ijlyq} &= \beta_1 Bankexposure_{iy-1} + \beta_2 Treatedborrower_{jy-1} \\ &+ \beta_3 Bankexposure_{iy-1} \times Treatedborrower_{jy-1} \\ &+ \text{Loan vars.}_l + Bank\ vars._{iq-1} + Firm\ vars._{jq-1} \\ &+ \text{[fixed effects]} + \varepsilon_{ijlyq}, \end{aligned}$$

where i indexes bank, j indexes firm, l indexes loan, y indexes year, and q indexes quarter (1 to 4). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
	All loan purposes			Working capital & Debt repay		
Dependent Variable: log(bank loan amount)	First-time	Second-time or Third-time	Fourth-time or more	First-time	Second-time or Third-time	Fourth-time or more
Bankexposure × treatedborrower	-1.5623*	-3.5020**	5.7666	-1.9923*	-4.2168**	10.7599***
	(0.8938)	(1.5723)	(3.6803)	(1.1798)	(1.9213)	(3.9893)
Bankexposure	0.5655*	1.8059**	0.3534	1.0757***	2.3122**	-0.6942
	(0.3057)	(0.8339)	(0.7244)	(0.3430)	(1.0797)	(0.8927)
Treatedborrower	0.2279***	0.4947***	-0.0951	0.1895*	0.2701	-0.1513
	(0.0777)	(0.1522)	(0.2455)	(0.0975)	(0.2154)	(0.3300)
Loan control	Yes	Yes	Yes	Yes	Yes	Yes
Firm control	Yes	Yes	Yes	Yes	Yes	Yes
Bank control	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8898	2574	1178	5943	1756	931
R-Squared	0.661	0.725	0.739	0.694	0.763	0.766

Existence of outstanding loans

Table 3.6 shows that having existing outstanding loans with firms significantly affect banks' new lending decisions. Banks, when making new loans, are likely to take the repayment of existing loans into consideration. To ensure that treated borrowers are able to repay their existing loans in full and on time, banks are likely to support them by issuing new loans so that they can function properly in times of tariff reduction shocks.

Just like in Table 3.5, I analyze the full sample and the sub-sample based on loan purposes separately. Focusing on Column (1) and (2), the coefficient for the interaction term is only negative and statistically significant in Column (1). Thus, compared to control borrowers without existing loans, banks significantly decrease new lending to treated borrowers without existing loans. More importantly, there is no statistically significant difference between new lending to treated borrowers with existing loans and control borrowers with existing loans. These two results provide evidence for the supportive role banks play to relationship borrowers. The results are robust in Column (3) and (4) when I use the sub-sample of loans for the purpose of working capital and debt repayment. The interaction term only enters negatively if the borrowers have no existing loans with the banks.

Table 3.6: New bank loans: existing loans and loan purposes

This table reports the coefficient estimates from the following regression model.

$$\begin{aligned} log(bank\ amount)_{ijlyq} &= \beta_1 Bankexposure_{iy-1} + \beta_2 Treatedborrower_{jy-1} \\ &+ \beta_3 Bankexposure_{iy-1} \times Treatedborrower_{jy-1} \\ &+ \text{Loan vars.}_l + Bank\ vars._{iq-1} + Firm\ vars._{jq-1} \\ &+ [\text{fixed effects}] + \varepsilon_{ijlyq}, \end{aligned}$$

where i indexes bank, j indexes firm, l indexes loan, y indexes year, and q indexes quarter (1 to 4). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)
	All loan purposes		Working capital & Debt repay	
Dependent Variable: log(bank loan amount)	Without existing loans	With existing loans	Without existing loans	With existing loans
Bankexposure × treatedborrower	-1.5018**	-1.2687	-2.2845***	-0.9282
	(0.6858)	(0.8661)	(0.7599)	(0.9792)
Bankexposure	0.6485*	0.5650	1.1807***	1.2582***
	(0.3315)	(0.5159)	(0.3648)	(0.4736)
Treatedborrower	0.2648***	0.2543***	0.2965***	0.3397***
	(0.0710)	(0.0720)	(0.0954)	(0.1211)
Loan control	Yes	Yes	Yes	Yes
Firm control	Yes	Yes	Yes	Yes
Bank control	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Year × Quarter FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	5228	4423	3421	2946
R-Squared	0.721	0.703	0.766	0.745

3.4.4 Does bank increase screening and monitoring?

Since exposed banks take higher default risks from the treated borrowers, **Hypothesis 3** states that more exposed banks make more effort in screening and monitoring treated borrowers when they issue new loans. First, banks usually conduct an information screening of borrowers prior to issuing new loans. The purpose of screening is for banks to acquire private information of borrowers and get more informed. Bank screening intensity is closely related to economic prospects (Ruckes, 2004). If more exposed banks increase screening intensity of treated borrowers compared to control borrowers, then they are able to acquire relatively more information about treated borrowers and differentiate treated borrowers more than less exposed banks. Thus, more exposed banks should have an informational advantage at pricing the new loans issued to treated borrowers. As a result, the higher the bank exposure is, the higher the loan spread differentiation of new loans issued to treated borrowers compared to control borrowers should be.

Second, banks generally exert some effort on monitoring borrowers after lending (Datta, Iskandar-Datta, and Patel, 1999; Wang and Xia, 2014; Cerqueiro, Ongena, and Roszbach, 2016; Ma, Stice, and Williams, 2019). However, monitoring costs can sometimes be so high that banks have to choose certain borrowers as their main targets. As more exposed banks take higher risks of defaulted debt, they have the incentive to select target treated borrowers more precisely so as to minimize their monitoring cost and maximize their monitoring potential. Loan spread differentiation helps banks identify the treated borrowers that are the most worth monitoring.

To check if exposed banks indeed increase loan spread differentiation of new loans made to treated borrowers more than that of new loans made to control borrowers, I follow Equation (2) and regress the standard deviation of loan spreads on $bankexposure \times treatedborrower$ and other controls, controlling for various sets of fixed effects. I first collapse the loan-year-quarter level data set into bank-type-year-quarter, with *type* taking the value one for treated borrowers and zero for control borrowers. *Loannumber* denotes the number of new loans issued to borrowers in each type in each year-quarter. (sd)*maturity* and $(sd)facilityamt$ are the standard deviation of loan maturity (in months) and loan amount (in millions) new loans issued to borrowers in each type in each year-quarter, respectively. *loanoverasset*. *equityoverasset*, and *incomeoverasset* are bank holding company characteristics in the previous quarter.

Table 3.7 demonstrates the effect of tariff reduction shocks on loan spreads differentiation. In all the columns, the coefficients for *bankexposure* $\times$ *treatedborrower* is positive and statistically significant, which is consistent with **Hypothesis 3**. More exposed banks increase loan spread differentiation of new loans made to treated borrowers compare to control borrowers to a greater extent than less exposed banks. To quantify the effect, I focus on Column (1) with bank, year-quarter, and type fixed effects. A one standard deviation increase in bank exposure increases the standard deviation of loan spreads of new loans issued to treated borrowers compared to control borrowers by 3.84, which is approximately 6% of the mean of $sd(loan\ spread)$. Unsurprisingly, bank exposure does not affect the standard deviation of loan spreads of new loans issued to control borrowers, as can be seen in the insignificant coefficients for *bankexposure* in Column (1) and (2).

Column (2) - Column (4) add on more strict fixed effects to the basic results in Column (1). For example, bank-year-quarter fixed effect and bank-type fixed effect control for any confounding shocks at the bank level and at the bank-type level. The effect of tariff reduction shocks on loan spread differentiation is robust across all four specifications, which provides indirect evidence for higher screening and monitoring intensity of exposed banks towards treated borrowers.

Table 3.7: New bank loan spreads and bank screening

This table reports the coefficient estimates from the following regression model.

$$sd(loan\ spread)_{ikyq} = \beta_1 Bankexposure_{iy-1} + \beta_2 Treated_k + \beta_3 Bankexposure_{iy-1} \times Treated_k$$
$$+ Loan\ vars._{ikyq} + Bank\ vars._{iq-1} + [fixed\ effects] + \varepsilon_{ikyq},$$

where i indexes bank, k indexes type, y indexes year, and q indexes quarter. The dependent variable is the standard deviation of loan spreads of new loans made by bank i to borrowers of type k in year y and quarter q. Type k can be either treated borrower or control borrower. $Treated$ is a binary variable that takes the value one if the type of borrowers is treated borrower. I control for number of loans issued, standard deviation of loan characteristics, and bank holding company characteristics in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)
Dependent Variable: sd(loan spread)				
Bankexposure × treated	113.1990***	166.9492***	132.2257**	246.9670***
	(43.2879)	(57.8005)	(62.1195)	(87.3111)
Bankexposure	1.7033	-0.0977		
	(26.4730)	(26.9811)		
Loannumber	0.7362***	0.9003***	-0.0983	0.2647
	(0.2127)	(0.1922)	(0.2138)	(0.4175)
Sd(maturity)	0.9613***	0.9361***	0.9460***	
	(0.1028)	(0.1037)	(0.2752)	
Sd(facilityamt)	0.0000***	0.0000***	0.0000**	
	(0.0000)	(0.0000)	(0.0000)	
Loanoverasset	1.0536	4.8128		
	(11.2415)	(11.6664)		
Equityoverasset	34.4004	9.3089		
	(93.5340)	(96.7963)		
Incomeoverasset	-103.7590	-110.8174		
	(296.1075)	(300.3701)		
Bank FE	Yes	No	No	No
Year × Quarter FE	Yes	Yes	No	No
Type FE	Yes	No	Yes	No
Bank × Year × Quarter FE	No	No	Yes	Yes
Bank × Type FE	No	Yes	No	Yes
Observations	3997	3946	874	812
R-Squared	0.390	0.400	0.697	0.715

3.4.5 Spillover effects on existing control firms

In addition to study banks' respond to tariff reduction shocks through newly issued loans, I further investigate if there is a spillover effect from existing treated borrowers to control borrowers through the bank lending network. Facing higher risks of debt default from existing treated borrowers, banks have the the incentive to enhance their monitoring of existing large borrowers. More specifically, large borrowers for one bank refer to firms that have borrowed large amounts of loans from this bank. Large borrowers not repaying debts on time would pose tremendous liquidity risks for exposed banks, in addition to the debt default risks from existing treated borrowers. Enhanced bank monitoring of existing borrowers may have some real effects on corporate policies. Previous literature has shown that better bank monitoring can improve corporate governance (Dass and Massa, 2011) and reduce disclosure (Vashishtha, 2014). Since, in this context, banks are more concerned about large borrowers' capability of repaying debts on time, I primarily study the spillover effect of tariff reduction shocks on the cash holding and risk-taking behaviors of control borrowers.

As explained in Section 3.4, I follow Khwaja and Mian (2008) to construct a loan-size weighted average of the bank exposure for the banks that the firm has borrowed loans from. Also, I separate control firms each year into halves based on their $firmexposure$. Control firms in the top half take the value one for $Higher\ firmexposure$. As control firms in the bottom half are the omitted group in this regression, the coefficient for $Higher\ firmexposure$ is relative to control firms in the bottom half.

I control for firm characteristics in the previous year as well as firm and year fixed effects in all columns in Table 3.8. Column (1) shows that control firms that are more exposed to tariff reduction shocks increase corporate cash holdings (normalized by total assets) relative to other control firms. On average, control firms in the top half increase cash holdings from one year before to one year after tariff shocks by about 0.6 percentage points compared to control firms in the bottom half. Follow the the suggestion by Dessaint and Matray (2017), I also calculate the firm-year level growth rate of the amount of cash holdings from one year before to one year after tariff shocks because total assets might change simultaneously. Column (2) reports the result using cash growth as the dependent variable. Consistent with the result in Column (1), the cash holding growth of control firms in the top half is 39.4 percentage points on average higher than that of control firms in the bottom half.

Next, I investigate the sources for the cash increase of control firms in the top half. The extra cash holdings can come from a decrease in short-term investment ($ST\ investment$) or R&D investment ($R\&D$), or an increase in revenue ($Sale\ growth$) or operating profits ($Operating\ margin$). Again, I use percentage growth of these items as dependent variables in Column (3) - Column (6) instead of scaling them by total assets since total assets include the amount of cash holdings. Compared to control firms in the bottom half, control firms in the top half increase short-term investment, decrease R&D investment, and decrease sale to a greater extent one year after tariff reduction shocks. These are some evidence suggesting that the cash increase of control firms in the top half should partly come from the lower level of R&D investment. Moreover, the fact that control firms in the top half tend to have greater growth of short-term investment and slower growth of R&D investment is consistent with banks' motive to ensure large borrowers to lower investment risks and hold more cash on hand, which is consistent with **Hypothesis 4**.

Table 3.8: Spillover effects on control firms

This table reports the coefficient estimates from the following regression model.

$$\Delta Firm\ outcome_{jy} = \beta_1 Higher\ Firmexposure_{jy-1} + Firm\ vars._{jy-1} + \alpha_j + \theta_y + \varepsilon_{jy},$$

where j indexes existing control borrower and y indexes year. An existing control borrower is defined as a firm that has some existing loans with sample banks and is not treated in years $y-1$ or y. The dependent variable is the change in various firm outcomes from year $y-2$ to year y. I choose $y-2$ as the reference year since it is the year not influenced by tariff shocks happened in year $y-1$. For a control firm j in year t,

$$firmexposure_{j,t} = \sum_i bankexposure_{i,t} \times \frac{existing\ loan_{i,j,t}}{existing\ loan_{i,t}}$$

The standard errors are clustered by firm. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
			Source of cash			
Dependent Variable	Δ Cash (% total assets)	Cash growth (%)	ST investment growth (%)	R&D growth (%)	Sale growth (%)	Operating margin growth (%)
Higher firmexposure	0.577***	39.377*	10.329**	-6.191**	-7.618***	-4.156
	(0.216)	(20.934)	(5.050)	(2.804)	(1.415)	(16.888)
Size	-0.695**	-142.030***	-21.739**	-0.218	3.903	-36.054
	(0.306)	(36.495)	(9.356)	(4.550)	(2.742)	(24.789)
Profitability	-1.348	97.333	8.030	90.233***	49.510***	173.250
	(2.376)	(137.259)	(49.431)	(23.697)	(10.794)	(115.415)
Tangibility	4.186*	-61.474	-51.780	-73.905***	-75.411***	31.028
	(2.159)	(190.297)	(49.643)	(26.332)	(13.751)	(148.439)
Leverage	1.776**	41.525	-9.707	-26.056**	-1.399	12.857
	(0.813)	(69.518)	(18.373)	(11.371)	(6.053)	(73.060)
Markettobook	0.126	34.618**	2.273	8.225***	7.381***	-11.501
	(0.220)	(13.641)	(4.832)	(1.209)	(1.027)	(14.681)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6515	6462	6014	4199	6616	6614
R-Squared	0.251	0.258	0.356	0.401	0.456	0.238

3.5 Robustness

3.5.1 Control firms that are treated in the same year as new loans

In the main analysis for new loans, I define control borrowers as firms in the industries that do not experience significant tariff reductions one year before new loans. However there are a small number of control borrowers that experience tariff reductions in the same year as new loans. I choose not to exclude them from the group of control borrowers in the main analysis due to two reasons.

Table 3.9: New bank loans to treated and control borrowers (excluding control firms treated in the loan year)

This table reports the coefficient estimates from the following regression model.

$$log(bank\ amount)_{ijlyq} = \beta_1 Bankexposure_{iy-1} + \beta_2 Treatedborrower_{jy-1} + \beta_3 Bankexposure_{iy-1} \times Treatedborrower_{jy-1} + Loan\ vars._l + Bank\ vars._{iq-1} + Firm\ vars._{jq-1} + [fixed\ effects] + \varepsilon_{ijlyq},$$

where i indexes bank, j indexes firm, l indexes loan, y indexes year, and q indexes quarter (1 to 4). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log(bank loan amount)					
Bankexposure × treatedborrower	-1.4218**	-1.1590*	-1.8139**	-2.2381**	-2.1438**
	(0.6515)	(0.6143)	(0.7565)	(0.9306)	(0.8810)
Bankexposure	0.6485**	0.6513**		1.1151***	0.9736**
	(0.2951)	(0.2969)		(0.3649)	(0.4806)
Treatedborrower	0.2429***	0.2451***	0.2869***	0.2159**	0.2399***
	(0.0576)	(0.0595)	(0.0611)	(0.0943)	(0.0882)
Facilityamt	0.0006***	0.0006***	0.0006***	0.0007***	0.0007***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Maturity	-0.0000	-0.0004	-0.0004	0.0010*	0.0004
	(0.0007)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Allindrawn	-0.0018***	-0.0018***	-0.0019***	-0.0016***	-0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Size	0.1094*	0.0861	0.0392	0.2288***	0.0965*
	(0.0600)	(0.0620)	(0.0710)	(0.0325)	(0.0492)
Profitability	-1.1764**	-1.0183*	-0.9963*	0.2232	-0.1536
	(0.5852)	(0.5636)	(0.5565)	(0.6829)	(0.6365)
Tangibility	-0.9924***	-1.2107***	-1.4535***	-1.1185***	-1.3978***
	(0.2038)	(0.2099)	(0.2166)	(0.3080)	(0.3291)
Leverage	-0.1306	-0.0648	-0.0398	-0.1117	-0.0019
	(0.1235)	(0.1273)	(0.1111)	(0.0978)	(0.1102)
Markettobook	0.0358	0.0382	0.0205	0.0333	0.0162
	(0.0282)	(0.0288)	(0.0276)	(0.0425)	(0.0409)
Loanoverasset	0.0629	0.1211		-0.0130	0.0471
	(0.2352)	(0.2197)		(0.3324)	(0.3343)
Equityoverasset	-1.4578	-0.9506		-3.9122***	-1.0763
	(1.0402)	(1.0070)		(1.4355)	(2.0311)
Incomeoverasset	-4.3259	-1.5717		9.6488***	1.1870
	(2.6570)	(3.3232)		(2.8754)	(6.5678)
Bank FE	Yes	Yes	No	No	No
Year FE	Yes	No	No	No	No
Quarter FE	Yes	No	No	No	No
Year × Quarter FE	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	No	No
Bank × Year × Quarter FE	No	No	Yes	No	No
Bank × Firm FE	No	No	No	Yes	Yes
Observations	12003	12003	11283	8882	8882
R-Squared	0.621	0.627	0.682	0.744	0.751

First, the possibility that banks lend less new loans to these borrowers should bias against me finding significant results in the main analysis. Second, it may take some time for the tariff reductions to be effective in the same year as new loans so new loans issued in the early months should be minimally affected. To mitigate the concern about these firms, I exclude them from the group of control firms and re-run the regressions in Table 3.3 and Table 3.4. The results are shown in Table 3.9 and Table 3.10 respectively. Compared to the main results, the coefficients of the variables of interest do not change much after removing these "controversial" firms from control borrowers. The main results are robust to this change.

Table 3.10: Bank supply to treated and control borrowers (excluding control firms treated in the loan year)

This table reports the amount of new loans banks supply to the treated and control borrowers. The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount) and bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
					Treated borrowers	
Dependent Variable: log(bank loan amount)	All US borrowers	Treated borrowers	Non-treated borrowers	All US borrowers	less-affected banks	more-affected banks
Bankexposure	-0.0664	-0.6989**	0.0800	0.0778	3.2775	-1.5751**
	(0.2140)	(0.3523)	(0.2336)	(0.2348)	(2.7061)	(0.6095)
Allindrawn	0.0006	-0.0002	0.0007	0.0006	0.0050	-0.0027
	(0.0005)	(0.0027)	(0.0006)	(0.0005)	(0.0038)	(0.0038)
Maturity	0.0041***	-0.0091	0.0046***	0.0041***	-0.0131*	-0.0070
	(0.0011)	(0.0059)	(0.0009)	(0.0011)	(0.0070)	(0.0096)
Facilityamt	0.0017***	0.0035***	0.0016***	0.0017***	0.0038***	0.0038***
	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0004)	(0.0004)
Loanoverasset	-0.2474**	-0.3009	-0.2439**	-0.2461**	-0.4985***	-0.2010
	(0.1070)	(0.2045)	(0.1059)	(0.1065)	(0.1598)	(0.3018)
Equityoverasset	-4.5712***	-5.2780***	-4.5242***	-4.5747***	-0.5672	-7.3286***
	(1.2788)	(1.8033)	(1.2839)	(1.2774)	(2.1038)	(2.4732)
Incomeoverasset	-4.3699	-3.4269	-4.3523	-4.3523	-15.2233	-4.0988
	(3.4830)	(9.1372)	(3.3647)	(3.4789)	(13.4437)	(14.2727)
Bankexposure × treatedborrower				-0.7709**		
				(0.3857)		
Firm × Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17128	1555	15573	17128	584	708
R-Squared	0.768	0.769	0.771	0.768	0.858	0.736

3.5.2 Industry clustering

I conduct a further robustness check to replace bank clustering with industry clustering. The reason is that tariff reductions are at industry-level, so it is important to show the results with industry clustering. I re-run the regressions in Table 3.3 and Table 3.4 with industry clustering and present the results in Table 3.11 and Table 3.12. The estimated coefficients are very similar in magnitude to those in the main results, with the coefficients in Table 3.12 more statistically significant than coefficients in Table 3.4. Thus, the main results are robust to industry clustering but I include bank clustering in the main results as it is commonly used in the previous literature.

Table 3.11: New bank loans to treated and control borrowers (industry clustering)

This table reports the coefficient estimates from the following regression model.

$$
\begin{aligned}
log(bank\ amount)_{ijlyq} &= \beta_1 Bankexposure_{iy-1} + \beta_2 Treatedborrower_{jy-1} \\
&+ \beta_3 Bankexposure_{iy-1} \times Treatedborrower_{jy-1} \\
&+ \text{Loan vars.}_l + Bank\ vars._{iq-1} + Firm\ vars._{jq-1} \\
&+ \text{[fixed effects]} + \varepsilon_{ijlyq},
\end{aligned}
$$

where i indexes bank, j indexes firm, l indexes loan, y indexes year, and q indexes quarter (1 to 4). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter. The standard errors are clustered by firm industry. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log(bank loan amount)					
Bankexposure × treatedborrower	-1.4218**	-1.1590*	-1.8139**	-2.2381**	-2.1438**
	(0.6515)	(0.6143)	(0.7565)	(0.9306)	(0.8810)
Bankexposure	0.6485**	0.6513**		1.1151***	0.9736**
	(0.2951)	(0.2969)		(0.3649)	(0.4806)
Treatedborrower	0.2429***	0.2451***	0.2869***	0.2159**	0.2399***
	(0.0576)	(0.0595)	(0.0611)	(0.0943)	(0.0882)
Facilityamt	0.0006***	0.0006***	0.0006***	0.0007***	0.0007***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Maturity	-0.0000	-0.0004	-0.0004	0.0010*	0.0004
	(0.0007)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Allindrawn	-0.0018***	-0.0018***	-0.0019***	-0.0016***	-0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Size	0.1094*	0.0861	0.0392	0.2288***	0.0965*
	(0.0600)	(0.0620)	(0.0710)	(0.0325)	(0.0492)
Profitability	-1.1764**	-1.0183*	-0.9963*	0.2232	-0.1536
	(0.5852)	(0.5636)	(0.5565)	(0.6829)	(0.6365)
Tangibility	-0.9924***	-1.2107***	-1.4535***	-1.1185***	-1.3978***
	(0.2038)	(0.2099)	(0.2166)	(0.3080)	(0.3291)
Leverage	-0.1306	-0.0648	-0.0398	-0.1117	-0.0019
	(0.1235)	(0.1273)	(0.1111)	(0.0978)	(0.1102)
Markettobook	0.0358	0.0382	0.0205	0.0333	0.0162
	(0.0282)	(0.0288)	(0.0276)	(0.0425)	(0.0409)
Loanoverasset	0.0629	0.1211		-0.0130	0.0471
	(0.2352)	(0.2197)		(0.3324)	(0.3343)
Equityoverasset	-1.4578	-0.9506		-3.9122***	-1.0763
	(1.0402)	(1.0070)		(1.4355)	(2.0311)
Incomeoverasset	-4.3259	-1.5717		9.6488***	1.1870
	(2.6570)	(3.3232)		(2.8754)	(6.5678)
Bank FE	Yes	Yes	No	No	No
Year FE	Yes	No	No	No	No
Quarter FE	Yes	No	No	No	No
Year × Quarter FE	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	No	No
Bank × Year × Quarter FE	No	No	Yes	No	No
Bank × Firm FE	No	No	No	Yes	Yes
Observations	12003	12003	11283	8882	8882
R-Squared	0.621	0.627	0.682	0.744	0.751

Table 3.12: Bank supply to treated and control borrowers (industry clustering)

This table reports the amount of new loans banks supply to the treated and control borrowers. The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount) and bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) in the previous quarter. The standard errors are clustered by firm industry. The significance of the coefficient estimate is indicated by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)	(6)
					Treated borrowers	
Dependent Variable: log(bank loan amount)	All US borrowers	Treated borrowers	Non-treated borrowers	All US borrowers	less-affected banks	more-affected banks
Bankexposure	-0.0664	-0.6989**	0.0800	0.0778	3.2775	-1.5751**
	(0.2140)	(0.3523)	(0.2336)	(0.2348)	(2.7061)	(0.6095)
Allindrawn	0.0006	-0.0002	0.0007	0.0006	0.0050	-0.0027
	(0.0005)	(0.0027)	(0.0006)	(0.0005)	(0.0038)	(0.0038)
Maturity	0.0041***	-0.0091	0.0046***	0.0041***	-0.0131*	-0.0070
	(0.0011)	(0.0059)	(0.0009)	(0.0011)	(0.0070)	(0.0096)
Facilityamt	0.0017***	0.0035***	0.0016***	0.0017***	0.0038***	0.0038***
	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0004)	(0.0004)
Loanoverasset	-0.2474**	-0.3009	-0.2439**	-0.2461**	-0.4985***	-0.2010
	(0.1070)	(0.2045)	(0.1059)	(0.1065)	(0.1598)	(0.3018)
Equityoverasset	-4.5712***	-5.2780***	-4.5242***	-4.5747***	-0.5672	-7.3286***
	(1.2788)	(1.8033)	(1.2839)	(1.2774)	(2.1038)	(2.4732)
Incomeoverasset	-4.3699	-3.4269	-4.3523	-4.3523	-15.2233	-4.0988
	(3.4830)	(9.1372)	(3.3647)	(3.4789)	(13.4437)	(14.2727)
Bankexposure × treatedborrower				-0.7709**		
				(0.3857)		
Firm × Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17128	1555	15573	17128	584	708
R-Squared	0.768	0.769	0.771	0.768	0.858	0.736

3.5.3 Excluding FTA years

Another reasonable concern is that the difference between banks' new lending to treated and borrowers is caused by higher lending to control borrowers instead of lower lending to treated borrowers. If there a sudden tariff reduction in other countries which benefits some industries relatively more than other industries, then it is definitely possible that the interaction term $bankexposure \times treatedborrower$ in Table 3.3 captures the increase in new lending to control borrowers. Following this logic, U.S. signing Free Trade Agreements (FTAs) with other countries and China joining WTO are milestone events that may cause increased new lending to control borrowers as they can export with lower tariff rates. Therefore, I remove the years in which these events took place and re-run the regression in Table 3.3 and Table 3.4.

Within the time frame of the data sample, U.S. signed FTAs with six countries/regions: Canada (September 1988), NAFTA (December 1992), Jordan (September 2001), Singapore (May 2003), Chile (June 2003), Australia (May 2004). Thus, I removed new loans issuance in 1988, 1993, 2001, 2003, and 2004 from the full sample as a response to these trade agreements and China joining the WTO in 2001. Table 3.13 and Table 3.14 show that the main results are robust to excluding new loans in FTA years. In fact, this change seems not affect largely either the magnitude or the significance level of the coefficients of interest.

Table 3.13: New bank loans to treated and control borrowers (excluding FTA years)

This table reports the coefficient estimates from the following regression model.

$$\begin{aligned} log(bank\ amount)_{ijlyq} &= \beta_1 Bankexposure_{iy-1} + \beta_2 Treatedborrower_{jy-1} \\ &+ \beta_3 Bankexposure_{iy-1} \times Treatedborrower_{jy-1} \\ &+ \text{Loan vars.}_l + Bank\ vars._{iq-1} + Firm\ vars._{jq-1} \\ &+ \text{[fixed effects]} + \varepsilon_{ijlyq}, \end{aligned}$$

where i indexes bank, j indexes firm, l indexes loan, y indexes year, and q indexes quarter (1 to 4). The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount), bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) and firm characteristics (size, profitability, tangibility, leverage, market-to-book ratio) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log(bank loan amount)					
Bankexposure × treatedborrower	-1.4218**	-1.1590*	-1.8139**	-2.2381**	-2.1438**
	(0.6515)	(0.6143)	(0.7565)	(0.9306)	(0.8810)
Bankexposure	0.6485**	0.6513**		1.1151***	0.9736**
	(0.2951)	(0.2969)		(0.3649)	(0.4806)
Treatedborrower	0.2429***	0.2451***	0.2869***	0.2159**	0.2399***
	(0.0576)	(0.0595)	(0.0611)	(0.0943)	(0.0882)
Facilityamt	0.0006***	0.0006***	0.0006***	0.0007***	0.0007***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Maturity	-0.0000	-0.0004	-0.0004	0.0010*	0.0004
	(0.0007)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Allindrawn	-0.0018***	-0.0018***	-0.0019***	-0.0016***	-0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Size	0.1094*	0.0861	0.0392	0.2288***	0.0965*
	(0.0600)	(0.0620)	(0.0710)	(0.0325)	(0.0492)
Profitability	-1.1764**	-1.0183*	-0.9963*	0.2232	-0.1536
	(0.5852)	(0.5636)	(0.5565)	(0.6829)	(0.6365)
Tangibility	-0.9924***	-1.2107***	-1.4535***	-1.1185***	-1.3978***
	(0.2038)	(0.2099)	(0.2166)	(0.3080)	(0.3291)
Leverage	-0.1306	-0.0648	-0.0398	-0.1117	-0.0019
	(0.1235)	(0.1273)	(0.1111)	(0.0978)	(0.1102)
Markettobook	0.0358	0.0382	0.0205	0.0333	0.0162
	(0.0282)	(0.0288)	(0.0276)	(0.0425)	(0.0409)
Loanoverasset	0.0629	0.1211		-0.0130	0.0471
	(0.2352)	(0.2197)		(0.3324)	(0.3343)
Equityoverasset	-1.4578	-0.9506		-3.9122***	-1.0763
	(1.0402)	(1.0070)		(1.4355)	(2.0311)
Incomeoverasset	-4.3259	-1.5717		9.6488***	1.1870
	(2.6570)	(3.3232)		(2.8754)	(6.5678)
Bank FE	Yes	Yes	No	No	No
Year FE	Yes	No	No	No	No
Quarter FE	Yes	No	No	No	No
Year × Quarter FE	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	No	No
Bank × Year × Quarter FE	No	No	Yes	No	No
Bank × Firm FE	No	No	No	Yes	Yes
Observations	12003	12003	11283	8882	8882
R-Squared	0.621	0.627	0.682	0.744	0.751

Table 3.14: Bank supply to treated and control borrowers (excluding FTA years)

This table reports the amount of new loans banks supply to the treated and control borrowers. The dependent variable is the logarithm of bank lending amount (in millions) in all sole-lender and syndicated loans. Suppose there are tariff reduction shocks for certain industries (treated industries) in year $y-1$, then $Bankexposure_{iy-1}$ is calculated as the proportion of all existing loans of bank i that lent to firms in the treated industries in $y-1$ and $Treatedborrower$ is a binary variable that takes the value one if the receiver j of a new loan made in year y is in the treated industries in year $y-1$. I control for loan characteristics (allindrawn, maturity, facilityamount) and bank holding company characteristics (loan-over-asset, equity-over-asset, income-over-asset) in the previous quarter. The standard errors are clustered by bank. The significance of the coefficient estimate is indicated by * for p < 0.10, ** for p < 0.05, and *** for p < 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
					Treated borrowers	
Dependent Variable: log(bank loan amount)	All US borrowers	Treated borrowers	Non-treated borrowers	All US borrowers	less-affected banks	more-affected banks
Bankexposure	-0.0664	-0.6989**	0.0800	0.0778	3.2775	-1.5751**
	(0.2140)	(0.3523)	(0.2336)	(0.2348)	(2.7061)	(0.6095)
Allindrawn	0.0006	-0.0002	0.0007	0.0006	0.0050	-0.0027
	(0.0005)	(0.0027)	(0.0006)	(0.0005)	(0.0038)	(0.0038)
Maturity	0.0041***	-0.0091	0.0046***	0.0041***	-0.0131*	-0.0070
	(0.0011)	(0.0059)	(0.0009)	(0.0011)	(0.0070)	(0.0096)
Facilityamt	0.0017***	0.0035***	0.0016***	0.0017***	0.0038***	0.0038***
	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0004)	(0.0004)
Loanoverasset	-0.2474**	-0.3009	-0.2439**	-0.2461**	-0.4985***	-0.2010
	(0.1070)	(0.2045)	(0.1059)	(0.1065)	(0.1598)	(0.3018)
Equityoverasset	-4.5712***	-5.2780***	-4.5242***	-4.5747***	-0.5672	-7.3286***
	(1.2788)	(1.8033)	(1.2839)	(1.2774)	(2.1038)	(2.4732)
Incomeoverasset	-4.3699	-3.4269	-4.3523	-4.3523	-15.2233	-4.0988
	(3.4830)	(9.1372)	(3.3647)	(3.4789)	(13.4437)	(14.2727)
Bankexposure × treatedborrower				-0.7709**		
				(0.3857)		
Firm × Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17128	1555	15573	17128	584	708
R-Squared	0.768	0.769	0.771	0.768	0.858	0.736

3.6 Conclusion

In this study I provide new evidence on the consequences of tariff reductions on banking activities and spillover effects through the bank-firm network. I exploit the variation in the proportion of existing loans lent to firms in the treated industries across banks for identification. One year after tariff reductions, high-exposure banks decrease new lending to treated borrowers compared to control borrowers to a greater extent than low-exposure banks. The heterogeneous effect on new lending to treated and control borrowers disappears when there is strong prior relationship between banks and treated borrowers. Interestingly, I also discover that there is a spillover effect on control firms through bank monitoring. Control firms more exposed to tariff reductions through bank monitoring increase cash holdings and short-term investments and reduce R&D expenditure compared to other control firms. This result is consistent with banks' motive to make large borrowers hold more cash and invest less in risky projects to reduce potential liquidity risks.

These findings are also relevant for bank regulatory policy debates. Ever since the global financial crisis, policy makers have been trying to promote regulatory policies for banks to reduce subprime lending and hold sufficient levels of reserves. This paper makes such policies even more relevant to stabilizing the financial sector as negative shocks on treated borrowers can be transmitted to control borrowers and influence cash holding and investment strategies for control borrowers through bank monitoring. Therefore, such regulatory policies are not only necessary during large scale financial crises but also beneficial when only some industries are experiencing hardship.

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