ECONOMIC SHOCKS AND FINANCIAL VULNERABILITY

A Dissertation
Submitted to
the Temple University Graduate Board

In Partial Fulfillment
of the Requirements for the Degree
DOCTOR OF PHILOSOPHY

---

by
Xuesong You
December 2021

Examining Committee Members:
Dr. Benjamin Collier, Advisory Chair, Department of Risk, Insurance, and Healthcare Management
Dr. Martin Grace, Department of Risk, Insurance, and Healthcare Management
Dr. Tianxiang Shi, Department of Risk, Insurance, and Healthcare Management
Dr. Rhiannon Jerch, Department of Economics
Dr. Connie X. Mao, External Member, Department of Finance

ABSTRACT

This three-chapter dissertation explores firms' responses to financial shocks in three settings: severe climate events, market crash, and unexpected loss shocks.

The first chapter examines how severe climate events affect small business financing outcomes and how they use credit to finance losses, using Hurricane Harvey as my setting. By using the credit reports data of 8,219 small businesses in the Harvey affected area, I estimate a treatment intensity difference-in-differences model where flooding at a firm's location is the measure of treatment. I find that Harvey-related flooding increased credit delinquencies, especially short-term delinquencies approximately one year after Hurricane Harvey. Delinquencies also increased among firms in the disaster area whose properties were not flooded, suggesting spillover effects from flooded areas. I also find that firms without existing debt took on debt following Harvey. Firms with existing debt lowered loan balances while applying for new credit.

The second chapter considers the funding challenges facing multiemployer defined benefit pension plans. I first explore whether the current funding rules have unintended consequences – triggering employer withdrawals. The Pension Protection Act of 2006 requires that a multiemployer pension plan with an actuarial funded percentage of less than 80% must take corrective actions to improve financial health. In this paper, a regression discontinuity design is used to establish the causal effect of funding rule requirements on employer withdrawals from multiemployer pension plans. I find that multiemployer pension plans subject to funding rule requirements are about 14 percentage points more likely to experience employer withdrawals. Next, I investigate whether employer withdrawals exacerbate the funding challenges of multiemployer pension plans. Using an event study methodology, I find that plans with ex ante employer withdrawal experiences are more vulnerable to financial shocks such as the 2008 financial crisis. This study provides important policy implications for regulators concerning best practices to build pension plan resilience to insolvency events.

The third chapter investigates how rivals' loss shocks affect a firm's pricing decisions. I develop a simple theoretical model and predict that a firm's relative financial position matters. Unaffected firms may benefit from rivals' loss shocks by charging a higher price. I empirically examine the relationship in the setting of the U.S. property/casualty insurance industry. I find that insurers who only write personal lines outperform their adversely affected rivals and charge a higher price following rivals' commercial-line loss shocks. The competitive effects of loss shocks are more pronounced in states where rate regulation is not stringent.

To my family Huilian, Meng, Yunyan, and Joe.

ACKNOWLEDGMENTS

I would first like to express my deepest gratitude toward my committee chair, Ben Collier. His continuous support and mentoring guided me through my journey as a Ph.D. student over the past five years. It has truly been my greatest learning experience: he has always had my best interest at heart, challenged me to think, and always delivered feedback in a constructive and empathetic way. Also, I would like to thank the rest of my committee members and external reader, Tianxiang Shi, Martin Grace, Rhiannon Jerch, and Connie Mao, for their insightful comments and kind guidance. The perspectives they provided substantially improved my dissertation.

I have benefited greatly from the generous advice of many faculty members in the department: Randy Dumm, Krupa Viswanathan, Jacqueline Volkman-Wise, Cameron Ellis, and Thorsten Moenig. I would also like to thank J. David Cummins for his influence at the early stage of my Ph.D. training on how to do good research, "keep at it, work hard, and don't cut corners." Special thanks to R. B. Drennan for showing me how to be a good educator and how to have fun in teaching.

Finally, I couldn't have made this far without the love and support from my parents and my lovely husband. Thank you for making this journey substantially easier.

TABLE OF CONTENTS

Page

LIST OF TABLES

Table Page

LIST OF FIGURES

CHAPTER 1
FINANCING SEVERE CLIMATE RISK: EVIDENCE FROM BUSINESSES DURING HURRICANE HARVEY

1.1 Introduction

Businesses have an increasing need to manage climate risk. In the past five years, the U.S. alone experienced 81 billion-dollar climate disasters with a total cost of $630 billion, accounting for one-third of the combined losses in the past 40 years (NOAA, 2021). For small firms with limited resources, funding losses from severe climate events may be challenging. A large literature has documented the financing frictions that small firms face. For example, collateral-constrained firms are less likely to insure or hedge (e.g., Rampini and Viswanathan, 2010; Rampini et al., 2014), rely more on cash and save out of cash flows (Nikolov et al., 2019). A firm's exposure to climate risk magnifies the financing problem: severe weather and rising seas will consume an increasing share of firms' retained earnings and prompt the needs for external financing; the interruption of cash flows following an event, however, will limit their ability to obtain credit. Using Hurricane Harvey as my setting, I study how small businesses finance severe climate risk following a major natural disaster.

Specifically, I analyze the credit reports of businesses located in the area affected by Hurricane Harvey. Previous research on businesses and severe climate events almost exclusively relies on either post-event surveys or aggregated economic outcomes. The credit reports allow me to capture more detailed financial information of firms in both affected and unaffected regions. My analyses thus extend to comparisons across firms over time (i.e., before and after the event). The setting of this study is Hurricane Harvey, a prototype of the increasing climate risk that firms face. Harvey, the second-costliest Atlantic hurricane in modern history, struck the Texas coast near Houston in August 2017 and caused $125 billion in economic losses (Blake and Zelinsky, 2018). Frame et al. (2020) estimate that climate change caused one-third of Harvey's total precipitation and $67 billion of direct economic damages. Climate scientists predict that severe events like Harvey will occur more frequently in the future (IPCC, 2018; Van Oldenborgh et al., 2017).

This paper has two goals. First, I investigate to what extent did Hurricane Harvey cause firm financial distress. Whether a firm can fulfill its financial obligations is an important measure of the firm's health after the storm. Loan performance speaks to the sufficiency of a firm's existing risk management, but also affects its future – a firm that does not make its loan payments today may face additional credit constraints tomorrow. Harvey's effect on firms is not obvious. Some reports indicate that the storm only modestly reduced firms' cash flows (JP Morgan Chase, 2018), while others describe more severe problems (Wall Street Journal, 2017; Houston Chronicle, 2018a). Second, I study how did firms use credit to finance losses. Credit is the most common type of external financing among small businesses (Federal Reserve Banks, 2020). I analyze firms' credit inquiries and debt balances on their credit reports to examine their need for external financing and their ability to access credit.

The credit report data include a random sample of 8,219 businesses. The firms in our data have fewer than 500 employees and are not subsidiaries of other firms. About 80% of these firms were drawn from the area affected by Hurricane Harvey; the remaining 20% are a national random sample, which serves as a control group. These credit data begin in June 2017 (two months before Harvey) and end in June 2018 (ten months post-Harvey). I match each firm's exact physical address with flood inundation data to determine the level of Harvey-related flooding at its location. About 40% of sampled firms in the disaster area were in flooded locations. I estimate treatment-intensity, difference-in-differences regression models to examine how the level of flooding affected firms' credit outcomes.

Regarding financial stress, I find a significant increase in delinquencies among firms flooded during Harvey. In the areas with the highest flood depth, on average 9.5% of firms' total loan balances became delinquent because of Harvey. This represents an 86% increase in impaired balances relative to pre-Harvey levels. Substantial heterogeneity underlies this average effect – about a tenth of these firms became delinquent on over half of their outstanding loan balance.

Harvey increased delinquencies of 90 days or less; I do not find a significant effect on the most severe impairments (such as bankruptcies), though the credit quality of some firms appears to continue to erode at the end of our time series. Delinquencies also increased among firms in the disaster area whose properties were *not* flooded, suggesting spillover effects from flooded areas. Such spillovers are especially important given the limited set of risk management strategies available to address contingent losses.

In addition to our baseline sample of non-subsidiary businesses (i.e., firms *without* parents), I also study the credit reports of 1,173 businesses *with* parents. These businesses have distinct credit reports from their parent companies. In contrast to our main findings, I find that businesses with parents do *not* become delinquent on their loans, suggesting flooding causes them less financial distress. One possible explanation for this observed difference is that subsidiaries' connection to the parent reduces financing barriers in a crisis (e.g., through internal capital markets, Campello, 2002; Desai et al., 2008). However, I do not investigate the underlying mechanisms which lead to these differences.

Regarding the financing of losses, I find that Hurricane Harvey increased the likelihood of firms applying for new credit, especially among firms already using credit before Harvey. Firms *without* existing debt were more likely to take on debt because of the storm. For example, consider the firms that experienced flooding of 2.7 feet or more (the top third of flooded firms). Firms *without* existing debt in these heavily flooded areas took on about 31% more debt than their counterparts outside the disaster area. On the other hand, firms *with* existing debt reduced their overall debt levels by 48% in the most flooded areas. The balances of some firms may be declining due to voluntary deleveraging (e.g., reducing debt due to smaller revenues after the storm); however, other firms with declining balances appear credit constrained, as they applied for new credit while their existing debt balances were declining.

This paper contributes to the literature on how severe climate events affect firms. Much of this literature comes from post-disaster surveys (e.g., Collier et al., 2020; Marshall et al., 2015), though several papers incorporate administrative data. Barrot and Sauvagnat (2016) show that natural disasters can adversely affect the supply chain partners of negatively affected firms. Gallagher et al. (2019) show that cash grants given to households can benefit local retailers in communities affected by severe tornadoes. Basker and Miranda (2018) examine firms' post-Katrina survival, productivity, and employment. They combine the Census's Longitudinal Business Database and its Survey of Business Owners and find that survival is less likely for smaller firms and for firms reporting more limited access to financing. This literature on climate risk speaks to a growing form of business uncertainty, and I offer a unique contribution to it by analyzing third-party credit reports.[1]

This paper also adds to the literature on financial constraint, financing distress, and their consequences (e.g., Kahl, 2002; Rampini and Viswanathan, 2010; Rampini et al., 2014; Sufi, 2009). Previous literature suggests that financially constrained firms and financially distressed firms may manage their financial needs differently (Nikolov et al., 2019). The findings of this paper further clarify the distinction. I observe that financially constrained firms are more likely to enter distress. For example, flooding caused independent businesses to fall behind on their debts, but flooding did not significantly affect businesses with parents. I also show that distressed firms' use of credit is more restricted.

This paper is structured as follows. I provide background on Hurricane Harvey in the next section. Section 1.3 describes the credit report data. Section 1.4 presents the empirical methodology. Section 1.5 discusses our results. In Section 1.6, I summarize our findings and discuss their implications.

[1]At least two papers examine *households'* credit reports following hurricanes. Gallagher and Hartley (2017) examine how Hurricane Katrina affected households' credit. They find a modest, short-term increase in credit card utilization, but on average Katrina *reduced* the debt balances of affected households. They suggest that this may result from households using flood insurance payments to settle their mortgages. Billings et al. (2019) examine the effects of Hurricane Harvey on household credit. They find very little or even positive effects on more affluent households; however, households entering the storm with existing financial constraints (e.g., higher credit card utilization) were more likely to experience negative financial outcomes including bankruptcy.

1.2 Background

On August 26, 2017, Hurricane Harvey made landfall near Rockport, Texas as a Category 4 tropical cyclone. Harvey stalled over the Houston metro area, dropping more than 27 trillion gallons of rain.[2] The resulting floodwaters covered more than a quarter of the Houston metro area. Nederland, Texas received over 60 inches of rain during Harvey, setting a new U.S. record for rainfall from a single event. Floodwaters damaged more than 300,000 structures and as many as 500,000 cars. Frame et al. (2020) estimate that climate change caused one-third of Harvey's total precipitation and $67 billion of direct economic damages.

In addition to direct physical damage, the storm also disrupted access to utilities and public infrastructure. More than 330,000 entities lost electricity due to Harvey-related flooding. Cable internet service was interrupted for more than 280,000 customers in the immediate aftermath, and continued to affect more than 150,000 customers a week later (FCC, 2017). U.S. Mail service was suspended in many locations from August 25 to September 11 (USPS, 2017). At least 500 roads were closed due to flooding and damage, and 118 were still closed after two weeks (NPR, 2017).

Major Disaster Declaration DR-4332-TX designated 41 counties to receive federal aid (FEMA, 2017b). I refer to these counties as the "disaster area" throughout this paper. The only form of federal assistance offered to businesses is disaster recovery loans from the Small Business Administration (SBA). Small businesses can borrow up to $2 million from this program to repair damaged property and/or offset revenue losses. One year following Hurricane Harvey, the overall approval rate for SBA disaster loans was about 40% (GAO, 2020).[3]

[2] Statistics throughout this section are from Blake and Zelinsky (2018) unless otherwise cited.

[3] Federal aid also includes funds to local governments for debris removal and repair of public property and infrastructure. Affected households can apply for federal grants and low-interest loans. The average grant amount was $8,900 (capped at $34,000); a fifth of households who applied for a grant received one (Walls and Cortes, 2018).

1.3 Data and Summary Statistics

I analyze data from Experian credit reports. To construct the sample, I randomly drew 10,200 firms that were listed as active businesses in the *ReferenceUSA* database in 2016. This includes a random sample of 8,000 firms in 49 Texas counties – 26 counties in the disaster area and 23 counties outside of the disaster area.[4] I stratified these 8,000 firms by county based on the number of firms reported for each county in the U.S. Census Bureau County Business Patterns (U.S. Census Bureau, 2017). The other 2,200 firms are a random sample from across the U.S., similarly stratified by state based on the number of businesses in each state. I observe each firm's credit reports at two time periods – June 30, 2017 (approximately two months prior to Harvey) and June 30, 2018 (approximately ten months post-Harvey). While most credit outcomes are reported at the two dates only, some variables provide more time-granular information, reporting outcomes in months or quarters. I use both structures of credit outcomes for our analysis.

Table 1.1 outlines our data filtering steps. First, the firm must have a credit report in June 2017. Second, I keep a single credit record for each firm, omitting duplicates. Experian provides credit reports at the firm level, so all establishments within a firm have the same credit report. In most cases, duplicates appear to be franchise locations or local branches of a large firm. Third, I omit firms that have a parent according to *ReferenceUSA*. Fourth, I exclude firms listed by Experian as having 500 or more employees based on the common standard that "small businesses" have fewer than 500 employees (e.g., SBA, 2014). I focus our analyses on businesses with fewer than 500 employees and without parents out of concern that larger, multi-business firms may have access to additional resources (e.g., internal capital markets) that make the credit reports of these firms difficult to compare with the other businesses in our sample. These filters reduce the data for our credit report analysis to the "Full Sample" of 8,219 firms. Lastly, in our sample for analysis of impairments, a

[4]According to the County Business Patterns of the U.S. Census Bureau (2017), 95% of businesses within the disaster area were located in these 26 counties. Hurricane Harvey primarily affected two SBA administrative districts in Texas, the Houston District, and the Lower Rio Grande Valley District.

firm must have positive loan balances on both June 30, 2017 and June 30, 2018. I restrict the sample in this way as only firms that are actively borrowing can have loan impairments. These filters create a smaller "Active Borrower Sample" of 2,614 firms.

Table 1.1: Data Cleaning and Filtering

Data Step	**Remaining Firms**
All firms with Experian credit records	10,200
Drop if no 2017 credit records	9,990
Drop duplicate credit records	9,722
Drop if has a parent (according to *ReferenceUSA*)	8,257
Drop if number of employees $\geq$ 500	8,219
Full Sample	**8,219**
Drop if 2017/2018 total loan balances = $0	2,614
Active Borrower Sample	**2,614**

Credit Variables. My primary outcome of interest in the credit report data is loan impairment, which I measure in several ways. First, I consider the proportion of total loan balances in four delinquency categories: 1-30 days delinquent, 31-60 days, 61-90 days, and over 90 days ("PctDelinquent, X days"). I then create a new variable ("PctImpaired") indicating the share of total loan balances that are not paid on time within the agreed terms, defined as:

$$\begin{aligned}\text{PctImpaired}_{it} = {} & (\text{PctDelinquent, 1-30 days})_{it} \\ & + (\text{PctDelinquent, 31-60 days})_{it} \\ & + (\text{PctDelinquent, 61-90 days})_{it} \\ & + (\text{PctDelinquent, over 90 days})_{it}. \end{aligned} \tag{1.1}$$

To evaluate more severe impairments, I examine the amount placed in collections in the last seven years, and the liability amount of legal filings (i.e., tax liens, judgments, and bankruptcies) over the previous seven years. Experian provides another impairment measure, Days Beyond Terms (DBT), on a monthly basis. DBT is a common measure of delinquency used by lenders. A firm's DBT is approximately the dollar-weighted average number of days the firm is past due on payments. Specifically,

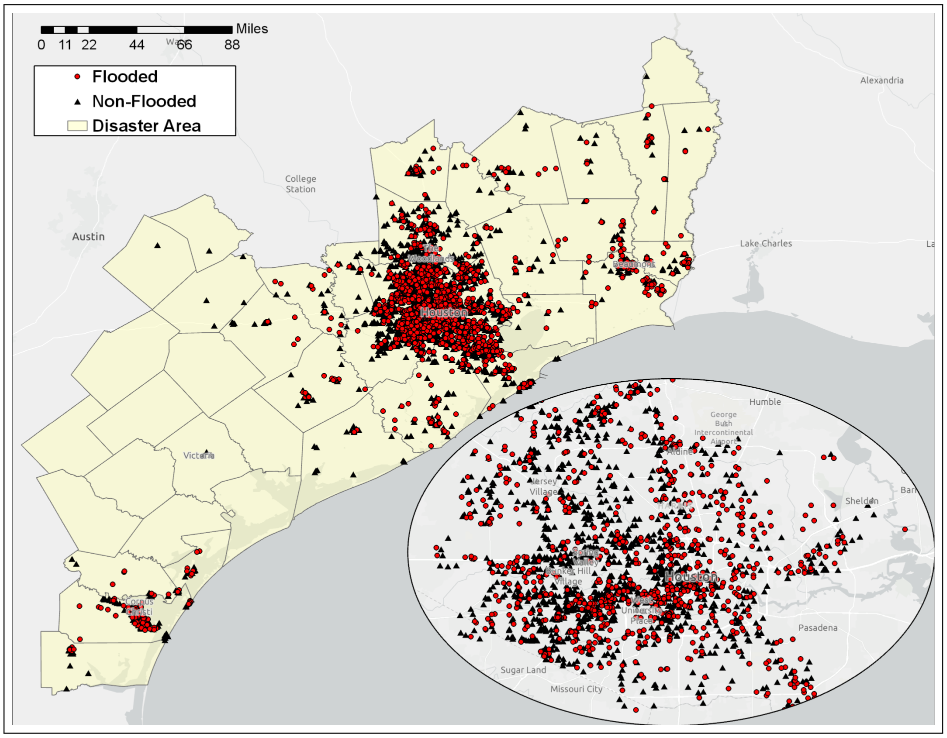
$$\begin{aligned}\text{DBT}_{it} = &\ (\text{PctDelinquent, 1-30 days})_{it} \times 15 \\ &+ (\text{PctDelinquent, 31-60 days})_{it} \times 45 \\ &+ (\text{PctDelinquent, 61-90 days})_{it} \times 75 \\ &+ (\text{PctDelinquent, over 90 days})_{it} \times 105. \end{aligned} \tag{1.2}$$

To examine how Harvey affected credit use, I study a firm's total loan balances and the number of new credit inquires. Other credit report variables that I use as controls include the number of employees and the number of years the firm has appeared in Experian's files.

Flood Variables. I use flood depth at the firm's location as our treatment variable. I geocode the primary business address of the firm as of June 2017 and match the coordinates to FEMA's estimated Harvey-related flood depth at that address ("Flood Depth"; FEMA, 2018). This measure of flooding uses water levels observed at river gauges and high water mark lines to interpolate flood depth throughout the disaster area. The estimated flood depths are continuous in feet, measuring up to 100 feet. Figure 1.1 presents the flooding distribution (flooded vs. non-flooded) of firms in the disaster area from our random sample, based on this FEMA flood depth data. About 39% of the firms located in this area are identified as flooded (red circles) and they are widespread across the disaster counties. For our primary analyses, I group flooded firms into terciles based on the flood depth at their location. The first tercile includes firms in areas with less than 1.7 feet of flooding. The third tercile includes firms in areas flooded 2.7 feet or more.[5]

[5] As a reference for the extent of damages caused by these levels of flooding, the U.S. Army Corps of Engineers reports that *homes* with a flood depth of 1.7 feet were about 30 percent damaged from Harvey and those with a flood depth of 2.7 feet were about 38 percent damaged (Houston Chronicle, 2018b).

Figure 1.1: Studied Firms in Disaster Area: Flooded vs. Non-Flooded



As a robustness check, I employ a second measure of flooding, "Remote Sensing" (FEMA, 2017a). This measure is binary (flooded vs. non-flooded) and uses Synthetic Aperture RADAR and Multispectral Imagery sensors to detect whether a particular location was flooded during Hurricane Harvey between August 26 and September 5, 2017.

Additional Control Variables. Using each firm's exact street address, I also identify its pre-Harvey flood risk zone designation using the FEMA National Flood Hazard Layer as of May 2017 (University of Texas, 2017). Flood risk zones comprise three broad categories, areas with: less than a 1% annual flood probability, a 1% or greater annual flood probability, or a 1% or greater annual flood probability and vulnerable to storm-induced wave damage. Further, I merge our data with the U.S. Census Bureau's American Community Survey (ACS, 2016), which provides demographic information by ZIP code (e.g., mean income, population, education, race).

Table 1.2: Summary Statistics

Variable	Total	Outside	No Flood, Disaster Area	Flooding: 1st Tercile (1, 1.69 ft]	Flooding: 2nd Tercile (1.69, 2.68 ft]	Flooding: 3rd Tercile >2.68 ft
Full Sample						
No. of Firms	8,219	3,051	3,171	717	608	672
Employees	9.52	10.06	9.14	8.50	9.96	9.51
	(27.71)	(30.69)	(24.32)	(28.36)	(28.63)	(26.92)
Years in File	16.02	16.48	15.67	15.49	16.81	15.38
	(10.64)	(10.84)	(10.52)	(10.52)	(10.73)	(10.23)
Total Balance ($)	25,669	32,726	25,070	7,617	27,849	13,739
	(349,843)	(500,607)	(232,620)	(44,564)	(294,560)	(151,200)
No. of Inquiries,	0.14	0.15	0.14	0.12	0.13	0.09
Q2 2017	(0.65)	(0.71)	(0.66)	(0.42)	(0.59)	(0.46)
Active Borrower Sample						
No. of Firms	2,614	980	1,007	209	209	209
Employees	16.44	17.52	15.53	14.38	18.92	15.31
	(42.15)	(46.45)	(37.25)	(42.85)	(46.29)	(38.02)
Total Balance ($)	77,890	100,814	74,683	25,959	72,967	42,706
	(615,050)	(879,615)	(403,303)	(79,739)	(488,401)	(269,138)
PctImpaired	0.15	0.17	0.14	0.14	0.12	0.11
	(0.28)	(0.3)	(0.28)	(0.27)	(0.25)	(0.26)
PctDelinquent,	0.07	0.07	0.08	0.07	0.06	0.04
1-30 days	(0.18)	(0.18)	(0.19)	(0.19)	(0.16)	(0.13)
PctDelinquent,	0.02	0.02	0.02	0.02	0.01	0.01
31-60 days	(0.09)	(0.1)	(0.09)	(0.08)	(0.03)	(0.07)
PctDelinquent,	0.01	0.02	0.01	0.01	0.01	0.01
61-90 days	(0.08)	(0.09)	(0.07)	(0.1)	(0.04)	(0.04)
PctDelinquent,	0.05	0.05	0.04	0.04	0.04	0.05
Over 90 days	(0.18)	(0.2)	(0.16)	(0.16)	(0.18)	(0.2)
DBT	7.51	8.98	6.66	6.72	6.05	6.93
	(20.66)	(22.88)	(18.77)	(18.53)	(19.27)	(21.55)
Collection ($)	1,555	3,194	516	887	492	603
	(25,954)	(41,883)	(4,783)	(5,803)	(4,262)	(4,776)
Legal Filing ($)	12,002	18,925	7,930	11,752	7,603	3,800
	(145,895)	(215,557)	(92,442)	(64,735)	(48,610)	(20,506)

Note: Values in the 1^{st} and 2^{nd} rows under each variable are means and (standard deviations), respectively. Variables are from the firm's credit report on June 30, 2017.

Summary Statistics. Table 1.2 provides summary statistics for our credit report data (measures are from the June 2017 credit reports). The first column describes the sample in total and is the focus of our description here; the remaining columns describe our control and treatment groups, which I discuss further in the sections below. The upper panel, marked "Full Sample," summarizes demographics, balances, and inquiries for our sample of 8,219 firms. Among these firms, the average number of employees is 9.5.[6] On average, these firms had 16 years of credit history in 2017. Their average loan balances totaled \$25,669 with a median value of \$0 – over 60% of the full sample had no loan balances before Harvey. Their average number of credit inquires in the second quarter of 2017 was 0.14. That is, for every seven firms in our full sample, I observe one credit inquiry made by a firm in the quarter.

The lower panel of Table 1.2 summarizes credit for the active borrower sample of 2,614 firms. These firms had average loan balances of \$77,890, with 15% of loan balances not paid on time before Harvey. Of the total balances, 7% were 1-30 days delinquent, 2% were 31-60 days delinquent, 1% were 61-90 days delinquent, and 5% were over 90 days delinquent. DBT indicates that firms are paying their debts 7.5 days beyond the invoicing due date on average. Also, the average amount placed in collections was \$1,555 and the average liability amount of legal filings (i.e., tax liens, judgments, and bankruptcies) was \$12,002. Overall, a relatively small proportion of these firms had any type of delinquencies, collections, or legal filings. For example, only 14% had delinquencies over 90 days and only 10% had any records of collections or legal filings before Harvey.

[6]The distribution of firms by size in our sample is very similar to the national distribution. For example, 62% of firms in our sample have fewer than 5 employees (versus 62% in the County Business Patterns Data) and 91% have fewer than 20 employees (versus 89%, Census Bureau, 2017).

1.4 Empirical Methodology

To examine how flooding from Hurricane Harvey affected firms' credit outcomes, I use difference-in-differences estimations in which flooding is the measure of treatment. The general model is:

$$\begin{aligned} y_{it} = \alpha_0 + \alpha_1 \mathrm{I}_t(\text{Post-Harvey}) + \alpha_2 \mathrm{I}_i(\text{Flooded}) \\ + \alpha_3 \mathrm{I}_t(\text{Post-Harvey}) \times \mathrm{I}_i(\text{Flooded}) + \varepsilon_{it}. \end{aligned} \tag{1.3}$$

where i indexes firms and t indexes time. y_{it} is a general term for the credit outcome of interest. I_t(Post-Harvey) is an indicator for post-Harvey periods and I_i(Flooded) is an indicator for firms that were flooded by Harvey. The coefficient α_3 captures the treatment effect.[7]

I implement two versions of this general model. The first is a difference-in-differences model which imposes treatments at increasing flood depths. Specifically, I estimate:

$$\begin{aligned} y_{it} = \beta_0 &+ \beta_1 \mathrm{I}_t(\text{Post-Harvey}) \times \mathrm{I}_i(\text{No Flood, Disaster Area}) \\ &+ \beta_2 \mathrm{I}_t(\text{Post-Harvey}) \times \mathrm{I}_i(\text{Flood 1st Tercile}) \\ &+ \beta_3 \mathrm{I}_t(\text{Post-Harvey}) \times \mathrm{I}_i(\text{Flood 2nd Tercile}) \\ &+ \beta_4 \mathrm{I}_t(\text{Post-Harvey}) \times \mathrm{I}_i(\text{Flood 3rd Tercile}) \\ &+ \theta \mathrm{I}_t(\text{Post-Harvey}) \times X_i + \mathrm{FE}_i + \mathrm{FE}_t + \varepsilon_{it}. \end{aligned} \tag{1.4}$$

I use a set of indicators for whether a firm was located in one of five groups at the time of Hurricane Harvey: (1) outside the disaster area (the omitted reference group); (2) in the disaster area but not flooded ("I(No Flood, Disaster Area)"); (3) in

[7] The model estimates the intent-to-treat (ITT) effect, rather than the average treatment effect on the treated, due to imprecision in measuring flooding. Billings et al. (2019) use Harvey flooding to estimate ITT effects on households' credit outcomes. I can more precisely estimate flooding as I measure flooding at the exact address, while they measure flooding at the census block level. However, our flood measure is still subject to measurement error because flood levels are modeled, firms may not be located at ground level, etc. This measurement error will partially attenuate our estimates relative to true average treatment effects.

the lowest flood depth tercile ("I(Flood 1st Tercile)"); (4) in the middle flood depth tercile ("I(Flood 2nd Tercile)"); (5) in the highest flood depth tercile ("I(Flood 3rd Tercile)"). I use firms in the disaster area that were not flooded to examine possible spillovers from the disaster, e.g., due to changes in consumer demand, utility outages, employee disruptions, etc. The treatment effects in Eq. (1.4) are captured by β_1 to β_4.

I interact a set of pre-Harvey control variables X_i with the post-Harvey indicator as a way of controlling for non-flood heterogeneity in Harvey's effects. The control variables include the firm's number of employees, the years that a firm had a credit file (a proxy for firm age), its industry (using the firm's 2-digit SIC code), its ZIP-level demographic information (logged mean income, logged population, the proportion white, proportion with bachelor's degrees, and the Gini coefficient for income). I also control for the flood risk zone of firms in the disaster area. Models include firm fixed effects (FE_i) and time fixed effects (FE_t). I also investigate several alternative specifications of flooding in the model (e.g., logged flood depth).

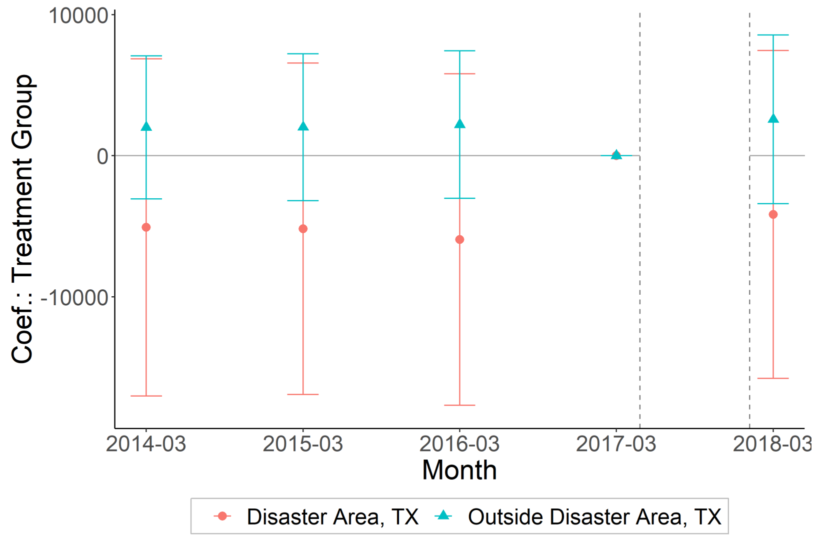
For several credit outcomes, the credit reports provide more time-granular information. To examine how these credit outcomes evolve over time, I implement an event study version of Eq. (1.3), which replaces I_t(Post-Harvey) with a set of indicators I_t(Time) for each time period (e.g., monthly values of DBT), expressed as:

$$\begin{aligned} y_{it} = \gamma_0 + \sum_t \gamma_{1t} \mathrm{I}_t(\mathrm{Time}) \times \mathrm{I}_i(\mathrm{Flooded}) + \sum_t \theta_t \mathrm{I}_t(\mathrm{Time}) \times X_i \\ + \mathrm{FE}_i + \mathrm{FE}_t + \varepsilon_{it}. \end{aligned} \tag{1.5}$$

The last observation before Harvey, in June 2017, serves as the reference period.

The regressions in Eq. (1.4) and (1.5) estimate a causal effect of flooding on the studied credit outcomes (e.g., delinquency) under two assumptions. Our first identifying assumption is that, given model controls, treatment assignment can be viewed as random with respect to the considered credit outcomes. Regarding this assumption, the timing and severity of flooding can be understood as quasi-random in the disaster area: flooding notably differs across hurricanes that affect the same

Figure 1.2: Number of Establishments at County Level



Note: The reference group is counties outside of Texas. The dotted vertical lines mark the time when Harvey occurred. Data are from Business Dynamics Statistics and Nonemployer Statistics of the U.S. Census Bureau (2018a,b). The data include sole proprietors with no paid employees and establishments of firms with fewer than 500 employees.

area due to variation in rainfall intensity and location. Since I control for FEMA flood risk zones, results can be interpreted as comparisons within a flood zone.

The second identifying assumption is that the control and treatment groups would respond comparably had they both been affected in the same way by Harvey, commonly called the "parallel trends" assumption. I examine pre-event trends in our outcomes of interest at the firm level (e.g., loan balances and inquiries) for the treatment and control groups using the event study model from Eq. (1.5). This analysis provides general support for the parallel trends assumption as none of the pre-Harvey coefficients are significantly different from zero (Figure A.2 of Appendix A). Additionally, I examine aggregate business statistics at the county level (e.g., establishment count, firm entry, firm exit, etc.) and implement difference-in-differences estimations, in which counties outside Texas are the control group and counties in Texas are the treatment group. I show in Figure 1.2 that pre-event trends on the number of establishments for counties in the Harvey disaster area and other counties in Texas do not statistically differ from the national trend (see Table A.1 for additional outcomes).

One additional consideration is how potential firm exits (i.e., permanent closures) due to Harvey may affect our estimates. Credit report data include closed firms – a firm's credit report remains available for several years after its last recorded entry. This allows us to estimate the effect of Harvey on severe impairment outcomes. For example, in the case of an insolvent firm declaring bankruptcy because of Harvey, the consequences are reflected in the liability amount of legal filings in the last seven years. Our second set of analyses examine firms' inquiries and balances after Harvey, and these *could* be affected by firm closures. The credit report does not indicate whether a firm has permanently closed. By inadvertently including closed firms, these regressions could underestimate credit demand measures such as inquiries since closed firms would not apply for credit. As a result, to the extent that Harvey increases credit use, our estimates of outcomes such as increasing inquiries and increasing balances should be understood as lower-bound estimates.

1.5 Results

This section describes the effects of flooding on three credit outcomes: impairments, inquiries, and balances. In the first two subsections, I examine overall impairment (1.5.1) and further decompose impairment by length of delinquency (1.5.2). I conduct the overall impairment analysis on firms *with* parents and compare the results in subsection 1.5.3. In Section 1.5.4, I discuss our event study results which use more time-granular data on impairment, inquiries, and balances, with additional focus on heterogeneity across firms.

1.5.1 Impairment

I evaluate whether Harvey caused firms to delay paying their loans by examining the share of loan balances that are not paid within the agreed time period ("PctImpaired"). Table 1.3 presents the results of estimating Equation (1.4) for the active borrower sample defined in Section 1.3.[8] In the table from Columns (1) to (4), I include our flood tercile variables and an indicator variable ("I(No Flood, Disaster Area)"), which captures businesses who were not flooded according to FEMA estimates but were located in a disaster-affected county. Thus, our reference group is firms that are located outside of the area affected by Harvey. I begin with a parsimonious specification (without any controls) in Column (1) and add fixed effects and controls step-wise across the columns. Our preferred model is Column (4), which includes firm controls (age, size, industry, and flood zone designation), ZIP code controls (mean income, population, etc.), and firm and time fixed effects. I also include an indicator for firms located in Texas to control for any potential systemic differences between these firms and those in other states.

Column (4) shows that flooding is positively and significantly related to the change in firm's impaired loan balances, and that the magnitude of the effect is largest for the most severely flooded firms. On average, for firms in the highest flood tercile, Harvey caused a 9.5% share of their total balances to become impaired 10 months after the disaster. Before Harvey, 11% of their loan balances were impaired on average (see Table 1.2), so this effect represents an 86% increase over the pre-Harvey level for these firms. The estimated regression coefficient provides the average effect within the third tercile; however, there is substantial heterogeneity in impairment among firms in the most flooded area. About one-third of these firms had any increase in impairment, and approximately 9% of the firms had over 50% of their loan balances become impaired.[9]

[8]The active borrower sample includes only firms who had positive balances in *both* 2017 and 2018, which provides a balanced panel. These active borrowers are the population of interest for assessing loan impairments because, mechanically, firms without loan balances cannot be be delinquent. As robustness tests, I conduct the same analysis and (1) include the 478 firms who had positive 2017 total balances but zero 2018 total balances, and (2) use the full sample, which includes firms with zero balances in 2017. In both cases, the results are similar to those presented in Table 1.3, although as one might expect, the sizes of the coefficients are smaller in the full-sample estimation (see Appendix A Table A.4).

[9]To provide further context, I examine changes in Experian's proprietary "Intelliscore" index of credit risk. The score ranges between 0 and 100 with higher scores indicating lower credit risk (Experian, 2013). For flooded firms with an increase in impairment, their average Intelliscore decreased from 50 to 35, about half of one standard deviation. Intelliscore groups businesses into one of five risk classes and half of these firms were downgraded by at least one risk class.

Table 1.3: Share of Balances that are Impaired, Active Borrower Sample

	PctImpaired						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(Post-Harvey) ×							
I(No Flood, Disaster Area)	0.028*** (0.010)	0.028*** (0.010)	0.022* (0.012)	0.034** (0.016)	0.034** (0.016)	0.016 (0.015)	0.046*** (0.015)
I(Flood 1st Tercile)	0.054*** (0.016)	0.054*** (0.016)	0.049*** (0.016)	0.061*** (0.019)			
I(Flood 2nd Tercile)	0.058*** (0.012)	0.058*** (0.012)	0.050*** (0.014)	0.062*** (0.017)			
I(Flood 3rd Tercile)	0.091*** (0.016)	0.091*** (0.016)	0.083*** (0.016)	0.095*** (0.019)			
I(Flooded)					0.072*** (0.016)		
ln(Flood Depth)						0.056*** (0.013)	
I(Flooded, Remote)							0.072*** (0.019)
I(TX)				−0.018 (0.016)	−0.018 (0.016)	−0.017 (0.015)	−0.019 (0.016)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Firms	2,614	2,614	2,614	2,614	2,614	2,614	2,614
Firm-Year Obs	5,228	5,228	5,228	5,228	5,228	5,228	5,228
R^2	0.004	0.792	0.793	0.793	0.792	0.793	0.792

Note: Dependent variable is the share of loan balances that are not paid on time within the agreed terms for a firm's continuously reported loans ($PctImpaired_i$). Column 1 shows the results without any fixed effects; the regression in Column 2 includes firm fixed effects; Column 3 adds in control variables. Our preferred model is in Column 4, in which I also include an indicator for firms located in Texas to control for any potential systemic differences between these firms and those in other states. Disaster area represents being in one of the 41 counties that were eligible for federal aid in the presidential disaster declaration. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Firms in the disaster area that did not experience flooding at their location also had a 3.4% share of their total balances move into impairment. This increase in impairment among non-flooded firms might indicate spillover effects from flooded areas (e.g., firms may have been unable to repay their lenders because of lost revenue from customer disruptions) or that the firm incurred damage at another location (e.g., damage to automobiles parked elsewhere).

The results are qualitatively similar throughout all specifications in Column (1) to Column (4). Inclusion of firm, industry, and ZIP code controls do not substantially change the estimated effects of flooding. Controlling for the flood zone of firms in the disaster area also does not change our results. That is to say, a firm's loan impairments depend on its actual flood experience in Harvey, irrespective of whether it was located in an area with a high risk designation on flood maps.

Similarly, I observe positive and significant effects using two alternative specifications of flooding based on our "Flood Depth" data: an indicator for whether the firm experienced any flooding ("I(Flooded)") in Column (5), and the logged continuous flood depth ("ln(Flood Depth)") in Column (6).[10] The results are also robust to using an alternative measure of flooding: Column (7) shows that impairment increased in flooded areas when using a flood indicator based on our remote sensing data ("I(Flooded, Remote)").[11]

1.5.2 Decomposition of Impairment and Severe Outcomes

I decompose impairment into four categories by length of delinquency: 1-30 days delinquent, 31-60 days delinquent, 61-90 days delinquent, and over 90 days delinquent. Columns (1) to (4) in Table 1.4 present the regression results for delinquencies. The dependent variables are the share of a firm's outstanding loan balances that are delinquent ("PctDelinquent") in each of the four categories. The largest effect from flooding is on the shortest-term delinquencies (1-30 days delinquent): on average, being in the highest tercile of flood depth led firms to make payments that were 1 to 30 days late on a 5.8% share of their total loan balances. This more than doubles their

[10]When using logged flood depth, I recode the cases in which the flood depth is zero as ln(Flood Depth) = 0 and identify these firms with the regression dummy, "I(No Flood, Disaster Area)."

[11]I also examined whether severe wind increased loan impairments using catastrophe modeling data from AIR Worldwide. However, wind data are quite limited, available for only 7.6% of the sampled firms in Texas. I do not observe any significant effects on loan impairments due to wind; however, this null result may be due to the limitations of these wind data.

pre-Harvey 1-30 day delinquent rate. I observe a smaller but also significant effect on 61-90 day delinquencies, these increased by a 2.2% share of their total balances for firms in the third flood depth tercile. This represents more than a 200% increase over their pre-Harvey levels. The most severe delinquency level (over 90 days delinquent) does not appear to be significantly affected.

In this table, I also evaluate more severe credit outcomes: the amount placed in collections (Column 5) and the liability amount in legal filings (i.e., tax liens, judgments, and bankruptcies, Column 6). These two variables reflect the cumulative liability amount of any collections and legal fillings in the previous seven years. I do not observe significant increases in these extreme credit outcomes for firms within the most flooded tercile.[12]

In Appendix A, I also look at the *number* of reported loans (versus the balances examined here), examining whether Harvey affected the share of loans that are impaired. This provides insights on how impairments are distributed across different loans owed by the same firm. I find that the ratio of impaired loans, particularly those that are on average 1-30 days and 31-60 days delinquent, increases for flooded firms, although the magnitude of the effect does not seem to differ significantly by the levels of flooding (Table A.6).

Taken together, the results show a meaningful decline in firms' loan performance due to Hurricane Harvey. I do not find that flooding affected the most severe credit outcomes, such as collections and legal filings. These outcomes are somewhat rare, so our analysis may have insufficient poIr to detect them (e.g., fewer than 3% of firms in the control group had an increase in collections or legal filings). In addition, it could be that the worst credit outcomes do not occur until after the end of our credit data, which ends just under a year post-Harvey. One-year outcomes, however, are important benchmarks in the literature, including in prominent studies using consumers' credit reports (e.g., Finkelstein et al., 2012). Our findings motivate future research to examine credit outcomes over longer time horizons.

[12] Although firms in the first flood tercile appear to have some significant increases in collections and firms in the second flood tercile had significant decreases in legal filings, these results are mainly driven by one or two observations.

Table 1.4: Share of Balances that are Delinquent, Collections, and Legal Filings

	PctDelinquent					
	(1) 1-30 days	(2) 31-60 days	(3) 61-90 days	(4) 90+ days	(5) ln(Collection)	(6) ln(Legal)
I(Post-Harvey) ×						
I(No Flood, Disaster Area)	0.010	0.003	0.011**	0.009	0.036	−0.055
	(0.011)	(0.006)	(0.005)	(0.010)	(0.056)	(0.039)
I(Flood 1st Tercile)	0.026*	0.017**	0.012**	0.007	0.189**	0.049
	(0.014)	(0.007)	(0.005)	(0.009)	(0.074)	(0.120)
I(Flood 2nd Tercile)	0.035**	0.014*	0.011*	0.002	−0.138	−0.145**
	(0.017)	(0.007)	(0.006)	(0.009)	(0.142)	(0.060)
I(Flood 3rd Tercile)	0.058***	0.008	0.022***	0.007	0.077	−0.151
	(0.014)	(0.008)	(0.005)	(0.011)	(0.123)	(0.096)
I(TX)	−0.017	0.00001	−0.005	0.003	0.095	−0.065
	(0.013)	(0.007)	(0.006)	(0.010)	(0.071)	(0.064)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes
No. of Firms	2,614	2,614	2,614	2,614	2,614	2,614
Firm-Year Obs	5,228	5,228	5,228	5,228	5,228	5,228
R^2	0.687	0.562	0.712	0.870	0.916	0.947

Note: Dependent variables from Columns 1 to 4 are the share of a firm's continuously reported loan balances that is delinquent ($PctDelinquent_i$) at four different levels: 1-30 days delinquent, 31-60 days delinquent, 61-90 days delinquent, and over 90 days delinquent. Dependent variable in Column 5 is the logged dollar amount placed in collections in the previous seven years ($ln(Collections_i)$). Dependent variable in Column 6 is the logged liability amount of legal filings (*i.e.*, tax liens, judgments, and bankruptcies) in the last seven years ($ln(Legal_i)$). The models include firm fixed effects, year fixed effects, and control variables. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

1.5.3 Impairment: Businesses with Parents

In this section, I investigate the effects of flooding on loan impairments for small businesses *with* parents. These businesses are subsidiaries in multi-business firms, according to *ReferenceUSA*, and have a distinct credit report from the parent company. Compared to businesses *without* parents, I expect that businesses with parents have additional resources that may reduce their financial distress and so improve their ability to meet their existing credit obligations following Harvey. For example, businesses with parents experience fewer financing frictions due to access to internal capital markets (e.g., Campello, 2002; Desai et al., 2008; Giroud and Mueller, 2019). While financing frictions are a well-documented distinction, subsidiaries may differ from independent businesses along several dimensions. It is important to note that our analyses here do not identify what features of businesses with parents may lead them to respond to Harvey differently.

Businesses with parents are excluded from the baseline sample that I use in the analyses above. The sample of businesses with parents includes 1,173 businesses, 313 of which are active borrowers. Compared to the baseline sample, these active borrowers are larger: the median business has 10 employees (versus 5 in the baseline sample, see Appendix A Table A.7). The industry composition also differs: 18% of these active borrowers *with* parents are in finance, insurance, and real estate versus only 8% in the baseline sample.

We replicate our preferred difference-in-differences specification on overall impairments (reported in Table 1.3, Column (5)). Table 1.5 reports our results for the samples with parents. Column (1) is a replication using our baseline (non-parent) sample for reference. In Column (2), we report the estimation using the sample of active borrowers *with* parents. Because of the smaller sample size, we use below- and above-median flood depth as the measure of flooding instead of depth terciles. In contrast to firms *without* parents, we do not observe significant changes in the share of loan balances that are impaired for flooded firms *with* parents. These results suggest that businesses with parents are less likely to enter financial distress, as measured by loan impairment, for a given level of flooding.

One potential concern is that the observed differences between Columns (1) and (2) might not be due to whether a business has a parent, but instead to compositional differences (e.g., industry, size) between businesses with and without parents. While our preferred specification in Column (2) of Table 1.5 controls for size and for features of the business with fixed effects, these controls might be insufficiently flexible. To address this, we estimate the model using a weighted approach (Imbens and Wooldridge, 2009) and report the result in Column (3). For this, we first estimate the probability of a firm having parents (i.e., propensity scores). We then weight the

Table 1.5: Active Borrowers with vs. without Parents

	PctImpaired			
	(1)	(2)	(3)	(4)
I(Post-Harvey) ×				
I(No Flood, Disaster Area)	0.034**	−0.002	−0.025	−0.062***
	(0.016)	(0.044)	(0.042)	(0.022)
I(Flood ≤ Median)	0.055***	−0.004	−0.012	−0.005
	(0.016)	(0.062)	(0.045)	(0.032)
I(Flood > Median)	0.090***	−0.009	−0.033	−0.041
	(0.018)	(0.052)	(0.051)	(0.033)
I(TX)	−0.018	0.020	0.017	0.007
	(0.016)	(0.050)	(0.049)	(0.038)
Parents	No	Yes	Yes	Yes
Weighted	No	No	Propensity Score	Industry & EE size
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes
No. of Firms	2,614	313	313	313
Firm-Year Obs	5,228	626	626	626
R^2	0.793	0.755	0.738	0.760

Note: Dependent variable is the share of loan balances that are not paid on time within the agreed terms for a firm's continuously reported loans ($PctImpaired_i$). The sample in Column 1 is our baseline active borrower sample with no parents. The sample in Columns 2 to 4 consists of active borrowers with parents, among whom 54 firms were flooded. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

regression on the active borrower sample *with* parents by the inverse probability of a firm not having parents. That is, we re-weight the regression for businesses with parents based on the composition of businesses in the baseline sample. We use a gradient boosting decision tree algorithm to improve predictive accuracy in the first step.[13] In Column (3), we show that the result remains similar to the unweighted regression result in Column (2).

[13] The predictor variables include size, age, industry, state, and ZIP-level demographic variables (e.g., mean income, population, education, race, income Gini index). Compared to a logistic regression, a decision tree is better at detecting nonlinear interactions between explanatory variables (Imbens and Wooldridge, 2009). A boosting approach further improves predictive power by implementing a sequential learning procedure where models are built on each other and adjusted by putting particular emphasis on the data points that previous models predicted poorly. The approach alleviates any misspecification concerns for the estimation of propensity scores (Imbens and Wooldridge, 2009). We find qualitatively consistent results using logistic regression.

One disadvantage of using a decision tree algorithm for the propensity score calculation is the loss of interpretability. For example, the algorithm indicates industry and number of employees are the top two predictors of whether a firm has parents, but the exact relationships remain unknown. For comparison, in Column (4) we simply weight the regression by industry and employee size categories in our baseline sample of businesses without parents. The estimated coefficients on flooded firms become slightly more negative but are still not statistically significant.

1.5.4 Event Studies of Impairment, Inquiries, and Balances

In this section, I exploit time-granular data from firms' credit reports to further assess the effects of flooding on impairment. I also examine whether Harvey led firms to apply for credit and if it affected their debt balances. In these analyses, I explore heterogeneity across groups with different characteristics (e.g., firms with and without existing debt). The regressions in this section follow Equation (1.5), which allows for the effects of Harvey to evolve over time. Because the full list of interacted coefficients is extensive, I summarize the estimation results in Figure 1.3. I display results using logged flood depth as the measure of flooding (rather than depth terciles) as it captures variation in flood intensity with a single variable, making the figure easier to interpret. I provide the full regression results and supporting analyses, including the event study results using flood terciles in Appendix A.

For impairment, I examine monthly Days Beyond Terms (DBT), which represents the average number of days that the firm is past due on its loan payments, weighted by loan balances (see Equation (1.2)). Each credit report file includes monthly DBT for the past six months. Our data, the June 2017 reports and the June 2018 reports, allow us to construct a pre- and post-Harvey panel of firms where January 2017 to June 2017 represent the pre-Harvey periods, a gap exists in the credit report data data from July 2017 to December 2017, and then January 2018 to June 2018 represent the post-Harvey periods.[14]

[14]For the DBT analysis, I only keep firm observations in a particular month with positive total balances and, thus, the estimation uses an unbalanced panel. The min and max number of firms

Panel A of Figure 1.3 plots the event study coefficients of monthly DBT on logged flood depth. The coefficients capture the average change in credit outcomes relative to June 2017 (i.e., approximately two months before Harvey) as a function of flood severity, compared to those outside the disaster area. I observe that monthly DBT appears to be worsening and it is increasing significantly in the last month of the data. In June 2018, for every 10% increase in flood depth, DBT increased by 0.15. For example, the flooded firms in our sample had a median flooding of 24 inches. Our estimates indicate that another 2.4 inches of flooding (i.e., a 10% increase) would cause firms to become 0.15 more days past due. The large confidence intervals illustrate substantial heterogeneity in the affected population.

Next, I examine the number of inquiries as a measure of a firm's demand for new credit. Unlike the analysis of impairments which only examined borrowers with loan balances, I extend my attention to the full sample of all 8,219 firms regardless of whether they had any existing debt. Information on inquiries is provided on a quarterly basis. Accordingly, I construct a balanced panel in which Q4 2016 to Q2 2017 represent the pre-Harvey periods, Q3 2017 is not observed in the data, and Q4 2017 to Q2 2018 represent the post-Harvey periods. I anticipate that credit demand may differ between firms who actively use credit and those who do not and so I examine inquiries by separating the sample into two groups: "non-borrowers" are firms who had no existing debt as of January 2017; "borrowers" are those who did.

Overall, I find that the number of credit inquiries increases significantly in flood intensity (shown in Appendix A, Table A.9). Panel B of Figure 1.3 plots the event study coefficients of quarterly inquiries on the logged flood depth for both non-borrowers and existing borrowers. The significant increase in number of inquiries comes from firms with existing debt. In particular, their inquiries increase significantly in Q1 2018 by 0.01 for every 10% increase in flood depth (e.g., another 2.4 inches more flooding from the median flood depth). In separate regressions, I examine inquiries using flood terciles (instead of logged flood depth), which clarifies the magnitude of the effect:

with positive balances in a particular month are 2,232 and 2,177, respectively. I also examined a balanced panel keeping only firms with positive balances in every month, and the results are consistent.

existing borrowers in the most flooded tercile increased their credit inquiries by 50% relative to pre-Harvey levels. The development of inquiries for *non-borrowers*, on the other hand, does not seem to change over time.

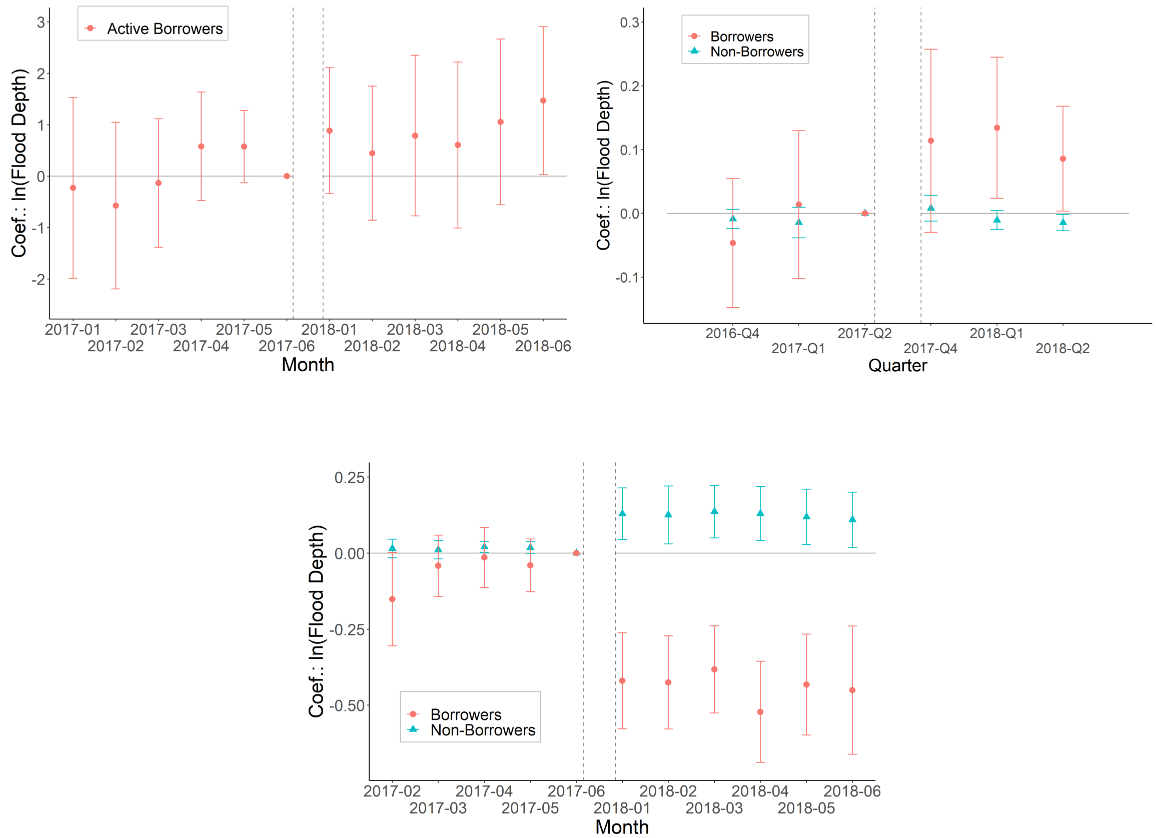
Regarding changes in loan balances, I also observe a divergence between existing borrowers and non-borrowers in Panel C of Figure 1.3.[15] Non-borrowers started borrowing in the post-Harvey periods and their total loan balances increased significantly with flood depth. I estimate that an increase of 2.4 inches of flooding (i.e., a 10% increase) causes non-borrowers to take on 1% more debt than those with a median flood depth of 24 inches. In contrast, flooding from Harvey caused a *decrease* in loan balances for existing borrowers. The loan balances of existing borrowers consistently stayed at a lower level compared to pre-Harvey periods. A 10% increase in flooding from Harvey caused a 3% *decrease* in loan balances for existing borrowers.[16]

Overall, my results in this section suggest that the role of credit during recovery differed across firms, depending on whether they had existing debt when Harvey occurred and how badly they were flooded. Flooding from Harvey led firms *without* existing debt to start borrowing. I do not observe a significant increase in inquiries among these firms, so it may be that these firms used existing credit lines. Harvey led firms *with* existing debt to apply for more credit. Several possibilities could explain why the balances of firms with existing debt fell. The increase in inquiries combined with the decrease in balances suggests that at least some firms were credit-constrained. At the same time, other firms may have decided to deleverage voluntarily due to reduced revenues following Harvey.

[15] I explore the probability of having any inquires and the probability of having any debt balances in Table A.10 and Figure A.3 of Appendix A. Similar trends are observed for both borrowers and non-borrowers.

[16] One possibility is that the observed differences between existing borrowers and non-borrowers is actually capturing effects of firm size. In Figure A.4 of Appendix A, I divide the data to compare larger firms (those with at least 10 employees) to smaller firms. I find that flooding increases the inquiries of larger firms, but not smaller firms; however, I do not find significant effects of flooding on the balances of either larger or smaller firms. I conclude that the borrower versus non-borrower comparison is capturing something distinct from firm size, perhaps more closely akin to debt overhang (Myers, 1977).

Figure 1.3: Evolution in DBT, Inquiries, and Balances



Note: Panel A and Panel C show the evolution of monthly DBT and monthly balances. The figures plot 95% confidence interval of event study coefficients (i.e., γ_{1t} in Equation (1.5)) of DBT and logged credit balances on the logged flood depth. The coefficients capture the average change in credit outcomes relative to June 2017 (i.e., approximately two months before Harvey) as a function of flood severity, compared to those outside the disaster area. The vertical, dashed lines mark the period July to December 2017, during which I do not observe monthly DBT and balances. Harvey occurred during that period. Similarly, Panel B shows the evolution of quarterly inquiries. The quarterly before Harvey, Q2 2017, is the reference period. The vertical, dashed lines mark the period Q3 2017, during which I do not observe quarterly inquiries. Harvey occurred during that period. For analyses of inquiries and balances in Panel B and Panel C, I compare two groups: firms with zero balances as of January 2017 ("non-borrowers") and those with positive balances at that date ("borrowers").

1.6 Discussion and Conclusion

I examine how firms were financially affected by Hurricane Harvey and how they used credit to finance losses. I study the business credit reports of small businesses and find that Hurricane Harvey increased loan delinquencies. These effects appear primarily in delinquencies of 90 days or less rather than collections or bankruptcies.

Firms in non-flooded areas also had a significant increase in loan impairment, which may be due to business disruptions. I also show that flooded firms without existing balances took on debt following Harvey. For flooded firms with existing balances, I observe an increase in new credit inquiries while existing balances decline, a pattern suggesting that these firms are credit constrained.

My results have several specific implications. First, the findings highlight the financial challenges imposed by a severe climate event on small firms. In many coastal communities, climate change is increasing the frequency and severity of storms like Harvey (IPCC, 2013). As a result, a larger share of firms' future cash flows will be consumed by preparing for and responding to these severe events. Small businesses are often the cornerstones of the local economy; the success of these communities in navigating climate change may be tied to their firms' ability to adapt.

Second, I show that although small firms have the need for external financing following a disaster, the role of credit is proven limited. It highlights the importance of working capital for a relatively large set of disaster-affected firms. Currently, the only federal assistance offered to firms is disaster loans. Still, some firms may be unable to access these loans. One potential public policy approach is to subsidize business savings accounts to encourage businesses to save more for severe events.

Lastly, I find spillover effects on loan impairments for non-flooded firms in the disaster area, potentially highlighting the business interruption's financial challenges. Even if a firm had business interruption coverage, the lack of significant property damage might have precluded any claims payments for the lost revenue. Considering the recent debate about business interruption coverage during the COVID-19 pandemic, insurers and policymakers might revise this coverage to protect firms more fully during severe events. Product innovations are likely necessary to overcome the inherent challenges of insuring decreases in revenue in settings like these.

CHAPTER 2
MULTIEMPLOYER DEFINED BENEFIT PENSION PLANS: EMPLOYER WITHDRAWALS AND FINANCIAL VULNERABILITY

2.1 Introduction

Multiemployer defined benefit (DB) pension plans are facing severe funding challenges. Sponsored by a group of private firms (instead of only one firm), multiemployer DB pension plans cover over 10 million participants. The participating employers are characterized by small businesses in the blue-collar industries such as construction, transportation, mining, and etc. According to the Pension Benefit Guaranty Corporation (PBGC, 2018), 99% of the multiemployer DB pension plans are underfunded. Even the Multiemployer Insurance Program operated under PBGC, the federally chartered corporation that protects participants in the event of a plan fails, is projected to become insolvent by 2026 (PBGC, 2020). The negative outlook of funding levels of multiemployer pension plans suggests that the retirement benefits of millions of people could be in danger in the near future.

To help strengthen the multiemployer pension plan's financial health, the Pension Protection Act of 2006 (PPA) introduced funding rules to identify financially challenged plans and specify requirements for improvements. The improvement requirements consist of either increasing employer contributions or decreasing participant retirement benefits or a combination of both. However, the associated costs imposed on sponsoring firms and their workers may be substantial and can thus undesirably create incentives for some of these firms to drop out of the plan (see, e.g., New York Times, 2014a). Therefore, the PPA funding rules pose an economic tradeoff between the benefits of corrective actions to a plan's funding and the costs of shrinking contribution base from pushing employers to leave the plan. The tradeoff creates uncertainty regarding the reform. On the one hand, conditional on firms staying with the plan, the act provides a discipline that would contribute to the plan's financial health. On the other hand, the act might increase withdrawals, further undermining the plan's financial health. In this paper, I explicitly explore whether employer withdrawals are the unintended consequences of the current funding rules.

Establishing the causal link between the PPA funding rules and employer withdrawals, however, is a difficult task. The plans subject to the improvement requirements are all financially challenged. Likely, any observed withdrawals could be motivated by sponsoring employers realizing the bleak financing future of the plan. Exiting a multiemployer pension plan might be a strategic move to avoid the daunting task of saving the plan if they believe the funding level will continue to erode.

To address the identification challenge, I exploit the fact that the funding rule requirements are set differently depending on a plan's funding. A plan's funding is an actuarial-driven projection of asset-to-liability percentage. According to PPA, a multiemployer pension plan with less than 80% funded percentage must take corrective actions. This gives rise to a regression discontinuity (RD) design where plans around the cutoff are similar in all observables, except that the regulation only disciplines the plans below the cutoff. Meanwhile, I use the 2008 market crash as an exogenous shock that generates large variations in these plans' funded percentage and thus quasi-random assignment to regulatory treatment around the cutoff. My focus is thus on what happens to the withdrawal behaviors following the shock around the cutoff. Under this framework, changes in withdrawal frequency for plans above and below the cutoff in a sufficiently narrow window provide an estimate of the funding rule effect.

My results show that plans subject to funding rule requirements are about 14 percentage points more likely to experience employer withdrawals. The funding rule effects are greater if a plan has a higher proportion of inactive participants (e.g., retirees). For example, consider a plan in which one actively working participant is supporting over two inactive participants. The corrective actions required by the current funding rules would increase the probability of employer withdrawals by 30%. I also take advantage of a temporary funding rule change as a natural experiment. I show that when the funding rules were temporarily switched off in 2009, the withdrawal frequency did not differ significantly for plans around the cutoff.

Next, I examine whether employer withdrawals exacerbate the funding challenge of multiemployer DB pension plans. I find that plans that have already experienced employer withdrawals are more vulnerable to financial shocks. Their funded percentages are significantly lower a few years following the financial crisis in 2008, compared to those without any pre-crisis withdrawal experiences. They struggle to expand employer base, suffer from a rising fraction of inactive participants, and are likely to experience more employer withdrawals in the future.

This paper contributes to a growing body of research on pension risk management in several folds. First, this study adds to the policy discussion on how to prevent future insolvency of multiemployer DB pension plans. Previous literature concerning pension solvency/downside risk often focuses on developing insolvency-based risk measures (Ai et al., 2015), optimal investment strategies (Josa-Fombellida and Rincón-Zapatero, 2004; Lin et al., 2014), and optimal contribution rates (Haberman et al., 2000; Huang and Cairns, 2006). I contribute new insights by evaluating the effects of current funding rules, an important regulatory tool whose purpose is to improve the solvency level of DB pension plans. My empirical analysis indicates that the funding rules have unintended consequences – employer withdrawals from multiemployer pension plans, further deteriorating pension funding levels. Any future policy changes should take this effect into consideration. To the best of my knowledge, this is the first study that establishes the causal link.

Second, this paper relates to a series of studies on corporate response to internal financial constraints. Using mandatory contributions to a DB pension plan as a proxy for changes in internal financial resources, researchers have examined firms' adjustment in capital expenditures and investments (Rauh, 2006), cost of capital (Campbell et al., 2012), and market price reactions (Franzoni and Marin, 2006; Franzoni, 2009). However, little is examined on how pension sponsoring firms, especially of those multiemployer pension plans, respond to corrective actions imposed by regulations. Thus, our study on employer's withdrawal decisions at the plan level complement the literature by filling this gap. This may also be considered as an initial step to fully explore the determinants of employers' withdrawal decisions at the corporate level.

Finally, this paper also contributes to the limited literature on multiemployer DB pension plans. Prior work has concentrated on the benefits and costs arising from its unique operating structure. For example, the economies of scale are well-documented (e.g., Caswell, 1976; Mitchell and Andrews, 1981; Ghilarducci and Terry, 1999). With centralized operation by a joint board of trustees consisting of representatives from both labor unions and employers, lower administrative costs are expected by participating employers. Meanwhile, the pooling of pension risk signifies the spillovers of any unfunded liabilities (Chambers, 2016). In this paper, I specifically investigate the role of employer withdrawals on plan funding challenges and how they exacerbate the financial vulnerability of multiemployer pension plans by external shocks.[1]

The remainder of the paper proceeds as follows. Section 2.2 provides the institutional background of multiemployer DB pension plans, the current funding rules, and how they relate to employer withdrawals. Section 2.3 details the regression discontinuity design and explains the funding rule effect on employer withdrawals. I then characterize multiemployer pension plans by their employer withdrawal experiences and investigate their heterogeneous funding performance after a financial shock in Section 2.4 and conclude the paper in Section 2.5.

2.2 Institutions and Data

This section describes the institutional framework I use for empirical analysis. The data on multiemployer DB pension plans are from U.S. Department of Labor (2020).

2.2.1 Multiemployer DB Pension Plan Funding Rules

In 2006, the Pension Protection Act introduced new funding rules to multiemployer DB pension plans. The act requires that all plans must be classified into

[1]Several reports have already discussed important factors associated with the funding issues of multiemployer pension plans. For example, Munnell et al. (2017) study the characteristics of plans that are projected to run out of money within the next 10 to 15 years. Munnell et al. (2019) assess the effectiveness of using benefit cuts as a tool to forestall plan insolvency.

Table 2.1: Funding Status and Requirements

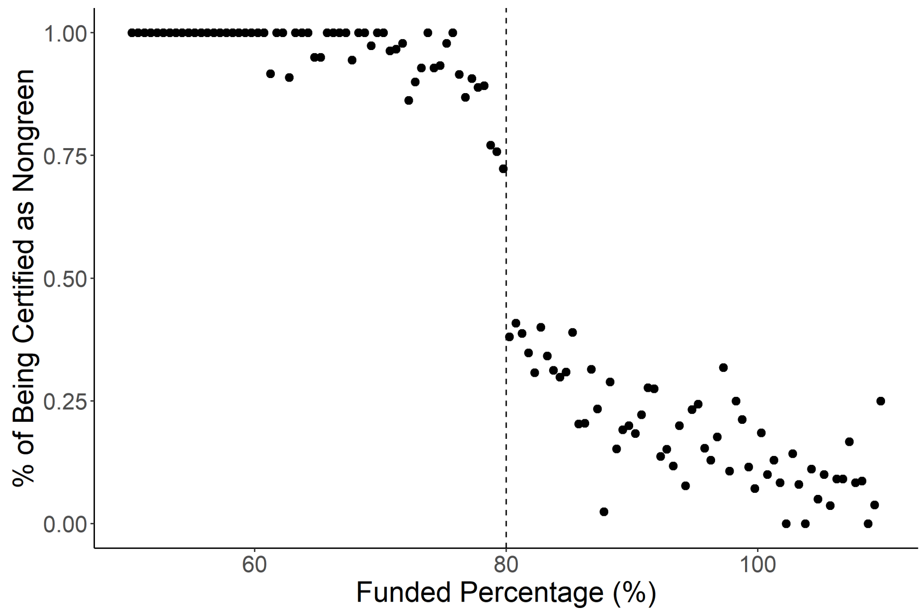
Status	Triggers	Requirements & Permitted Actions
Green Zone	Funded Percentage (FP) $\geq$80% and Funding Deficiency (FD) within 7 years	No improvement actions required
Yellow Zone	Funded Percentage (FP) <80% or/and FD within 7 years	Funding Improvement Plan • Increase contribution rates • Reduce future benefit accruals
Red Zone	FD within 4 years or FD within 10 years and critical last year or FD within 5 years and FP$\leq$65% or Insolvent within 5 years or Insolvent within 7 years and FP<65%	Rehabilitation Plan • Increase contribution rates • Reduce future benefit accruals • Reduce adjustable benefits (not in pay status)

one of the following zone statuses ranked by financial health from good to bad: (1) green zone (also known as "neither critical nor endangered status"); (2) yellow zone ("endangered status"); or (3) red zone ("critical status").[2] A plan's actuary must certify their zone status annually at the beginning of a plan year. A plan certified in yellow/red zone (hereafter, nongreen zone) is required to take corrective actions for improvement. In the 2008 plan year (the first year when the funding rules became effective), 77.5% of all 1,369 multiemployer DB pension plans were identified in the green zone.

Funding zone triggers. The triggers of the funding zone statuses, presented in Table 2.1, are mainly based on three dimensions: funded percentage, time until projected funding deficiency, and time until projected insolvency. According to the funding rule structure, plans with a funded percentage less than 80% are classified as in nongreen zone and, thus, subject to funding improvement requirements.

[2]Prior to PPA, the Multiemployer Pension Plan Amendments Act of 1980 (MPPA) created a "reorganization" index to identify financially troubled multiemployer DB pension plans. These plans are subject to higher minimum funding standards and are required to take actions (e.g., by increasing contribution) to improve their financial conditions. However, the index was rarely triggered. Only four plans were reported to be in reorganization status in the 2009 plan year, whereas 934 plans were identified as in yellow/red zone in the same plan year under the new PPA funding rules (PBGC, 2013).

Figure 2.1: Assignment to Nongreen Zone



Note: Bin size = 0.5 percentage point.

Figure 2.1 depicts reported funding status as a function of a plan's funded percentage since 2008. The x-axis represents the funded percentage, aggregated in 0.5-percentage-point bins. In the y-axis, I calculate the fraction of multiemployer plans certified as in the nongreen zone within each bin. There clearly exists a discontinuity at the 80 funded percentage point. About 72% of plans with a funded percentage between [79.5%, 80%) were certified as in nongreen zone, while the fraction drops to 38% for plans with a funded percentage between [80%, 80.5%). The fact that the funding zone certification is determined by funded percentage in a discontinuous fashion is crucial for my identification strategy (see Section 2.3.1).

The funded percentage used for funding status certification is on an actuarial basis. It is defined as the actuarial value of assets divided by the present value of accrued liability. For the determination of the actuarial value of assets, a plan's enrolled actuary can use either the current value or a value smoothed over up to five years. To discount a plan's accrued liability, the long-run investment rate of return is commonly used. As a result, funded percentages may better reflect a plan's long-run funded level rather than the current funded level. One important consideration is whether there exists opportunistic manipulation of the funded percentage around the cutoff, given the discretion granted to the actuary. I further the discussion in Section 2.3.2.

Employer withdrawal timing and incentives. Following a nongreen zone certification, a plan's board of trustees must develop a set of schedules with proposed improvement actions. The agreement must be renegotiated to reflect the changes during a two-year adoption period when collectively bargained between a union and sponsoring employers. If no agreement is reached, a default schedule is automatically implemented. In the case when a sponsoring employer does not agree to any proposed schedules or the default one, the only option left is to withdraw from the plan.

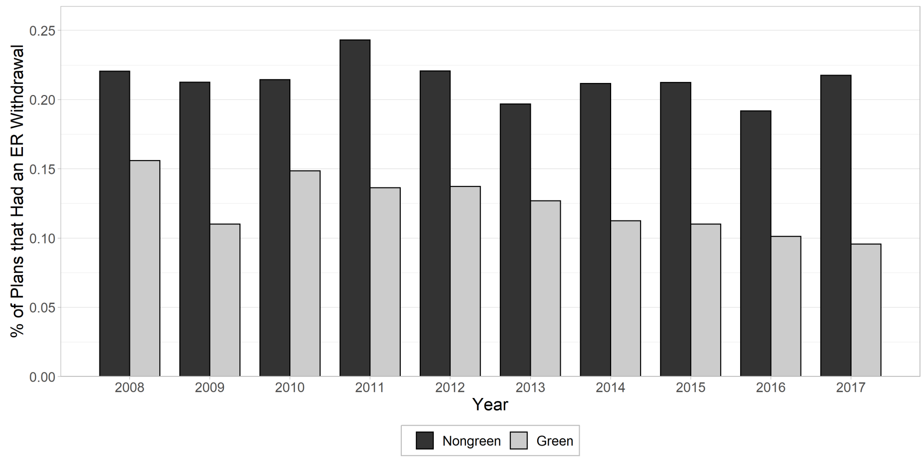
According to the PPA funding rules, there are two possible actions a nongreen zone plan's trustees may propose for funding improvement: contribution increases and benefit reductions. From the perspective of sponsoring employers, both actions are likely to motivate withdrawals. First, the only role of employers is to make contributions to the plan in accordance with the collective bargaining agreements. An increase in contribution rates essentially raises the costs of participation. Those employers who are unable to afford it might be scared away. Second, cutting benefits could threaten an employer's ability to attract and retain employees. Accordingly, employers may exit to maintain their competitive advantages.

In Figure 2.2, I group multiemployer DB pension plans by their zone status and compare the fraction that experienced any employer withdrawals during a plan year. Consistently over the post-2008 periods, I observe that employer withdrawals are significantly more prevalent in plans certified as in nongreen zone.

2.2.2 Financial Crisis and WRERA

The 2008 financial crisis placed unexpected financial distress to multiemployer pension plans. The average funded percentage dropped by 19 percentage point in a single year and the fraction of plans in green zone fell from 76.5% to 31.8% (PBGC,

Figure 2.2: Funding Status and Employer Withdrawals



Note: Figure presents the fraction of multiemployer pension plans that experienced any employer withdrawals during a plan year, by two groups: plans certified as in green zone and in nongreen zone. Data are from U.S. Department of Labor (2020).

2013, 2018). Hence, the financial shock provides a source of variation in plans' funded percentage and funding status that I can use to help identify the funding rule effects (see more in Section 2.3.1).

To provide temporary relief and help pension plans cope with the economic downturn, the Worker, Retiree, and Employer Recovery Act of 2008 (WRERA) was enacted on December 23, 2008. Under WRERA, multiemployer plans can elect to freeze their plan status for the 2009 plan year and remain in the same status as in the previous year. Meanwhile, those already certified as in nongreen zone status in the 2008 plan year can elect to defer their promised improvement progress. A plan's enrolled actuary should still certify a plan's actual funding status without regard to WRERA. This temporary change only applies to the 2009 pan year. Although the funding relief also relies on plans' willingness to engage, the vast majority submitted their WRERA elections to IRS (PBGC, 2013). For example, among the 552 plans who fell from green zone (in 2008) to nongreen zone (in 2009), 96% elected to freeze their green zone status.

The enactment of WRERA thus can be used as a natural experiment when the disciplinary actions from nongreen zone certification were temporarily switched off in

2009. If the disciplinary requirements trigger employer withdrawals, we should expect a significant difference in withdrawal frequency *only* in years when the requirements were in effect but not in 2009, comparing green zone and nongreen zone plans. For direct comparison, I limit my focus to the 2009 and the 2010 plan year, respectively, to study the funding rule effects on employer withdrawals.

2.2.3 Data and Sample

I study plan-level data from Form 5500 Annual Reports of multiemployer DB pension plans (U.S. Department of Labor, 2020). The plan administrator of a multiemployer plan is required under ERISA to disclose their financial and operational information each year via filing Form 5500 and accompanying schedules & attachments. Most of the data used in this paper are from Schedule MB, which contains a plan's actuarial information prepared by the plan's enrolled actuary. Any information associated with employer withdrawals is reported in Schedule R. The 2008 actuarial information from Schedule MB is hand-collected. I merge these data with industry-level information (e.g., total employment, percentage of employed workers who are covered by a collective bargaining agreement) from Hirsch and Macpherson (2021) by a plan's 2-digit NAICS business code.

To construct our sample, I first require that a plan is in the green zone for the 2008 plan year. Note that a plan's 2008 status depends on its financial information in 2007 and earlier, so it's determined before the financial crisis. I focus on these plans to ensure that they are homogeneous in characteristics. Next, I exclude plans that were terminated or merged into other plans between 2008 and 2011. Third, I omit plans that have fewer than two contributing employers during a plan year. These filtering requirements are used out of concern that the decision-making process might differ for participating employers in plans at the verge of termination or those with only one remaining employer. Fourth, I only keep plans that cover at least 100 participants, since the filing standards are different for small pension plans (i.e., with less than 100 participants) (DOL, 2018). For example, they only need to file a simplified version of the financial information schedule. In most cases, multiemployer pension plans comply to the large plan filing requirements. Lastly, a plan must report their actuarial funded percentage and whether they had experienced an employer withdrawal. These filters lead to a sample of 971 plans, accounting for about 71% of all multiemployer pension plans in the 2008 plan year (Table 2.2).

Table 2.2: Data Filtering

Filtering Step	**Remaining Plans**
Total number of active plans in 2008 plan year	1,369
Drop if not in green zone for 2008 plan year	1,061
Drop if terminated or merged into other plans in the next two years	1,022
Drop if contributing employer < 2	994
Drop if participant < 100	987
Drop if 2009/2010 funded percentage or employer withdrawals not reported	971
Full Sample	**971**

The first column of Table 2.3 provides basic descriptive statistics for our full sample as of 2008. On average, these plans had 7,778 number of participants and had been operating for 43 years at the beginning of 2008. With an average dependency ratio of 1.77, it indicates that every 100 active workers were supporting 177 inactive participants (e.g., retirees). Also, on average 35% of these plans had orphan participants, whose employers had already exited from the plan. The mean fraction of assets invested in stocks is 0.32.

The market crash of 2008 imposed a significantly negative impact on the multiemployer pension plans' investment income. 58% (562 out of 971) and 39% (344 out of 971) plans fell into nongreen zone in the 2009 and 2010 plan years, respectively (also see Appendix B Figure B.1). I compare these plans to those who stayed in green zone and conduct simple t-tests for differences in mean. Our key outcome of interest is employer withdrawals. It appears that plans in nongreen zone were more likely to experience employer withdrawals. During the 2009 plan year, 22% of nongreen zone plans had at least one employer exiting from the plan, while only 12% of the green zone plans reported an employer withdrawal. Similarly, the fractions of plans who experienced withdrawals are 0.22 and 0.15 for plans in nongreen and green zones, respectively, during the 2010 plan year.

Plans in nongreen zone also appear to have some distinct characteristics. For example, they are more likely to have existing orphan participants than plans who stayed in green zone. The differences are statistically significant in both the 2009 and 2010 plan years. On the other hand, other plan characteristics (e.g., size, age, number of contributing employers, and percentage of assets invested in stocks) seem very similar.

The summary statistics confirm that nongreen zone certification is correlated with some characteristics that may have their own influence on employer withdrawals, some of which may be unobservable. The funding rule effects thus can be hard to disentangle. In the next section, I propose a regression discontinuity design to address the identification issue.

2.3 Funding Rule Effects on Employer Withdrawals: A RD Design

In this section, I analyze the effect of funding rule requirements on employer withdrawals. The treatment group is nongreen zone plans subject to improvement requirements. More precisely, I look at plans certified in green zone as of the beginning of 2008. These plans were all financially healthy prior to the financial crisis. I then use the financial crisis as a source of variation in these plans' funded levels. By applying a regression discontinuity (RD) design to the years following the financial shock, I establish the causal relationship between improvement requirements from nongreen zone certification and employer withdrawals.

More details regarding the RD design are described in Section 2.3.1. I show the validity of my identifying assumptions in Section 2.3.2 and present my main results in Sections 2.3.3 & 2.3.4.

Table 2.3: Summary Statistics

		2009			2010		
	2008	Green	Nongreen	t-stat	Green	Nongreen	t-stat
Panel A. Plan Characteristics							
Withdrawal	0.16	0.12	0.22	-4.16***	0.15	0.22	-2.34**
	(0.37)	(0.32)	(0.41)		(0.36)	(0.41)	
Participant	7,777.88	6,841.56	8,643.44	-0.77	7,076.21	9,248.22	-0.99
	(33,717.34)	(40,584.24)	(28,751.76)		(35,766.59)	(30,783.64)	
No. of Employers		186.66	214.20	-0.69	185.98	216.40	-0.71
		(602.48)	(635.45)		(580.22)	(663.76)	
Dependency Ratio	1.77	1.82	1.89	-0.38	1.95	2.74	-2.46**
	(2.73)	(3.34)	(2.55)		(3.25)	(5.45)	
I(Orphan)	0.32	0.31	0.39	-2.74***	0.29	0.37	-2.38**
	(0.47)	(0.46)	(0.49)		(0.46)	(0.48)	
Age	42.83	44.04	43.68	0.59	44.49	45.45	-1.58
	(9.41)	(9.65)	(9.23)		(9.82)	(8.59)	
Stock%	0.32	0.26	0.28	-1.38	0.29	0.28	0.57
	(0.22)	(0.19)	(0.19)		(0.21)	(0.21)	
Panel B. Industry Characteristics							
Employment (000)	7,861.29	7,314.672	7,638.37	-2.11**	6,794.57	7,058.68	-1.60
	(2,511.66)	(2,203.93)	(2,559.16)		(2,121.16)	(2,620.89)	
Union Covered	0.11	0.11	0.12	-0.29	0.11	0.11	-0.24
Employment%	(0.07)	(0.08)	(0.08)		(0.08)	(0.08)	
No. of Plans	971	409	562		627	344	

Note: FP represents a plan's actuarial funded percentage used for funding status certification. Values in the first and second rows for each variable are means and standard deviations (in parentheses). In Panel A, all variables except *Withdrawal* are values at the beginning of a plan year. The variable *Withdrawal* indicates whether a plan experienced any employer withdrawals during a plan year. The 2008 Form 5500 did not require plans to report their number of contributing employers. In Panel B, values at industry level are from the previous year. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

2.3.1 Empirical Methodology

I start with a simple approach by regressing employer withdrawals against nongreen zone certification as:

$$\text{Withdrawal}_i = \beta_0 + \beta_1 \text{Nongreen}_i + \mathbf{X_i}\delta + \eta_i, \tag{2.1}$$

where i indexes plans; Withdrawal_i is a binary indicator for a plan i experiencing employer withdrawals during a plan year; Nongreen_i represents the treatment, defined as a binary indicator for a plan i being certified as nongreen zone status and required to improve their funding status; $\mathbf{X_i}$ is a set of plan-level and industry-level controls. The plan-level control variables, valued at the beginning of the plan year, include the plan's number of participants, number of contributing employers, inactive-to-active participant ratio, age, whether the plan had any orphan participants, and the proportion of assets invested in stocks. The industry-level control variables include prior-year total employment and the percentage of employed workers who are covered by a collective bargaining agreement in the plan's industry. Regressions report robust standard errors clustered by industry.

My coefficient of interest, β_1, can be interpreted as the average treatment effect of funding rule requirements on the probability of employer withdrawals during a plan year. Yet, the estimates of β_1 might be biased if there exist unobserved characteristics (not captured by $\mathbf{X_i}$) that influence both employer withdrawals and a plan's funding status. As a result, the error term η_i might be correlated with the treatment, Nongreen_i.

To address the potential identification problem, I employ a regression discontinuity (RD) design by exploiting the discontinuity at the cutoff of 80% funded percentage for a plan's funding status certification. As previously mentioned, a multiemployer pension plan with less than 80% funded percentage must be certified as in nongreen zone status and adopt corrective actions to improve their funded levels. The financial crisis generates a wide range of variations in plans' funded percentages, which leads to a quasi-random assignment of funding rule requirements near the cutoff following the shock. To the extent that plans around the cutoff are similar along all dimensions but only differ by their certified funding zone status, I am able to draw causal inference regarding whether funding rule requirements trigger employer withdrawals. I provide further evidence in the next section 2.3.2 that plans do not self-sort into one side of the cutoff or the other and that plan characteristics other than funding zone status stay balanced at the cutoff.

I propose a *fuzzy* regression discontinuity (FRD) design (Hahn et al., 2001), considering that funded percentage less than 80% does not perfectly predict our treatment – a nongreen zone plan (see Figure 2.1). I do not observe a perfectly sharp separation into treatment at the 80 funded percentage point mainly for two reasons. First, funding status is not solely determined by a plan's funded percentage, as mentioned in Section 2.2. There also exist other criteria, including number of years for a plan to occur funding deficiency and to be insolvent, which are not observed in our data. These other criteria mostly explain observed treatment on the "wrong" side of the cutoff – those nongreen zone plans with a funded percentage greater than 80%. Second, a plan's actuary usually uses projected and unaudited funded percentage to certify a plan's funding status, in compliance to PPA that an annual actuarial certification must be submitted to IRS at the beginning of a plan year. The funded percentages reported on Schedule MB of Form 5500, however, are filed after a plan year ends and, most importantly, audited by an independent qualified public accountant. Therefore, there exist instances when a plan's actuary certifies with unaudited funded percentage that the plan's funding status is green at the beginning of the plan year, but the audited funded percentage on Form 5500 turns out less than 80 percentage point. I observe 42 such cases for the 2009 and 2010 plan years in total.

The estimation of an FRD design is equivalent to a two-stage least square (2SLS) regression framework (Cook, 2008) using the discontinuity as an instrument (IV) for withdrawal analysis in Eq. (2.1). Specifically, I estimate the first-stage regression as:

$$\text{Nongreen}_i = \gamma_0 + \gamma_1 I(\text{FP}_i < 0.8) + \gamma_2 f(\text{FP}_i) + \mathbf{X_i}\Omega + \varepsilon_i, \tag{2.2}$$

where

$$\begin{aligned} f(\text{FP}_i) = {} & \psi_1(\text{FP}_i - 0.8) + \psi_2(\text{FP}_i - 0.8) \times I(\text{FP}_i < 0.8) \\ & + \psi_3(\text{FP}_i - 0.8)^2 + \psi_4(\text{FP}_i - 0.8)^2 \times I(\text{FP}_i < 0.8); \end{aligned} \tag{2.3}$$

FP_i represents the running variable – the plan i's actuarial funded percentage used for funding zone certification; $I(\text{FP}_i < 80\%)$ captures the discontinuity, indicating whether a plan's funded percentage is under 80%; $f(\text{FP}_i)$ represents the functional form that delineates the relationship between funded percentage and nongreen zone certification. Given the nonlinear relationship as displayed in Figure 2.1, I adopt second-order polynomials of funded percentage as the preferred specification of $f()$ as in Eq. (2.3). Simply put, I allow the polynomial coefficients to vary below and above the cutoff.

I apply the FRD design and compare these plans' withdrawal frequencies in 2009 and 2010, separately. Under WRERA, the trustees of the plans in our sample had the option to "freeze" their 2008 funding status and elect to stay in the green zone no matter what status they were actually certified for 2009. Among the 552 plans certified as in nongreen zone in 2009, 529 elected to freeze their green status from the previous year (PBGC, 2013). Thus, if the funding rules caused more employer withdrawals, I would expect significant differences in probability of withdrawals only in the 2010 plan year but not in the 2009 plan year, comparing plans just below and above the cutoff.

2.3.2 Internal Validity of RD Design

One important identifying assumption of an RD design is that the plan trustees are unable to *precisely* manipulate a plan's funded percentage. Particularly, if a plan's trustees can self-sort into the green zone by manipulating its funded percentage above the cutoff, the assignment to the treatment group around the cutoff would not be considered random. The causal inference of my estimation would be undermined.

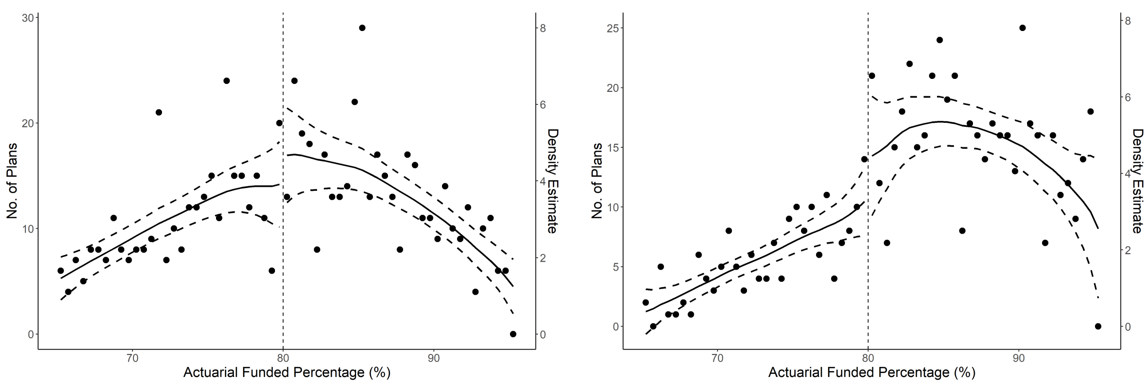
To validate this assumption, I perform formal tests to verify that there is no evidence of sorting to the treatment. If the manipulation exists, I should expect significantly more plans clustered just to the right of 80% funded percentage and a jump in the density of the running variable. In Figure 2.3, I plot the density function of the running variable – a plan's funded percentage against the number of plans in

0.5-percentage-point bins. The density function appears smooth in both the 2009 and 2010 plan years. McCrary's density tests also confirm that there does not exist statistically significant discontinuity in the density of funded percentage at the cutoff (McCrary, 2008).

Figure 2.3: McCrary Tests

Panel A: 2009 Plan Year

Panel B: 2010 Plan Year



Note: Figures plot number of multiemployer pension plans (in dots) in bins of actuarial funded percentages (running variable). Bin size = 0.5 percentage point. The curve plots local linear density estimator and its 95% confidence interval following McCrary (2008).

Second, a valid RD design also requires that plans with a funded percentage on either side of the 80% cutoff have balanced baseline covariates as an indication of local randomization of the treatment. Our first round of sample screening as specified in Table 2.2 only keeps plans in green zone at the beginning of 2008, which ensures the financial homogeneity of these plans – they were all financially healthy prior to the 2008 financial crisis. Next, I compare the sample means for observable covariates below- and above-cutoff across 30-percentage-point windows (i.e., [0.5, 0.8) vs. [0.8, 1.1], see Appendix B Table B.1) and conduct formal RD estimate testing for the discontinuity around the cutoff. Specifically, each covariate is regressed on the indicator whether a plan's funded percentage is under 80% (the discontinuity variable), second-order polynomials of funded percentage, and their interactions.[3] A

[3] I run the following regression: $y_i = \alpha I(\mathrm{FP} < 0.8) + \theta_1(\mathrm{FP}_i - 0.8) + \theta_2(\mathrm{FP}_i - 0.8) \times I(\mathrm{FP}_i < 0.8) + \theta_3(\mathrm{FP}_i - 0.8)^2 + \theta_4(\mathrm{FP}_i - 0.8)^2 \times I(\mathrm{FP}_i < 0.8) + \nu_i$, where y_i represents each covariate and FP represents a plan's actuarial funded percentage. Regressions are clustered by industry.

statistically insignificant estimate on the discontinuity variable would indicate that the covariate evolves smoothly over the cutoff. In Columns 3 and 6 of Appendix B Table B.1, I report the p-values of the discontinuity estimators (α) for the 2009 and 2010 plan years, respectively. In general, I find no evidence of discontinuity for each covariate near the cutoff.[4] Figures B.2 and B.3 of Appendix B also provide graphical evidence for the lack of discontinuities in the baseline covariates.

I further implement placebo tests to ascertain that the observed discontinuity for nongreen zone assignment is indeed driven by the 80% threshold under the current funding rule structure. That is, I replace the 80% funded percentage cutoff with a series of "false" cutoffs over the range between 75% and 85% and estimate γ_1 as in Eq. (2.2). As shown in Appendix B Figure B.4, for both the 2009 and 2010 plan years, none of the coefficients are statistically different from zero at a 95% confidence level except the actual cutoff of 80%.

In summary, by showing that plan trustees did not self-sort into their preferred funding zones, that the discontinuity for nongreen zone certification is the result of the funding rule cutoff, and that all baseline covariates stay balanced on either side of 80% funded percentage, I find supporting evidence that my FRD design represents a quasi-random assignment of funding rule requirements around the cutoff and any observed difference in employer withdrawals is caused by the treatment.

2.3.3 Main Results

Table 2.4 presents our estimation results by two different models: ordinary least squares (OLS) and 2SLS under our FRD design. Columns 1-4 and Columns 5-8 show the estimation applied to the 2009 and 2010 plan years, respectively. Recall that in the 2009 plan year the multiemployer pension plans in our sample were allowed to

[4] I do find that the discontinuity estimators on the 2010 dependency ratio and the percentage of union covered employment, respectively, are marginally significant. However, it is possible that the observed significance is random due to multiple testing problems (Lee and Lemieux, 2010). To address this, I conduct a Seemingly Unrelated Regression (SUR) by combining each equation with different baseline covariates as dependent variables and perform a single χ^2 testing for all α being zeros. For both the 2009 and 2010 plan years, I fail to reject the null hypothesis that all covariates evolve continuously across the cutoff.

freeze their green zone status because of the enactment of WRERA and, therefore, the funding rules were temporarily switched off. Differently, in 2010 the funding rule requirements were back in effect and any plans certified as in nongreen zone must take corrective actions by increasing contribution, cutting benefits, or both.

In Columns 1 and 5, I begin with naive OLS regressions without any controls. The estimates suggest that plans with a certification of nongreen zone are more likely to experience employer withdrawals during the plan year. However, after controlling for plan and industry characteristics, the significance disappears for both the 2009 and 2010 plan years, as in Columns 2 and 6. The size of the estimates also largely reduces. This implies that the OLS estimates are likely to be biased if there still exist unobserved characteristics that influence both nongreen zone certification and employer withdrawals and that I am unable to control for.

Our FRD design helps eliminate the endogeneity problem. Columns 3-4 and 5-6 present the IV estimates under the FRD design. I show that without the enforcement of any funding rule requirements in the 2009 plan year, there are no significant differences in probability of employer withdrawals for green zone plans and nongreen zone plans (Columns 3-4). However, when the funding rule was in effect in the 2010 plan year, being certified as nongreen zone status increases the probability of a plan experiencing employer withdrawals by 14.0% (20.6%) with (without) controls (Columns 7-8).

Robustness Tests. For our main results, I focus on the one-year withdrawal frequency as it captures a cleaner effect of the nongreen zone determination from the same plan year. However, it is likely that the contributing employers are still in the collective bargaining process and any withdrawal decisions are not made until a year later, which may explain the lack of significant results in the 2009 plan year. Given that the adoption periods of a funding improvement plan commonly last for two years, I replicate our main analysis on the two-year withdrawal frequency of a plan (see Appendix B Table B.2). For plans certified as in nongreen zone at the beginning of the 2009 plan year, the probability of experiencing employer withdrawals during a

Table 2.4: Estimation Results by Models

	2009				2010			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nongreen	0.089**	0.041	0.059	−0.016	0.073**	0.014	0.206*	0.140**
	(0.044)	(0.025)	(0.292)	(0.235)	(0.032)	(0.023)	(0.111)	(0.069)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Cluster by Industry	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	971	971	971	971	971	971	971	971

Note: Dependent variable is a dummy variable indicating whether a multiemployer pension plan had experienced employer withdrawals within the plan year. Columns 1-2 and 5-6 present ordinary least squares (OLS) coefficient estimates; Columns 3-4 and 7-8 present present the two-stage least square (2SLS) instrumental variable regression coefficient estimates. Our preferred model is in Columns 4 and 8, in which I also control for plan-level and industry-level characteristics. Regressions report robust standard errors clustered by industry. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

two-year period is still not significantly different from that of the green zone plans. Differently, for the 2010 plan year, the estimated coefficients on the nongreen zone certification remain statistically significant and become slightly larger – they are 17 percentage points more likely to experience withdrawals within two years. Together, I rule out the possibility that the observed differences between these two plan years are the results of delayed withdrawal responses.

For robustness check, I also apply 2SLS regressions to samples with different bandwidths around the cutoff and report the results in Table B.3 of Appendix B. For example, the $+/-5$ samples in Columns (9) & (10) only include plans with a funded percentage between 75% and 85%, a 5 percentage point on each side of the cutoff. The results remain largely consistent regardless of the bandwidth selections for both the 2009 and 2010 plan years. As one might expect, the coefficient estimates and standard errors become larger as the bandwidth gets narrower, suggesting the estimates are less precise because of restricted sample size.

Moreover, our results are robust to different estimation specifications. One concern of parametric regressions is that the estimators might be biased if an incorrect functional form is specified (Lee and Lemieux, 2010). I use a linear functional form in Eq. (2.2) as robustness check. I also apply nonparametric local linear regressions to our samples with a triangular kernel. That is, higher weights are assigned to plans with a funded percentage closer to the cutoff. Under both specifications, the estimates stay qualitatively similar.

Taken together, I find significant impact of funding rule requirements on participating employers' decisions to withdraw from a multiemployer pension plan. In the absence of any funding improvement requirements for nongreen zone plans in 2009 due to WRERA, I do not observe any significant differences in probability of employer withdrawals. However, when the funding rule requirements came back effective in 2010, nongreen zone plans were about 14 percentage points more likely to experience employer withdrawals.

2.3.4 Characterize Funding Rule Effects

In this section, I investigate how the funding rule effects on employer withdrawals differ based on plan's characteristics. In particular, I test whether plans with high dependency ratios are more likely to experience employer withdrawals under the funding improvement requirements. The dependency ratio, defined as the inactive-to-active participant ratio, reflects a plan's reliance on current active workers. I hypothesize that nongreen zone plans with a high dependency ratio need to put more pressure on participating employers by, for example, imposing higher contribution increases per active worker or/and deeper benefit cuts.[5] Otherwise, they would not be able to make promised progress under the funding rule requirements. As a result, employers may be more likely to exit from these plans.

Therefore, I use the interaction between nongreen zone certification and the logged value of dependency ratio as a proxy for the possible *intensity* of required improvement

[5] I manually reviewed the proposed schedules under a funding improvement plan or a rehabilitation plan of 2010 nongreen zone plans. On average, the maximum proposed contribution increase is 91% for nongreen zone plans with a dependency ratio in the top quintile ($\geq$2.3) and 43% for plans with a dependency ratio <2.3. The difference is statistically significant at 5% confidence level.

actions and the burden placed on participating employers. I estimate the following regression:

$$\text{Withdrawal}_i = \beta_0 + \beta_1 \text{Nongreen}_i + \beta_2 \text{Nongreen}_i \times log(\text{DependencyRatio}_i) + \mathbf{X_i}\delta + \eta_i. \tag{2.4}$$

where I(FP$_i$ <0.8) and I(FP$_i$ <0.8)$\times log$(DependencyRatio$_i$) are used as two instruments for Nongreen$_i$ and Nongreen$_i$ $\times$ log(DependencyRatio$_i$), respectively. The coefficient of interest is β_2.

Table 2.5: Interaction Between Nongreen Zone and Dependency Ratio

	Withdrawal	
	(1)	(2)
Nongreen	0.115*	0.120*
	(0.064)	(0.068)
Nongreen × log(Dependency Ratio)	0.140***	
	(0.054)	
Nongreen × I(High Dependency Ratio)		0.178**
		(0.074)
Controls	Yes	Yes
Cluster by Industry	Yes	Yes
N	971	971

Note: Table reports 2SLS instrumental variable estimates of the treatment effect ("Nongreen") and its interaction with a plan's dependency ratio level. Dependency ratio is defined as the ratio of inactive participants to active participants. I(High dependency ratio) is an indicator for plans with a dependency ratio in the top quintile ($\geq$2.3). Regressions report robust standard errors clustered by industry. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

In Table 2.5, I present the instrumental variable estimation results of Eq. (2.4). I find that when the funding rules were in effect in the 2010 plan year, nongreen zone plans with a higher dependency ratio were significantly more likely to experience employer withdrawals within a year. Specifically, the withdrawal probability increases by 1.4 percentage points for every 10% increase in dependency ratio (Column 1). In Column 2, I examine nongreen zone plans with a dependency ratio in the top quintile ($\geq$2.3), which further clarifies the effect. Their probability of experiencing withdrawals are 17.8 percentage points higher than other nongreen zone plans.

2.3.5 Sorting in Post-2011 Periods

Starting from the 2011 plan year, I observe a jump in the density function of our running variable, funded percentage, at the cutoff of 80% (See Appendix B Figure B.5). This suggests sorting into the treatment for plans in our sample. A McCrary's test also confirms that the discontinuity is statistically significant. For this reason, I do not apply our FRD design to the sample periods after 2011.

There are two possible explanations for the observed sorting behavior. First, since the nongreen plans in our sample are *required* to improve their funding levels starting from 2010, this creates asymmetric incentives where plans with a funded percentage below the cutoff are collectively moving upwards, while those above the cutoff are not. Naturally, it is likely to see a jump in the density of funded percentage just to the right of the cutoff. Second, I observe cases, although rare, when a plan's enrolled actuary switched to favorable actuarial assumptions for a higher funded percentage. For example, the actuaries are permitted discretion to use either the current value or a value smoothed over up to five years for the determination of assets. From 2011 to 2018, I find 68 cases in our sample where a favorable asset value was used by switching the methods, which suggests a certain degree of manipulation. This number only represents a lower bound since I am not able to further tell from the data whether a plan changed the number of years to smooth when a smoothing method is used.

2.4 Employer Withdrawals and Financial Vulnerability

In this section, I investigate whether employer withdrawals exacerbate the funding challenge of multiemployer DB pension plans. In particular, I look at how *ex ante* differences in withdrawal experiences relate to differential responses in funding performance following the 2008 financial crisis.

Employer withdrawals may influence a plan's financial resilience for several reasons. Typically when an unforeseen financial shock hits a plan, the sponsoring employers and their workers must fill the gap of any funding shortfalls. Employer withdrawals reduce the size of employer base that is available to absorb the costs (see, e.g., New York Times, 2014b). In the case when an employer is in bankruptcy and forced to withdraw from an *underfunded* plan, the plan must make up the withdrawn employer's share of unfunded liabilities. The pressure placed on the remaining employers can be even higher. If the plan chooses to take on riskier investing strategies to fund the gap, an event like the financial crisis may lead to more severe funding shortfalls.

Therefore, I argue that irrespective of the mechanism at work, multiemployer plans that had employers withdraw before a shock are more susceptible to unexpected funding shortfalls. The financial crisis of 2008 provides us a setting for testing. Specifically, I pick out the financially healthy plans (in green zone) *before* the financial crisis and group them into two categories: plans with orphans and plans without orphans. Plans with (without) orphan represent those who had (had not) experienced employer withdrawals prior to the market crash. I compare their changes in funded percentage following the crisis in the post-2008 periods and estimate:

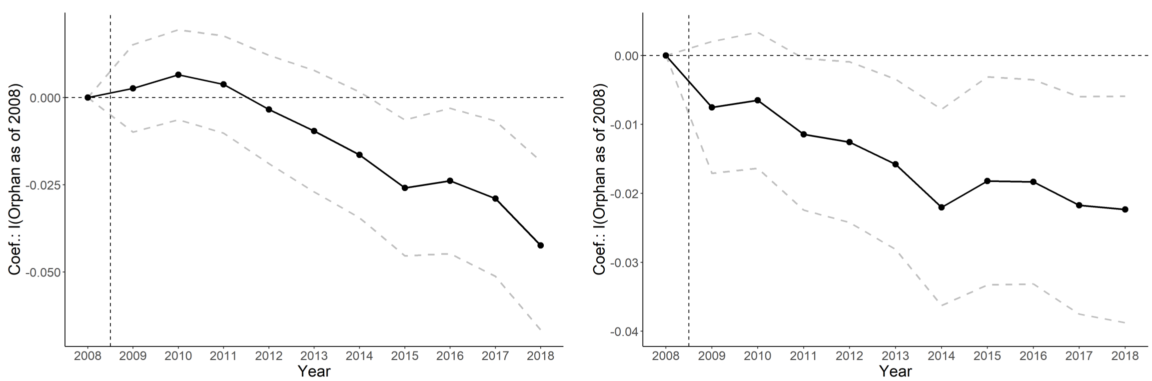
$$\begin{aligned}\text{Funded Percentage}_{it} = \alpha_0 + \sum_t \alpha_{1t}\, \mathrm{I}_t(\text{Year}) \times \mathrm{I}_i(\text{Orphan}_{2008}) \\ + \alpha_2 X_{i,t-1} + \mathrm{FE}_i + \mathrm{FE}_t + \varepsilon_{it},\end{aligned} \tag{2.5}$$

where i indexes plans and t indexes time. $\mathrm{I}_i(\text{Orphan}_{2008})$ is an indicator for plans that already had orphan participants at the beginning of 2008 prior to the financial crisis. My coefficients of interest, α_{1t}, capture the average change in funded percentage in year t from the beginning of 2008 for plans with orphans relative to those without orphans. I also control for plan characteristics at $t-1$, including logged participant, logged dependency ratio, percentage of assets invested in stocks, expense-to-asset ratio, and logged value of plan age. The detailed results are reported in Appendix B Table B.4.

Figure 2.4 plots α_{1t} over time by looking at the actuarial funded percentage and current funded percentage, respectively. As mentioned earlier, actuarial funded percentage reflects a plan's expected long-run financial health, while current funded percentage reflects the current financial health at the beginning of each plan year.[6] I observe that both funded percentages of plans with orphans stayed worse than plans without orphans in the post-crisis periods, which suggests less ability to recover from the shock. Ten years after the crisis in 2018, their actuarial and current funded percentage are significantly lower by 4.2% and 2.2%, respectively, compared to plans without orphans.

Figure 2.4: Evolution of Funding for Plans with Orphan Participants

Panel A: Actuarial Funded Percentage　　Panel B: Current Funded Percentage



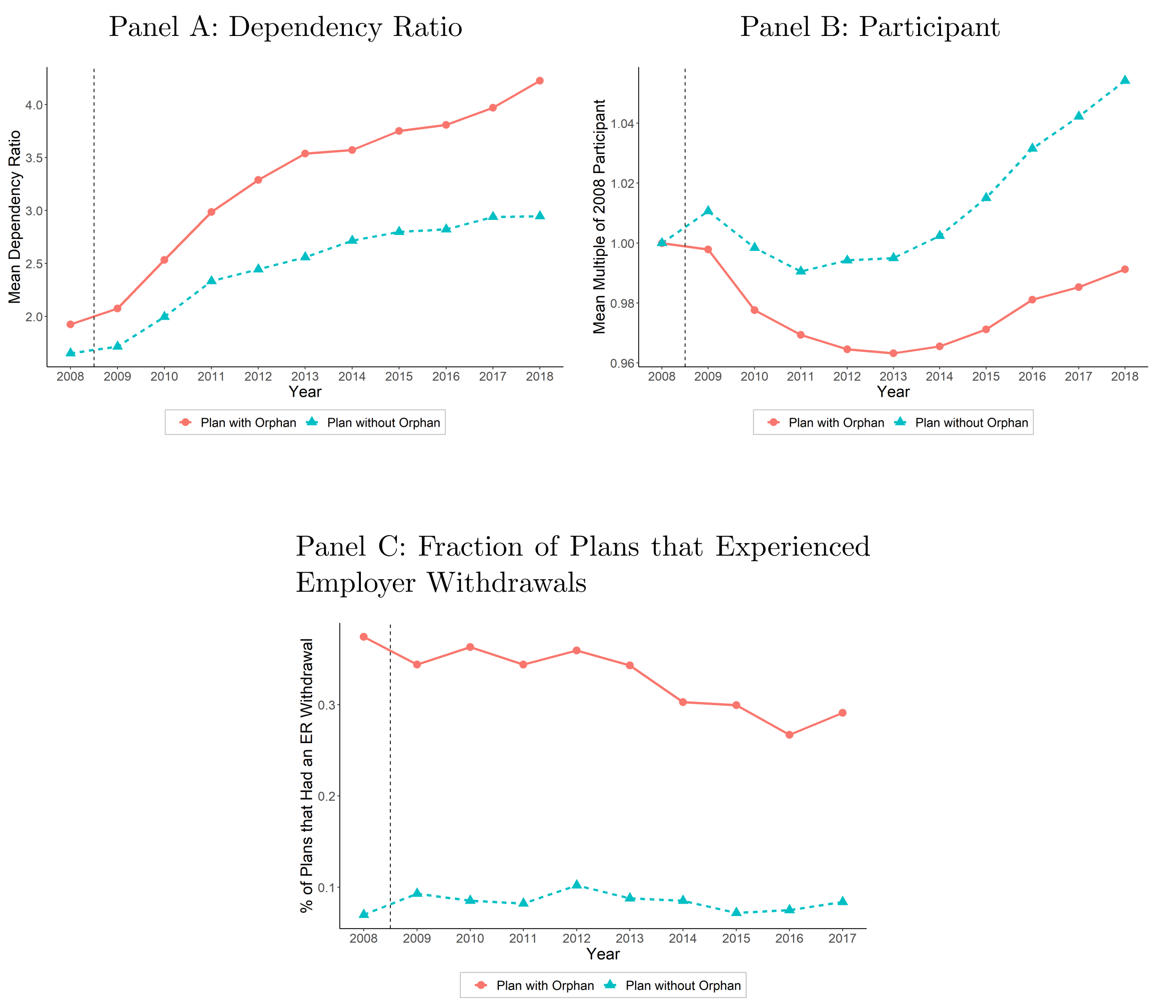
Note: Figures plot 95% confidence interval of event study coefficients (i.e., α_{1t} in Eq. (2.5)). The coefficients capture the average change in funded percentage from the beginning of 2008 for plans with orphans relative to plans without orphans. The vertical line marks the time when financial crisis occurred in September 2008.

Other outcomes of employer withdrawals consist of rising dependency ratio, shrinking employer base, and even more employer withdrawals in the future. In Figure 2.5, I show that the mean dependency ratio for plans with orphans is more than doubled from 1.9 to 4.2. Also, they seem to struggle with attracting new employers since on average their total number of participants has stayed the same or become even lower. Differently, the mean dependency ratio of plans without orphans increased in a slower

[6]Current funded percentage is defined as the market value of assets divided by the current liability. The interest rate used to discount the current liability is based on 30-year Treasury securities (instead of a plan's investment rate of return).

speed, and I observe a modest increase (i.e., 5%) in the number of participants from 2008 to 2018. Last but not the least, it is disturbing to see that plans with existing orphan participants at the beginning of 2008 are significantly more likely to experience additional withdrawals. In Panel C, I show that around 30% of these plans experienced employer withdrawals each year from 2008 to 2018, while the fraction is consistently lower at 10% for plans with no existing orphan participants.

Figure 2.5: Plans with Orphans v.s. without Orphans as of 2008



Note: Figures compare characteristics by two groups: plans *with* orphan participants and plans *without* orphan participants at the beginning of 2008. Dependency ratio is defined as inactive-to-active participant ratio. In Panel B, the participant count of each plan in each year is scaled by its 2008 participant count, to facilitate the comparison. On average, plans *with* orphans are comparatively larger. The mean number of participants in 2008 is 14,634 for plans *with* orphans and 4,522 for plans *without* orphans, respectively.

Overall, the consequences of employer withdrawals are alarming. Plans that have experienced employer withdrawals before are more vulnerable to financial shocks. It is harder for them to expand employer base. Thus, the administrative costs are shared with less participants, and the larger economies of scale relative to single-employer plans are not fully functional (see the comparison of economies of scale in Appendix B Table B.5). The rising dependency ratio implies that one active worker will need to support more inactive participants in the event of an unforeseen funding shortfall. It is also worrisome that plans that had experienced employer withdrawals before are more likely to experience withdrawals in the future because this creates a snowball effect that jeopardizes the sustainability of these pension plans.

2.5 Discussion and Conclusion

In this paper, I investigate whether the funding rules of PPA trigger employer withdrawals from multiemplyoer DB pension plans and how withdrawals impact pension plans' financial health. Since the 2008 financial crisis, the average funding ratio of underfunded multiemployer pension plans has been steadily declining, even when the financial market has fully recovered and multiple corrective actions were imposed to improve funding status as required by the funding rules of PPA. I find that employer withdrawals play an important role in explaining this puzzle. Based on a regression discontinuity design at the 80% funding cutoff, together with an exogenous shock from the 2008 financial crisis to guarantee a quasi-random experiment, I demonstrate that plans subject to funding rule requirements are 14 percentage point more likely to experience an employer withdrawal during a plan year. Moreover, I provide evidence that plans who have experienced employer withdrawals are more vulnerable to unexpected financial shocks. Plans with existing orphan participants consistently have higher future employer withdrawals and lower funded percentages until the end of our sample, ten years after the financial crisis.

This study has several important policy implications. First, while the objective of funding rules is to restore financial viability of troubled multiemployer pension plans, policy makers should also pay attention to the unintended consequences, i.e., more employer withdrawals, of regulatory interventions. In the ongoing wave of funding rule discussions, a series of stricter funding rules have been proposed for the purpose of preventing future plan insolvencies (CRS, 2020). I point out that any modifications to the current funding rules should be accompanied with measures to prevent employer withdrawals. For example, requiring lower discount rates will naturally lead to more multiemployer pension plans being classified as in nongreen zone and therefore, trigger more employers leaving from troubled plans. Imposing certain limitations on withdrawal together with the implementation of such funding rule requirements may significantly improve the outcome of corrective actions.

Second, it is critical to fairly assess the withdrawal liability and make sure that the orphan participants will not impose extra burdens on remaining contributing employers. The current withdrawal liability is assessed based on the funded percentage *at the time* of withdrawal, without taking into consideration the uncertainties of various involved risks and possible future funding shortfalls. This partly explains why plans who have experienced employer withdrawals are more susceptible to unforeseen financial shocks. I believe that, instead of the static approach, a risk-adjusted method for evaluating withdrawal liability is necessary and can help alleviate the orphan participant risks. Market-consistent valuations of assets and liabilities have been essential components of modern insurance regulations, such as Solvency II. Moreover, stricter withdrawal rules to completely remove orphan participant risks are also possible. For example, if employers can be responsible for the risk transfer costs for their portion of pension liabilities upon withdrawal, then the orphan participants' pension risks can be fully transferred to an annuity provider through pension buyins and buyouts (Lin et al., 2017).

Finally, it would be desirable to impose a more stringent oversight on plans with funded percentage close to the 80% cutoff. I find potential cases where favorable actuarial assumptions were switched between years by a plan's enrolled actuary to inflate their funded percentage and consequently avoid corrective actions, although actuaries are permitted a certain degree of discretion to do so. A closer monitoring of plans with funded percentage near the 80% cutoff may help deter any motivations of manipulation and urge plans to take actions promptly before getting worse.

While I examine the casual inferences between funding rules and employer withdrawals at the plan level, it would be interesting to further investigate the withdrawal behavior at a more granular level. For instance, it would be insightful to study withdrawal decisions from an employer' perspective. By specifically looking at a firm's financial position and internal resources, I might be able to capture more nuances regarding the drivers of a firm's withdrawal decisions and propose options at the firm level to deter withdrawals from multiemployer pension plans. Another important topic would be to take a closer look at the underfunding determinants for multiemployer pension plans and perform a comparative study of the funding levels between single-employer and multiemployer pension plans. A better understanding of these relationships could significantly help us tackle the funding challenge faced by multiemployer pension plans. I leave these for future research.

CHAPTER 3
DOES ONE SHOCK AFFECT ALL: EVIDENCE FROM PROPERTY/CASUALTY INSURANCE INDUSTRY

3.1 Introduction

The traditional view of insurance pricing is all about underwriting and insurers' exposure to unanticipated losses. A large set of literature on the topic has revolved around the pricing response to adjustment in expected costs (e.g., Lai et al., 2000), financing frictions (e.g., Ge, 2020), and demand elasticity (e.g., Cummins and Danzon, 1997), following an event that directly affects an insurer's existing loss portfolios. However, we are lack of understanding of how loss shocks influence *unexposed* insurers.

Indeed, insurance pricing is essentially a financing decision. An insurer should incorporate not only its own financial needs, but also the actions of other market participants into their rate-setting process (see, e.g., Benoit, 1984). A firm's position in relation to its rivals may have important implications for its product performance in the market (Campello, 2006; Fresard, 2010). The failure of competitors can even turn into a blessing and increase the market value of unaffected parties (Lang and Stulz, 1992; Egginton et al., 2010).

In this paper, I propose that there exists a competitive dimension in insurance pricing decisions. Following a loss shock, the change in market participants' relative financial position may also affect the pricing performance of *unexposed* insurers. In particular, I firstly develop a simple theoretical model where two insurers compete in a product market. Assuming that customer demand is sensitive to insurers' default risk, I predict that rivals' loss shocks may create competitive advantage for unaffected insurers and allow them to charge a higher price for being in a better financial position.

To test my prediction, I study the U.S. property/casualty insurance industry and exploit the variation of market composition in each individual state. I estimate how insurers who *only* write personal lines change insurance prices in response to commercial-line loss shocks that affect their rivals. Here, any loss shocks from commercial lines are unlikely to directly affect the pricing decisions of personal-only writers other than through product market dynamics. More precisely, I look at their price levels *relative* to market rivals, instead of the *absolute* levels. The interpretation of my results can thus be safely attributed to competitive effects of loss shocks rather than unspecified effects of industry capital capacity constraint. I find that insurance prices are relatively higher for personal-only writers than their adversely affected rivals following rivals' commercial loss shocks, suggesting that a firm benefits from the downfall of enemies.

However, there may exist omitted variables that explain the observed pricing response from unexposed insurers who only write personal lines. For example, both commercial and personal lines may experience large losses following the same catastrophe. The commercial-line loss shocks could pick up the effect of this potential co-movement. Also, the conventional measure of loss shocks – loss reserve development from previously issued policies (see, e.g., Cummins and Danzon, 1997) – may contain measurement errors. An insurer could over-reserve or under-reserve for other purposes (Grace and Leverty, 2012). To mitigate the concern, I exploit a regulatory change that creates an exogenous *decrease* in insurance losses from commercial liability lines – the enactment of noneconomic damages caps. By limiting the size of awards from pain and suffering a plaintiff may receive, the reform lowers the insurance costs of a commercial-liability-line writer. The instrumental variable estimates provide strong evidence that unaffected insurers respond to rivals' loss shocks based on their relative financial positions. A one-standard deviation increase in rivals' commercial loss shocks allows personal-only writers to charge a price that is 15% higher.

Also, I take advantage of the variation in rate regulation systems at different states and explore how the effects of rivals' loss shocks differ across markets of different competitive environments. In particular, I expect that insurers in states with non-stringent rate regulation are more sensitive to their rivals' competitive position. In these states, insurers are allowed to set insurance rates without state regulators' approval and can adjust their pricing strategies more freely. As expected, I observe that the competitive effects of rivals' commercial loss shock are significant higher in states not stringently regulated.

The contribution of this paper is threefold. First, this paper contributes to the extensive literature on insurance pricing. The existing theories already provide well-established frameworks to explain how price reacts to market conditions. For example, the probability updating theory predicts that insurance prices might be affected if both policyholders and insurers update their loss expectations following catastrophic events such as hurricanes (Lai et al., 2000). The risky debt theory provides evidence of the inverse relationship between price and the financial quality of an insurer (Cummins and Danzon, 1997). The capacity constraint theory hypothesizes that when the supply of capital in the market is excessive, insurers can charge lower prices; when the supply of capital is insufficient, insurers tend to charge higher prices (Winter, 1994; Gron, 1994). This paper draws upon them and makes progress by adding a new perspective that the competitive dimension of insurance pricing also exists. By looking at loss shocks that only affect rivals, I can isolate the competitive effect of loss shocks from other forces, such as the internal capital market (Ge, 2020; Giroud and Mueller, 2019). Additionally, this paper is one of the first to explore the effect of loss shocks that *increase*, rather than *decrease*, the financial quality of insurers.

Second, this paper adds to the literature on financial quality and product market behaviors (e.g., Opler and Titman, 1994). The existing studies typically use leverage (Fresard, 2010) or cash holdings (Campello, 2006) to measure a firm's financial strength, which makes it difficult to disentangle changes in financial position due to idiosyncratic shocks from common shocks. In this paper, I am able to clearly identify the source of financial shocks that are specific to each market participant. This helps resolve the possibility of any spurious relationship between actions taken by both a firm and its rivals.

Third, my findings add to the evidence of market discipline. Several papers have documented that the market penalizes insurers with high default risk through decreasing prices (Sommer, 1996) and declining premium growth (Epermanis and Harrington, 2006). I show in the paper that unaffected insurers are rewarded through higher prices following a loss shock.

I organize the remainder of the paper as follows. In Section 3.2, I develop a simple model that predicts the relationship between an insurer's pricing decision and rivals' loss shocks. Section 3.3 describes the setting and methodology for empirical analysis. Section 3.4 discusses my main results and examines the differential effects of rivals' loss shocks on prices based on market characteristics. Section 3.5 concludes.

3.2 Model and Hypothesis

In this section, I develop a simple model to illustrate how rivals' loss shocks affect an insurer's pricing performance. The setup of the model draws on Cummins and Danzon (1997), who explore how a firm's insurance pricing responds to loss shocks that deplete its capital. By extending the setting into a competitive market, I show that an insurer's pricing behavior is also dependent on its rivals' relative position following a loss shock.

3.2.1 The Model

Consider two insurers, insurer i and insurer j, competing in a product market ($i \neq j$). Assume each insurer faces a demand function that depends on of i) the price and the default risk of the insurer itself; and ii) the price and the default risk of its rival. Denote the demand function (or dollar amount of promised insurance payment) by $Q(p^i, b^i, p^j, b^j)$, where p represents the "unit price" per dollar of insurance payment; b represents an insurer's default put option per dollar of liabilities as an inverse function of asset-to-liability ratio x (i.e.,$b_x < 0$).[1]

More specifically, the assumptions of their relationships are described as follows. First, an insurer i's demand is a decreasing and concave function of its own price p^i and its own default put option b^i, or $Q_{p^i} < 0$, $Q_{p^ip^i} < 0$; $Q_{b^i} < 0$, $Q_{b^ib^i} < 0$. That is, as insurer i's price or default risk of its own increases, their demand reduces.

[1]By modeling insurance liabilities as risky debt, the unpaid amount of promised insurance compensation in the event of an insurer's default is equivalent to $Max(0, Liability - Asset)$ (see Merton, 1974).

The more the price or the default risk of its own increases, the more their demand is weakened. Second, Q^i is an increasing and convex function of its rival j's price p^j and default put option b^j, or $Q_{p^j} > 0$, $Q_{p^j p^j} > 0$; $Q_{b^j} > 0$, $Q_{b^j b^j} > 0$. In other words, a higher price or a higher default risk by the rival firm j helps strengthen insurer i's demand. The greater the price or the default risk of rival j increases, the more insurer i's demand is strengthened. Third, holding other things constant in a competitive market, if insurer i decreases its price, the rival insurer j has an incentive to defend its market share by lowering price as a response, i.e., $\partial p^j / \partial p^i > 0$.

Now consider a one-period model with two dates (time 1 and time 2), where insurer i begins at time 1 with assets A_1^i, preexisting liabilities (maturing at time 2) with a face value of L_1^i, and equity $A_1^i - L_1^i$. Note that L_1^i here simply represents the promised loss payments to policyholders without taking into account any possibilities of insurer i's failure. After adjusting for default risk, the value of policyholder's claims is equal to the riskless present value of any promised payments less the value of default put option, or $L_1^i[e^{-r} - b(x_1^i)]$, where r represents the risk-free discount rate and x_1^i the asset-to-liability ratio at time 1.

At time 1, insurer i makes decisions on issuing new equities E_2^i and new policies Q^i (maturing at time 2) with price p_2^i. Accordingly, right following the issuance the assets of insurer i becomes $A_1^i + (E_2^i + p_2^i Q^i)$ and the value of liabilities adjusted for default risk $(L_1^i + Q^i)[e^{-r} - b(x_2^i)]$, where x_2^i is the asset-to-liability ratio at time 2. By applying the put-call parity, the values of equity adjusted for default risk right before (C_1^i) and after (C_2^i) the issuance of new equity and new policies at time 1 are, respectively,

$$C_1^i = A_1^i - L_1^i[e^{-r} - b(x_1^i)] \tag{3.1a}$$

$$C_2^i = [A_1^i + (E_2^i + p_2^i Q^i)] - (L_1^i + Q^i)[e^{-r} - b(x_2^i)] \tag{3.1b}$$

where $x_1^i = A_1^i / L_1^i$; $x_2^i = (A_1^i + E_2^i + p_2^i Q^i)/(L_1^i + Q^i)$.

Following Cummins and Danzon (1997), the objective of insurers is set to maximize the value added to equity such that the issuance of new equity and policies at time 1 is for the best interest of both old and new stockholders as well as old and new policyholders. That is,

$$\max_{p_2^i, E_2^i} C_2^i - (E_2^i + C_1^i) = Q^i[p_2^i - e^{-r} + b(x_2^i)] + L_1^i[b(x_2^i) - b(x_1^i)] \tag{3.2}$$

On the right-hand side of Eq. (3.2), the first term represents the tradeoff from issuing new policies between adding value to equity in the amount of $Q^i p_2^i$ and raising the value of adjusted liabilities in the amount of $Q^i[e^{-r} - b(x_2^i)]$. The second term represents the change in the default value from old liabilities.

The first order conditions with respect to the price of new policies p_2^i and the newly issued equity E_2^i for this maximization problem are, respectively,

$$\begin{aligned} &\left[Q^i + (Q_p^i + Q_{p^j}^i \frac{\partial p_2^j}{\partial p_2^i} + Q_{b^j}^i b_x^j x_p^j \frac{\partial p_2^j}{\partial p_2^i})[p_2^i - e^{-r} + b(x_2^i)]\right] \\ &\qquad + \left[Q_b^i[p_2^i - e^{-r} + b(x_2^i)] + Q^i + L_1^i\right] b_x^i x_p^i = 0. \end{aligned} \tag{3.3}$$

$$\left[Q_b^i[p_2^i - e^{-r} + b(x_2^i)] + Q^i + L_1^i\right] b_x^i x_e^i = 0. \tag{3.4}$$

where Q_p^i, $Q_b^i = \frac{\partial Q^i}{\partial p_2^i}, = \frac{\partial Q^i}{\partial b^i}$, respectively; b_x^i, x_p^i, $x_e^i = \frac{\partial b^i}{\partial x^i}, = \frac{\partial x^i}{\partial p_2^i}, = \frac{\partial x^i}{\partial E_2^i}$, respectively; b_x^j, x_p^j, $x_e^j = \frac{\partial b^j}{\partial x^j}, = \frac{\partial x^j}{\partial p_2^j}, = \frac{\partial x^j}{\partial E_2^j}$, respectively. Eq. (3.3) explicitly shows how competitor j's reaction and its effect on insurer i's demand are simultaneously considered in determining the optimal price of insurer i. By combining Eq. (3.3) and Eq. (3.4), the overall effect of insurer i's price change on its demand after taking into account the competitive factor can be summarised as:

$$\frac{dQ^i}{dp_2^i} = Q_p^i + \left(Q_{p^j}^i \frac{\partial p_2^j}{\partial p_2^i} + Q_{b^j}^i b_x^j x_p^j \frac{\partial p_2^j}{\partial p_2^i}\right) = \frac{-Q^i}{p_2^i - e^{-r} + b(x_2^i)} < 0. \tag{3.5}$$

The two terms in parentheses from Eq. (3.5) represent the competitive effects from competitor j's reaction in response to insurer i's initial action in price change. Intuitively, after observing a decrease in insurer i's price, competitor j would also tend to lower its price to maintain its clientele. However, in this way competitor j also sacrifices the opportunity to further strengthen its solvency level through charging high price from newly issued policies. It is worth noting that the only difference between a monopolistic market and a competitive market lies exactly in these two terms[2]. Nevertheless, it is easy to find that the sign of the overall effect from Eq. (3.5) is still negative, consistent with the sign of Q^i_p, suggesting that the competitive effect from competitor j only moderates the main effect from own price sensitivity of demand Q^i_p.

Based on the optimal solutions from Eq. (3.3) and Eq. (3.4), I turn my attention to one central question in the following section: how does the optimal price of insurer i respond to competitor j's retroactive loss shocks?

3.2.2 Response of Price to Competitor's Loss Shocks

Assuming a retroactive loss shock increases the face value of competitor j's pre-existing liabilities L^j_1, insurer i's optimal price change in response to the loss shock is:

$$\frac{\partial p^i_2}{\partial L^j_1} = \frac{Q^i_{b^j} + \left[Q^i_{pb^j} + \frac{\partial p^j_2}{\partial p^i_2}\left(Q^i_{p^j b^j} + Q^i_{b^j b^j} b^j_x x^j_p\right)\right][p^i_2 - e^{-r} + b(x^i_2)]}{-D/b^j_x x^j_{L_1}} \tag{3.6}$$

where D is the second derivatives of the objective function with respect to insurer i's price p^i_2; to ensure there exists a local maximum, it is required that $D < 0$.

Eq. (3.6) suggests that competitor j's loss shocks may influence insurer i's price. The direction of the response depends on how an increase in competitor j's insolvency risk following the shock affects insurer i's own price sensitivity of demand ($Q^i_{pb^j}$) and cross price sensitivity from competitor j ($Q^i_{p^j b^j}$). Suppose the loss shock only threatens the financial quality of competitor j, it is more likely that insurer i's demand would become less sensitive to its own price change, but more sensitive to competitor j's price change (that is, $Q^i_{pb^j} > 0$ and $Q^i_{p^j b^j} > 0$). In this case, a positive relationship between insurer i's price and competitor j's loss shock is expected, or $\partial p^i_2/\partial L^j_1 > 0$. Intuitively, concerned about the credibility of insurer j to payoff the promised loss

[2] In Cummins and Danzon (1997), the overall effect of insurer i's price change on its demand is $\frac{dQ^i}{dp^i_2} = Q^i_p < 0$, since they do not consider any competitive elements in their model.

payments following the shock, insurer j's policyholders would prefer to reenter the market and buy the same policies from insurer i for its better financial quality. Given the advantage, insurer i could benefit from the demise of competitor j by charging higher prices without being worried about losing customers.

Interestingly, this prediction implies that even if a loss shock does not directly affect an insurer's existing business, its insurance prices may still change. To the extent that rival firms become financially constrained from the shock, an insurer will benefit from being in a better financial position by commanding a higher price. In what follows, I take the theoretical analysis into data and test the prediction empirically.

3.3 Empirical Design

To test for the predictions from my theoretical model, I use the U.S. property/casualty (P/C) insurance industry as a laboratory. The competitive nature of the industry, across various lines of business and geographies, provides a rich setting for me to study the interplay between financial shocks and product market performance (i.e., pricing behaviors) in competitive markets.

3.3.1 Setting, Sample, and Variable Construction

The empirical analysis focuses on insurers who write personal lines, including homeowners, farmowners, and private passenger auto. I define each individual state as a distinct product market, where the competing firms vary. An insurer may write business in several states. But their position relative to state rivals could differ and therefore have different competitive performance as reflected in pricing behaviors. More importantly, U.S. insurance regulation is state-based. To the extent that state governments develop their own rules, such as rate regulation, competitive environments differ substantially. I further exploit this variation in Section 3.4.2.

In a given state market, I examine the pricing of insurers who *only* write personal lines and consider their "rivals" as insurers who write both personal and commercial lines. More precisely, I am interested in how rivals' commercial loss shocks affect insurance prices of personal-only writers. Considering that loss shocks from commercial lines only directly affects rivals' business, any observed pricing changes from personal-only writers can thus be safely ascribed to the competitive dynamics in the product market.

I employ P/C insurer annual financial statement data filed with the National Association of Insurance Commissioners (NAIC) over the period of 1995 to 2017. I use the following criteria to construct my sample. First, an insurer must have positive liabilities, assets, losses incurred, and premiums written. Second, I only include insurers that are operating for at least two years so that there are no missing values for all variables. Third, to ensure that an insurer plays a non-trivial role in the market (i.e., in a state), I only keep firms with at least 0.05 percent market share of personal lines in a state market.[3]

Pricing variable. The outcome of interest is insurance price. The variable is defined as premiums earned divided by the present value of incurred losses (see, e.g., Cummins et al., 2010). In the denominator, the incurred losses (including loss adjustment expenses) are discounted using the U.S. treasury yields obtained from the FRED database of the Federal Reserve Bank of St. Louis. I also apply the chain-ladder method (Lemaire, 2013) to estimate the payout proportions in each line of business using the industry-wide Schedule P data from the A. M. Best Aggregates & Averages.[4] The measure thus can be interpreted as the "unit price" charged by an insurer to deliver one dollar of expected loss payout. The information of premiums earned and incurred losses are available by state by line. So I combine homeowners, farmowners, and private passenger auto lines and estimate the personal-line price variable at the firm-state level.

[3]For robustness check, I also examine firms with at least 0.1/0.2 percent market share of personal lines in a state market. The selection standard, however, has no bearing on my results.

[4]The chain ladder method is an actuarial method that estimates the average ratio of loss payments from one valuation date to another, assuming that the historical pattern will continue in the future.

Loss shock variables. To estimate an insurer's loss shocks, I follow Cummins and Danzon (1997) and use one-year loss reserve development (LRD) for losses incurred in prior years, scaled by the firm's prior-year total liability. The variable is constructed at firm level.

One-Year LRD_{it} represents an insurer i's adjustment of expected losses from $t-2$ to $t-1$ for policies issued in years $< t$. It is directly linked to a firm's surplus (Cummins and Danzon, 1997; Weiss and Chung, 2004) as:

$$\begin{aligned} \text{Surplus}_{it} = \text{Surplus}_{i,t-1} &+ \text{New External Capital}_{it} + \text{New internal Capital}_{it} \\ &- \text{One-Year LRD}_{it}. \end{aligned} \tag{3.7}$$

Intuitively, following an unexpected loss event that elicits a surge of claims, an insurer may adjust its loss reserves upward by reducing surplus to maintain reserve adequacy. Accordingly, a *positive* value of LRD (or a positive loss shock) suggests a *decrease* in financial strength, keeping everything else constant. I further decompose LRD into commercial-line and personal-line, separately, as a way to specify the source of loss shocks.[5] More specifically, for insurer i at time t,

$$\begin{aligned} \text{Shock}_{it} &= \text{CommercialShock}_{it} + \text{PersonalShock}_{it} \\ &= \frac{\text{Commercial LRD}_{it}}{\text{Total Liability}_{i,t-1}} + \frac{\text{Personal LRD}_{it}}{\text{Total Liability}_{i,t-1}}. \end{aligned} \tag{3.8}$$

Control variables. I include several additional control variables, including an insurer's surplus-to-liability ratio, the logged value of assets (a proxy for firm size), credit rating from A.M. Best, reinsurance usage, and geographic diversification. The surplus-to-liability ratio measures a firm's ability to absorb loss shocks to old liabilities using existing surplus. Both asset size and credit rating capture an insurer's financial strength and so are directly related to product performance (e.g., pricing).

[5]Personal lines defined in this paper consist of homeowners, farmowners, private auto liability, and private auto physical damage. Commercial lines include all other lines except international and reinsurance.

I assign a value of one if an insurer has a A^- or better rating, and zero otherwise.[6] Reinsurance usage and geographic diversification reflect an insurer's risk exposure and risk-bearing ability. I use the geographic Herfindahl-Hirschman Index (GHHI) to proxy a firm's business concentration across states by direct premiums written.

Table 3.1: Summary Statistics

	Personal-Line Writers		Personal-Line *only* Writers		Personal-Line *and* Commercial-Line Writers ("Rivals")	
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) Mean	(6) SD
Firm-state-level Variables						
Personal Price$_t$	1.774	0.625	1.685	0.527	1.788	0.638
Personal Market Share$_t$	0.010	0.022	0.008	0.013	0.010	0.023
Personal Market Share$_{t-1}$	0.010	0.023	0.008	0.013	0.010	0.024
No. of firm-state-year obs.	103,097		13,938		89,159	
Firm-level Variables						
Commercial Shock$_{t-1}$	-0.002	0.028	0.000	0.000	-0.003	0.032
Surplus$_{t-1}$/Liability$_{t-2}$	0.722	0.392	0.722	0.391	0.707	0.365
Reinsurance Usage$_{t-1}$	0.463	0.326	0.379	0.376	0.433	0.287
Geographic HHI$_{t-1}$	0.617	0.360	0.802	0.306	0.563	0.360
Best's Rating$_{t-1}$	0.714	0.452	0.351	0.477	0.815	0.389
Log(Asset$_{t-1}$)	18.633	1.829	17.305	1.249	19.331	1.790
No. of firm-year obs.	18,002		1,683		16,319	

Note: Variables are winsorized at the 1st and 99th percentiles. To ensure that an insurer plays a non-trivial role in the product market, I only keep firms with at least 0.05 percent market share of personal lines in a state market. Personal lines include homeowners, farmowners, private passenger auto. Commercial lines include all other lines except international and reinsurance. *Personal Price* is defined as premium earned divided by the present value of losses incurred from personal lines. *Personal Market Share* is an insurer's share of direct premiums written among all personal-line writers in a state market. *Commercial Shock* is defined as the one-year loss reserve development (LRD) from commercial lines, scaled by a firm's prior-year total liabilities. *Reinsurance usage* is defined as reinsurance ceded divided by the sum of direct premiums written and reinsurance assumed. *Geographic HHI* is defined as the sum of squares of the share of direct premiums written across 50 states plus the District of Columbia. *Best's Rating* is a dummy variable equals one of an insurer has a A^- or better rating, and zero otherwise. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Summary statistics. Table 3.1 shows summary statistics of personal-line writers from 1995 to 2017. The variables at the firm-state level describe competitive outcomes

[6]The A^- threshold is specified given prior research and anecdotal evidence that a downgrade to A^- below rating creates the most challenge for insurers to retain and develop business (see, e.g., Epermanis and Harrington, 2006; Business Insurance, 2003).

in a market in a given year. As one may expect, the majority of players (in count) in the state markets are insurers who write both personal and commercial lines (or "rivals" in my setting). They account for 86% of the total number of firm-state observations over the years. Their average market share is 1%, which is slightly larger than insurers who *only* write personal lines (0.8%). However, this is mainly driven by a few large rivals since the top players with the largest market share in a state market tend to write both lines.[7] These rivals also charge relatively higher personal-line prices. Their average unit price is about 1.8 per dollar of expected loss payment, while the mean price for personal-line-only writers is 1.7.

By looking at these firms' characteristics further, rivals who write both personal and commercial lines on average experience *negative* commercial loss shocks, indicating a reserve redundancy and a favorable business environment from commercial lines. Compared to personal-only writers, they also tend to be larger, better-rated, more geographically diversified, and use more reinsurance. Their surplus-to-liability ratio is slightly lower, although the difference does not appear statistically significant.

3.3.2 Main Specification

For my testing design, I investigate the link between loss shocks and insurance pricing. My model predicts that irrespective of the source of a loss shock, as long as the shock influences the relative financial position of a firm in a competitive market, they will be reflected in pricing performance.

[7]For example, the highest value of market share among all personal-line writers in my sample is 28 percent in West Virginia from State Farm Mutual Automobile Insurance Company. The company writes both personal and commercial lines. In contrast, the highest market share written by a personal-only insurer is 15 percent in New York by GEICO.

In my baseline model, I look at insurers who *only* write personal lines and examine how rival firms' loss shocks from commercial lines affect personal-line insurance pricing. More specifically,

$$\begin{aligned}\text{PersonalPrice}_{ist} =& \gamma_1 \text{RivalCommercialShock}^s_{j\neq i,t-1} \\ &+ \gamma_2 X_{i,t-1} + \text{FE}_{is} + \text{FE}_t + \varepsilon_{ist}\end{aligned} \tag{3.9}$$

where i and j denote firm, s denotes state, t denotes year. The dependent variable is insurer i's personal-line prices in state s in year t, minus its state-year average among *all* personal-line writers. The variable after the adjustment thus gauges an insurer's price relative to its state rivals in a given year.

My main variable of interest is the commercial loss shocks experienced by insurer i's rivals in state s who write *both* personal and commercial lines in year $t-1$. Specifically, I define it as:

$$\text{RivalCommercialShock}^s_{j\neq i,t-1} = \mu^s_{j\neq i}(\text{CommercialShock}_{j,t-1}) \tag{3.10}$$

where $\mu^s_{j\neq i,t-1}$ represents the mean among all rival insurers j (of insurer i) participating in state s who write *both* personal and commercial lines. Simply put, I estimate the commercial loss shock variable (at firm level) for each rival insurer and then take the average across all rivals.[8] Accordingly, the measure is a reflection of the average level of commercial loss shocks experienced by insurer i's rival firms in state s. Note that a rival insurer may write commercial business in several states. So this variable also incorporates shock information from all other states. In the case when state peers are on average adversely affected by commercial loss shocks, or $\text{RivalCommercialShock}^s_{j\neq i,t-1} > 0$, insurer i can be considered in a relatively better financial position and, hence, would be able to perform better by charging a higher price. A positive sign on γ_1 is expected.

[8]The results are robust to using a weighted average by an insurer's liabilities (instead of using a simple average). However, as one may expect the weighted average measure contains lower dispersion.

I also add firm-state fixed effects, FE_{is}, to account for time-invariant unobserved heterogeneity regarding the business of a specific insurer in a particular state. In addition, $X_{i,t-1}$ include a set of time-varying control variables that are specific to insurer i and that may directly affect an insurer's pricing decisions (e.g., size, credit rating, reinsurance usage, geographic diversification, etc.). All the control variables are similarly adjusted by subtracting their state-year averages among *all* personal-line writers. For this reason, I only include year fixed effects. Regressions report heteroskedasticity-robust standard errors clustered by state.

3.3.3 Instrument for Commercial Loss Shocks

An insurer's loss reserve development (LRD), however, may be an endogenous decision that relates to their pricing strategy. LRD consists of not only an insurer's reevaluation of risks in past policies but also loss reserve errors. It is well-documented that there exists a discretionary element in the reserving error measure and an insurer has other motivations to manipulate their loss reserves (e.g., Petroni et al., 2000; Grace and Leverty, 2012).

To address the potential endogeneity concern, I exploit the enactment of a specific type of tort reform, noneconomic damages caps (NEDC), as a source of exogeneous variation in insurance losses at state level. More specifically, I instrument for $\text{RivalCommercialShock}^{s}_{j\neq i,t-1}$ with state rivals' average share of commercial liability business written in states that enacted NEDC at time $t-1$. I estimate the following regression in the first stage,

$$\begin{aligned}\text{RivalCommercialShock}^{s}_{j\neq i,t-1} =&\eta_1\text{ShareNEDC}^{s}_{j\neq i,t-1} + \eta_2 X_{i,t-1} \\ &+ \text{FE}_{is} + \text{FE}_t + \mu_t \end{aligned} \tag{3.11}$$

where

$$\begin{aligned}
&\text{ShareNEDC}^s_{j\neq i,t-1} = \mu^s_{j\neq i}(\text{ShareNEDC}_{j,t-1}) \\
&= \mu^s_{j\neq i}\left[\sum_{j,s,t-1} \frac{\text{CommercialLiabPremiumsWritten}_{j,s,t-1}}{\text{TotalPremiumsWritten}_{j,t-1}} \times \text{NEDC}_{s,t-1}\right]; \qquad (3.12)
\end{aligned}$$

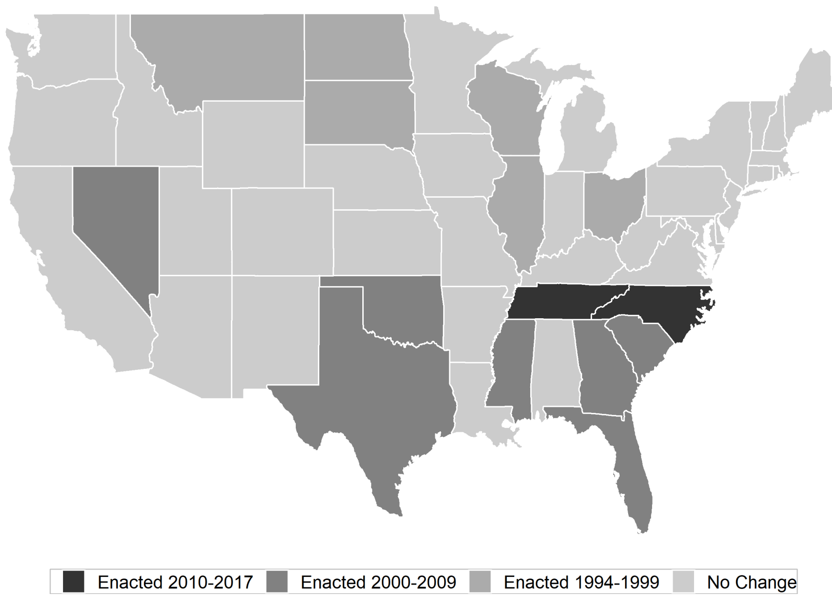
$\text{NEDC}_{s,t-1}$ is a dummy variable equals one if a state s enacts NEDC in year $t-1$, and equals zero for any years before/after $t-1$.

To be clear, I construct the instrument in the following steps. First, an insurer may write business in several states. For each rival insurer j, I calculate the share of a firm's total direct premiums written in commercial liability lines that are from state s in year $t-1$. I multiply the share by $\text{NEDC}_{s,t-1}$ and sum the product over all the states. The sum, $\text{ShareNEDC}_{j,t-1}$, measures a rival insurer j's share of commercial liability business exposed to the regulatory change in year $t-1$. I then take the average of $\text{ShareNEDC}_{j,t-1}$ across all the rival firms participating in state market s.

To be a valid instrument, two conditions must be satisfied. First, the instrument should be strongly related to an insurer's commercial loss shocks (or relevance restrictions). Tort reforms commonly involve a change in U.S. legal system that either lowers the damages received by the victim from liability lawsuits or reduces the ability to bring tort litigation into courts. Among various types of tort reforms, NEDC has been found the most influential in lowering insurance losses (Born et al., 2009; Born and Karl, 2016) and reducing loss reserve volatility (Born et al., 2020) on commercial liability lines (e.g., medical practice). By limiting the size of court awards that a plaintiff may receive from pain and suffering, the effect on insurance loss amounts is immediate. Figure 3.1 displays the spatial distribution of states that enacted NEDC between 1994 and 2017. In total, 19 states adopted the reform over the 23 years.

Second, the instrument should only affect personal insurance pricing through its influence on the commercial loss shock variable (or exclusion restrictions). The only element in personal lines that may relate more closely to tort reform is private passenger auto liability, but no effects of NEDC on losses have been found (e.g., Born, 2017). To completely isolate any conceivable effects, I also drop private auto business from

Figure 3.1: Enactment of Noneconomic Damages Caps



Note: Between 1994 and 2017, 19 states enacted noneconomic damages caps (NEDC), a tort reform that limits the size of court awards that a plaintiff may receive from pain and suffering.

personal lines and examine only homeowners/farmowners lines for additional analysis. The enactment of NEDC therefore represents a state-level shock that exogenously decreases the insurance costs in commercial lines, but not in personal lines. Reasonably, any observed influence on personal-line pricing is transmitted only through its association with the change in an insurer's relative position in a product market.

3.4 Results: The Effect of Rivals' Loss shocks on Insurance Pricing

3.4.1 Main Results

In Table 3.2, I estimate my baseline model as in Eq. (3.9) and examine the response of an insurer's personal-line pricing to rivals' loss shocks from commercial lines. I look at insurers who *only* write personal lines. For these firms, their existing business is not exposed to loss shocks from commercial lines. Therefore, there is little reason to believe that these shocks have a direct effect on their personal-line pricing other than through its association with product market dynamics. Column (1) presents the OLS results. The coefficient on rivals' commercial loss shocks is positive and significant. Recall that an *increase* in rivals' loss shocks is an indication of a *decrease* in their financial strength, all else equal. This suggests that a personal-line writer would benefit from an adverse loss shock that hurts their state rivals who also write commercial lines.

Table 3.2: Baseline Estimation

	Personal Price_{st}		Homeowners/Farmowners Price_{st}	
	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)
Rival Commercial Shock^s_{t-1}	2.492***	30.936**	12.167**	28.768**
	(0.995)	(12.795)	(5.805)	(13.819)
Controls	Yes	Yes	Yes	Yes
Firm-state FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Cluster by state	Yes	Yes	Yes	Yes
Observations	13,938	13,938	5,339	5,339
First Stage: Rival Commercial Shock^s_{t-1}				
ShareNEDC^s_{t-1}		−0.072***		−0.052***
		(0.015)		(0.013)
F-stat		23.70		56.18

Note: Dependent variable is adjusted by removing state-year average across all personal-line writers, so the variable measures an insurer's personal-line price relative to that of its rivals. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

I report the instrumental variable (IV) results in Column (2). In the first stage, I exploit the enactment of NEDC, a tort reform that limits non-economic damage awards, as a source of exogenous changes in rival firms' commercial-line losses. The first-stage results are summarized in the bottom part of Table 3.2. The coefficient on the instrument is statistically significant. A large *Cragg-Donald Wald F*-statistic ($>$ 10) indicates that it is a sufficiently strong instrument for commercial-line loss shocks. The negative sign confirms that the enactment of NEDC lowers an insurer's expectation of loss payments from commercial lines.

In the second stage, the IV estimate of the effect of rivals' commercial shock on personal-line pricing is significantly positive, suggesting that an insurer outperforms its adversely affected state rivals by charging a higher price. The effect spills over to business lines that are not directly affected by the loss shock. In terms of magnitude, a one-unit increase in rivals' commercial loss shocks leads to 31 units increase in personal-line prices. To further clarify the effects, a one-standard-deviation increase in rivals' loss shocks allows personal-only writers to charge a unit price that is 0.26 higher. This is about 15% higher than the average price of their affected competitors (see Column (5) of Table 3.1).

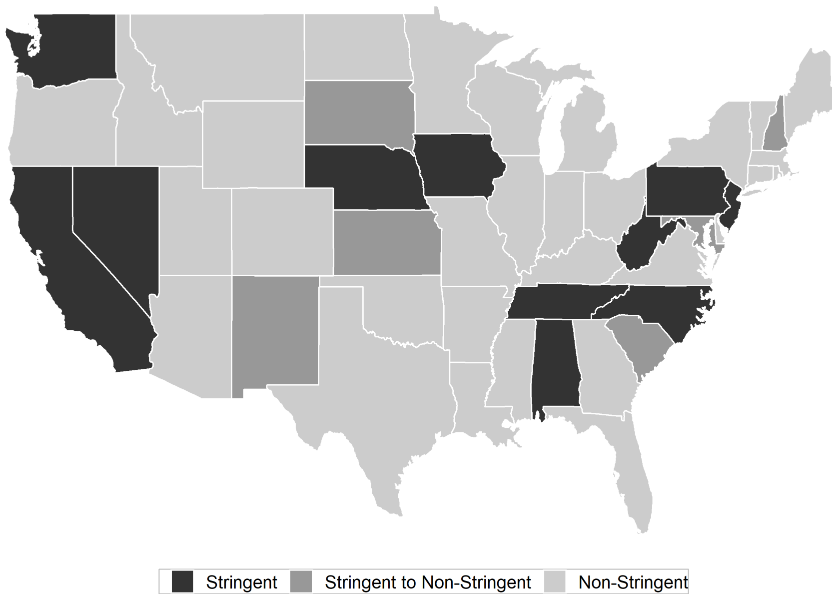
In Column (4), I estimate similar regressions by looking at the pricing of homeowners/farmowners, two personal lines of business that are mainly property-related and so further mitigate my concern that the enactment of NEDC also influences personal liability lines.[9] The positive effect of commercial-line loss shocks remains statistically significant. The IV estimates in Column (4) indicate that if rivals' commercial loss shocks increase by one standard deviation, an insurer can charge a price that is 0.23 units higher from homeonwers/farmowners policies, or 11% more than the average price charged by their adversely affected rivals (see Column (5) of Appendix Table C.1).

3.4.2 Competitive Environment Differences

In this section, I further characterize the product market based on their competitive environment and investigate whether the impact of rivals' commercial loss shocks differs across these markets. In particular, I exploit the variation of the insurance rate regulation system set up by state regulators. In some states, any proposed price change must be reviewed and approved by the regulator. The so-called "prior approval" system thus discourages the force of market competition in determine in-

[9]Property lines have short payout tails, while liability lines have long payout tails. Based on the aggregated historical payout pattern from Schedule P of the NAIC annual statement, insurers pay about 95% of homeowners/farmowners claims within two years, suggesting that most of their loss payments are property-related. For comparison, only about 20% of product liability and medical malpractice claims are paid within two years.

Figure 3.2: State-Level Rate Regulation on Personal Lines



Note: A state is defined as having stringent rate regulation if the state uses the "prior approval" filing system in a given year, and non-stringent otherwise (Harrington, 2004). Between 1994 and 2017, the rate regulation systems on personal lines remain stable in most states.

surance pricing. In contrast, some other states adopt a competitive rating system where insurers are granted more freedom to set their own insurance prices with little or no intervention.[10] Accordingly, the two systems represent disparate competitive environments where the competitive effect of loss shocks may differ.

Since the outcome of interest throughout the paper is personal-line prices, I classify state markets into two categories: states with stringent rate regulation on personal lines in a given year and those with non-stringent rate regulation. A market is defined as stringently regulated if the state uses the "prior approval" system (Harrington, 2004). The information of rate filing system in each state is collected from the Compendium of State laws on Insurance Topics of NAIC.

Figure 3.2 displays the geographic distribution of state-level rate regulation on personal lines based on their stringency level. Throughout the sample period of 1994-

[10]For example, under the "file and use" system, insurers must file their rates before the implementation but they do not need to be approved by state regulators. Under the "use and file" system, insurers simply need to inform regulators within a certain period after they launch their rates.

2017, 12 states have maintained stringent rate regulation on personal lines; 31 states have preserved non-stringent rate regulation; 8 states switched from stringent to non-stringent rate regulation around 2000. For the subsequent subgroup analysis, I limit my focus on states with consistent rate regulations over my sample period.[11]

Table 3.3: Subgroup Analysis: 2SLS

	Personal Price$_{st}$		
	Stringent (1)	Non-stringent (2)	Diff. Stringent − Non-stringent (3)
Rival Commercial Shock$^{s}_{t-1}$	1.473	39.092**	0.05*
	(27.997)	(16.605)	
Controls	Yes	Yes	
Firm-state FEs	Yes	Yes	
Year FEs	Yes	Yes	
Cluster by state	Yes	Yes	
Observations	3,482	8,664	

Note: A market is defined as "stringent" if the state uses the "prior approval" filing system in a given year, and "non-stringent" otherwise. Dependent variable is adjusted by removing state-year average across all personal-line writers, so the variable measures an insurer's personal-line price relative to that of its rivals. Column 3 presents the p-value of the test that compares coefficients between stringent and non-stringent subgroups. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

I apply the baseline regressions as in Eq. (3.9) to these two subgroups and display the IV estimates in Table 3.3. An insurer's personal-line pricing sensitivity to rivals' commercial loss shocks is more pronounced in state markets with non-stringent rate regulation, as indicated in Column (2). The coefficient on instrumented commercial loss shocks from state rivals are only statistically significant in states with a favorable competitive environment. More specifically, if rivals' commercial loss shocks increase by one standard deviation, the personal-only writers are able to charge a price that is 0.39 units (or 22%) higher than their affected rivals. A comparison between the coefficients across the two subgroups in Column (3) also show that the magnitude of pricing response to rivals' loss shocks is significantly larger in states with non-stringent rate regulation.

[11] The long-run and short-run effects of deregulation on a state's rate regulation may differ and so confound the competitive effects of loss shocks (see, e.g., Grace et al., 2013).

3.5 Discussion and Conclusion

In this paper, I study an insurer's pricing response to rivals' loss shocks. In particular, I start with a simple theoretical model that predicts the relationship between an insurer's optimal price level and rivals' loss shocks. The prediction implies that an insurer would gain competitive advantage by charging a higher price for not being adversely affected. By using the P/C insurance market as my empirical setting, I find that insurers who *only* write personal lines outperform their affected rivals by commanding a higher price following rivals' loss shocks from commercial lines. The findings suggest that there exists a competitive dimension in insurance pricing. I further show that the competitive effect of loss shocks varies by the competitive environment where insurers participate. The competitive effect of loss shocks is more pronounced in state markets where the rate regulation is consistently non-stringent.

The implications of this study are as follows. First, my findings suggest that loss shocks across lines of business across firms may impact a firm's financial strategies. As we see a growing number of regional catastrophic events in recent years (e.g., wildfires in California, winter storms in Texas), it is important to understand how the market responds to the changing environment, particularly in unexposed regions. This paper shows that insurers do not make pricing decisions in isolation, but take into account the financial positions of their rivals. Thus, for an insurer who writes business in multiple markets, the change in its financial status due to one unexpected loss event may motivate other players to adjust their strategies accordingly. The product market dynamics may also apply to other financial decisions such as innovation investments (e.g., big data and insurtechs), an area that has been discussed the most when it comes to competitive advantage in the insurance industry (Insurance Business, 2021). Future research might consider exploring the interaction.

Second, this paper investigates how one loss shock influence all participants in the market. But the link that I identify in this study differs from the literature on contagion through a firm's internal network (Ge, 2020; Giroud and Mueller, 2019) or counterparty risk (Park and Xie, 2014; Chen et al., 2020). I find that there also exists a competitive effect of financial shocks. Empirical evidence regarding how this competitive effect and contagion effect interact remains scarce (Jorion and Zhang, 2007). I leave it to future research.

Third, my results indicate that the regulatory environment matters to insurers when making pricing decisions. Emerging studies also show that regulatory frictions encourage cross-subsidization across states (Sen and Tenekedjieva, 2021). Given that most P/C insurers write business in more than one state, it would be interesting to explore how they take advantage of the difference in regulatory climate across state markets to enhance their overall competitive performance in the industry.

BIBLIOGRAPHY

ACS (2016). American community survey data. https://www.census.gov/programs-surveys/acs/news/data-releases.html.

Ai, J., P. L. Brockett, and A. F. Jacobson (2015). A new defined benefit pension risk measurement methodology. *Insurance: Mathematics and Economics 63*, 40–51. Special Issue: Longevity Nine - the Ninth International Longevity Risk and Capital Markets Solutions Conference.

Barrot, J.-N. and J. Sauvagnat (2016). Input specificity and the propagation of idiosyncratic shocks in production networks. *The Quarterly Journal of Economics 131*(3), 1543–1592.

Basker, E. and J. Miranda (2018). Taken by storm: Business financing and survival in the aftermath of Hurricane Katrina. *Journal of Economic Geography 18*(6), 1285–1313.

Benoit, J.-P. (1984). Financially constrained entry in a game with incomplete information. *The RAND Journal of Economics*, 490–499.

Billings, S. B., E. Gallagher, and L. Ricketts (2019). Let the rich be flooded: The unequal impact of Hurricane Harvey on household debt. *Working Paper*, 1–61.

Blake, E. S. and D. A. Zelinsky (2018). National Hurricane Center Tropical Cyclone Report: Hurricane Harvey. Technical Report AL092017, National Hurricane Center, https://www.nhc.noaa.gov/data/tcr/AL092017_Harvey.pdf.

Born, P. (2017). The influence of tort reform on auto liability losses and premiums. *Journal of Insurance Issues*, 61–89.

Born, P., W. K. Viscusi, and T. Baker (2009). The effects of tort reform on medical malpractice insurers' ultimate losses. *Journal of Risk and Insurance 76*(1), 197–219.

Born, P. H., E. M. Eastman, and W. K. Viscusi (2020). Reducing medical malpractice loss reserve volatility through tort reform. *North American Actuarial Journal 24*(4), 626–646.

Born, P. H. and J. B. Karl (2016). The effect of tort reform on medical malpractice insurance market trends. *Journal of Empirical Legal Studies 13*(4), 718–755.

Business Insurance (2003). Downgrade shakes SCOR.

Campbell, J. L., D. S. Dhaliwal, and J. Schwartz, William C. (2012). Financing Constraints and the Cost of Capital: Evidence from the Funding of Corporate Pension Plans. *The Review of Financial Studies 25*(3), 868–912.

Campello, M. (2002). Internal capital markets in financial conglomerates: Evidence from small bank responses to monetary policy. *The Journal of Finance 57*(6), 2773–2805.

Campello, M. (2006). Debt financing: Does it boost or hurt firm performance in product markets? *Journal of Financial Economics 82*(1), 135–172.

Caswell, J. W. (1976). Economic efficiency in pension plan administration: A study of the construction industry. *Journal of Risk and Insurance*, 257–273.

Census Bureau (2017). County business patterns. https://www2.census.gov/programs-surveys/acs/.

Chambers, B. (2016). *Multiemployer defined benefit pension plans' liability spillovers: Important connections in US unionized industries.* The University of Utah.

Chen, H., J. D. Cummins, T. Sun, and M. A. Weiss (2020). The reinsurance network among us property–casualty insurers: Microstructure, insolvency risk, and contagion. *Journal of Risk and Insurance 87*(2), 253–284.

Collier, B. L., A. F. Haughwout, H. C. Kunreuther, and E. O. Michel-Kerjan (2020). Firms' management of infrequent shocks. *Journal of Money, Credit and Banking 52*(6), 1329–1359.

Cook, T. D. (2008). "waiting for life to arrive": a history of the regression-discontinuity design in psychology, statistics and economics. *Journal of Econometrics 142*(2), 636–654.

CRS (2020). Policy Options for Multiemployer Defined Benefit Pension Plans. Congressional Research Services.

Cummins, J. D. and P. M. Danzon (1997). Price, financial quality, and capital flows in insurance markets. *Journal of financial intermediation 6*(1), 3–38.

Cummins, J. D., M. A. Weiss, X. Xie, and H. Zi (2010). Economies of scope in financial services: A dea efficiency analysis of the us insurance industry. *Journal of Banking & Finance 34*(7), 1525–1539.

Desai, M. A., C. F. Foley, and K. J. Forbes (2008). Financial constraints and growth: Multinational and local firm responses to currency depreciations. *The Review of Financial Studies 21*(6), 2857–2888.

DOL (2018). 2018 Instructions for Form 5500: Annual Return/Report of Employee Benefit Plan. U.S. Department of Labor.

Egginton, J. F., J. I. Hilliard, A. P. Liebenberg, and I. A. Liebenberg (2010). What effect did aig's bailout, and the preceding events, have on its competitors? *Risk Management and Insurance Review 13*(2), 225–249.

Epermanis, K. and S. E. Harrington (2006). Market discipline in property/casualty insurance: Evidence from premium growth surrounding changes in financial strength ratings. *Journal of Money, Credit and Banking*, 1515–1544.

Experian (2013). Intelliscore Plus V2.

FCC (2017). Communications Status Reports: August 26, 2017–September 5, 2017. Federal Communications Commission, https://www.fcc.gov/harvey.

Federal Reserve Banks (2020). 2020 Report on Employer Firms: Small Business Credit Survey. https://www.fedsmallbusiness.org/medialibrary/FedSmallBusiness/files/2020/2020-sbcs-employer-firms-report.

FEMA (2017a). FEMA Harvey Remote Sensing. https://data.femadata.com/NationalDisasters/HurricaneHarvey/Data/. Accessed: 2019-04-22.

FEMA (2017b). Texas Hurricane Harvey (DR-4332-TX). Federal Emergency Management Agency, https://www.fema.gov/disaster/4332.

FEMA (2018). FEMA Harvey Flood Depths. https://data.femadata.com/NationalDisasters/HurricaneHarvey/Data/DepthGrid/. Accessed: 2019-09-14.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, and O. H. S. Group (2012). The Oregon Health Insurance Experiment: Evidence from the first year. *The Quarterly Journal of Economics 127*(3), 1057–1106.

Frame, D. J., M. F. Wehner, I. Noy, and S. M. Rosier (2020). The economic costs of Hurricane Harvey attributable to climate change. *Climatic Change 160*(2), 271–281.

Franzoni, F. (2009). Underinvestment vs. overinvestment: Evidence from price reactions to pension contributions. *Journal of Financial Economics 92*(3), 491–518.

Franzoni, F. and J. M. Marin (2006). Pension plan funding and stock market efficiency. *the Journal of Finance 61*(2), 921–956.

Fresard, L. (2010). Financial strength and product market behavior: The real effects of corporate cash holdings. *The Journal of finance 65*(3), 1097–1122.

Gallagher, J. and D. Hartley (2017). Household finance after a natural disaster: The case of Hurricane Katrina. *American Economic Journal: Economic Policy 9*(3), 199–228.

Gallagher, J., D. Hartley, and S. Rohlin (2019). Weathering an unexpected financial shock: The role of cash grants on household finance and business survival. *FRB of Chicago Working Paper No. WP-2019-10*.

GAO (2020). Small Business Administration: Disaster loan processing was timelier, but planning improvements and pilot program evaluation needed. Technical Report GAO-20-168, United States Government Accountability Office, https://www.gao.gov/assets/710/704409.pdf.

Ge, S. (2020). How do financial constraints affect product pricing? evidence from weather and life insurance premiums. *Journal of Finance*.

Ghilarducci, T. and K. Terry (1999). Scale economies in union pension plan administration: 1981–1993. *Industrial Relations: A Journal of Economy and Society 38*(1), 11–17.

Giroud, X. and H. M. Mueller (2019). Firms' internal networks and local economic shocks. *American Economic Review 109*(10), 3617–49.

Grace, M. F., R. W. Klein, and S. Tennyson (2013). The effects of regulatory reforms in the south carolina auto insurance market. *Journal of Insurance Regulation 32*.

Grace, M. F. and J. T. Leverty (2012). Property–liability insurer reserve error: Motive, manipulation, or mistake. *Journal of Risk and Insurance 79*(2), 351–380.

Gron, A. (1994). Capacity constraints and cycles in property-casualty insurance markets. *The RAND Journal of Economics*, 110–127.

Haberman, S., Z. Butt, and C. Megaloudi (2000). Contribution and solvency risk in a defined benefit pension scheme. *Insurance: Mathematics and Economics 27*(2), 237–259.

Hahn, J., P. Todd, and W. Van der Klaauw (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica 69*(1), 201–209.

Harrington, S. E. (2004). Effects of prior approval rate regulation of auto. *Deregulating Property-Liability Insurance: Restoring Competition and Increasing Market Efficiency 285*.

Hirsch, B. T. and D. A. Macpherson (2021). Union membership and coverage database. http://www.unionstats.com/.

Houston Chronicle (2018a). 85 percent of Lake Houston area businesses reopen after Harvey. Elliott Lapin, https://www.chron.com/neighborhood/kingwood/business/article/85-percent-of-Lake-Houston-area-businesses-reopen-13166003.php.

Houston Chronicle (2018b). Flood insurance weakness leads to repeat disasters. Mark Collette, 8 July 2018.

Huang, H.-C. and A. J. Cairns (2006). On the control of defined-benefit pension plans. *Insurance: Mathematics and Economics 38*(1), 113–131.

Imbens, G. W. and J. M. Wooldridge (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature 47*(1), 5–86.

Insurance Business (2021). Insurers that innovate gain underlying competitive advantage.

IPCC (2013). Working Group I contribution to the IPCC fifth assessment report, Climate change 2013: The physical science basis. Intergovernmental Panel on Climate Change.

IPCC (2018). Impacts of 1.5 C global warming on natural and human systems. Intergovernmental Panel on Climate Change.

Jorion, P. and G. Zhang (2007). Good and bad credit contagion: Evidence from credit default swaps. *Journal of Financial Economics 84*(3), 860–883.

Josa-Fombellida, R. and J. P. Rincón-Zapatero (2004). Optimal risk management in defined benefit stochastic pension funds. *Insurance: Mathematics and Economics 34*(3), 489–503.

JP Morgan Chase (2018). Bend, don't break: Small business financial resilience after Hurricanes Harvey and Irma. JP Morgan Chase Institute, https://institute.jpmorganchase.com/content/dam/jpmc/jpmorgan-chase-and-co/institute/pdf/institute-bend-dont-break.pdf.

Kahl, M. (2002). Economic distress, financial distress, and dynamic liquidation. *The Journal of Finance 57*(1), 135–168.

Lai, G. C., R. C. Witt, H.-G. Fung, R. D. MacMinn, and P. L. Brockett (2000). Great (and not so great) expectations: An endogenous economic explication of insurance cycles and liability crises. *Journal of Risk and Insurance*, 617–652.

Lang, L. H. and R. Stulz (1992). Contagion and competitive intra-industry effects of bankruptcy announcements: An empirical analysis. *Journal of financial economics 32*(1), 45–60.

Lee, D. S. and T. Lemieux (2010). Regression discontinuity designs in economics. *Journal of economic literature 48*(2), 281–355.

Lemaire, J. (2013). *Automobile insurance: actuarial models*, Volume 4. Springer Science & Business Media.

Lin, Y., T. Shi, and A. Arik (2017). Pricing buy-ins and buy-outs. *Journal of Risk and Insurance 84*, 367–392.

Lin, Y., K. S. Tan, R. Tian, and J. Yu (2014). Downside risk management of a defined benefit plan considering longevity basis risk. *North American Actuarial Journal 18*(1), 68–86.

Marshall, M. I., L. S. Niehm, S. B. Sydnor, and H. L. Schrank (2015). Predicting small business demise after a natural disaster: An analysis of pre-existing conditions. *Natural Hazards 79*(1), 331–354.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics 142*(2), 698–714.

Merton, R. C. (1974). On the pricing of corporate debt: The risk structure of interest rates. *The Journal of finance 29*(2), 449–470.

Mitchell, O. S. and E. S. Andrews (1981). Scale economies in private multi-employer pension systems. *Ilr Review 34*(4), 522–530.

Munnell, A., J.-P. Aubry, W. Hou, and A. Webb (2019). Multiemployer plans: evaluating a proposal to spread the pain. *Journal of Pension Economics & Finance 18*(1), 124–139.

Munnell, A. H., J.-P. Aubry, and C. Crawford (2017). Multiemployer pension plans: Current status and future trends. *Available at SSRN 3122634*.

Myers, S. C. (1977). Determinants of corporate borrowing. *Journal of Financial Economics 5*(2), 147–175.

New York Times (2014a). Insurer warns some pooled pensions are beyond recovery. Mary Williams Walsh .

New York Times (2014b). Thought Secure, Pooled Pensions Teeter and Fall. Mary Williams Walsh .

Nikolov, B., L. Schmid, and R. Steri (2019). Dynamic corporate liquidity. *Journal of Financial Economics 132*(1), 76–102.

NOAA (2021). NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters. https://www.ncdc.noaa.gov/billions/overview.

NPR (2017). Scores of roads closed, 50,000+ displaced: Houston still has a long way to go. David Schaper, https://www.npr.org/2017/09/07/549169437/100-roads-closed-50-000-displaced-houston-still-has-a-long-way-to-go.

Opler, T. C. and S. Titman (1994). Financial distress and corporate performance. *The Journal of finance 49*(3), 1015–1040.

Park, S. C. and X. Xie (2014). Reinsurance and systemic risk: The impact of reinsurer downgrading on property–casualty insurers.

PBGC (2013). Multiemployer Pension Plans: Report to Congress Required by the Pension Protection Act of 2006. Pension Benefit Guarantee Corporation.

PBGC (2018). 2018 pension insurance data tables. https://www.pbgc.gov/sites/default/files/.

PBGC (2020). PBGC Annual Report 2020. Pension Benefit Guarantee Corporation.

Petroni, K. R., S. G. Ryan, and J. M. Wahlen (2000). Discretionary and non-discretionary revisions of loss reserves by property-casualty insurers: Differential implications for future profitability, risk and market value. *Review of Accounting Studies 5*(2), 95–125.

Rampini, A. A., A. Sufi, and S. Viswanathan (2014). Dynamic risk management. *Journal of Financial Economics 111*(2), 271–296.

Rampini, A. A. and S. Viswanathan (2010). Collateral, risk management, and the distribution of debt capacity. *The Journal of Finance 65*(6), 2293–2322.

Rauh, J. D. (2006). Investment and financing constraints: Evidence from the funding of corporate pension plans. *The Journal of Finance 61*(1), 33–71.

SBA (2014). Table of Small Business Size Standards. U.S. Small Business Administration, .

Sen, I. and A.-M. Tenekedjieva (2021). Pricing of climate risk insurance: Regulatory frictions and cross-subsidies. *Available at SSRN*.

Sommer, D. W. (1996). The impact of firm risk on property-liability insurance prices. *Journal of Risk and Insurance*, 501–514.

Sufi, A. (2009). Bank lines of credit in corporate finance: An empirical analysis. *The Review of Financial Studies 22*(3), 1057–1088.

University of Texas (2017). 2017 Texas address and base layers story map. https://ut-austin.maps.arcgis.com/apps/MapSeries/.

U.S. Census Bureau (2017). County business patterns: March 12, 2017. https://www.census.gov/data/datasets/2017/econ/cbp/2017-cbp.html.

U.S. Census Bureau (2018a). 2018 business dynamics statistics. https://www.census.gov/data/datasets/time-series/econ/bds/bds-datasets.html.

U.S. Census Bureau (2018b). Nonemployer statistics: March 12, 2018. https://www.census.gov/programs-surveys/nonemployer-statistics/data.html.

U.S. Department of Labor (2020). Form 5500 datasets. https://www.dol.gov/agencies/ebsa/about-ebsa/our-activities/public-disclosure/foia/form-5500-datasets.

USPS (2017). U.S. Postal Service continues to emphasize safety and restore service after Tropical Storm Harvey.

Van Oldenborgh, G. J., K. Van Der Wiel, A. Sebastian, R. Singh, J. Arrighi, F. Otto, K. Haustein, S. Li, G. Vecchi, and H. Cullen (2017). Attribution of extreme rainfall from Hurricane Harvey, August 2017. *Environmental Research Letters 12*(12), 124009.

Wall Street Journal (2017). Small businesses say federal-disaster aid needs strengthening. Ruth Simon and Joseph De Avila https://www.wsj.com/articles/small-businesses-say-federal-disaster-aid-needs-strengthening-1507667045.

Walls, M. and D. H. Cortes (2018). Recovering from disasters: Evaluating FEMA's Housing Assistance Program in the 2017 hurricane season. Wharton Risk Management and Decision Processes Center https://riskcenter.wharton.upenn.edu/lab-notes/recovering-from-disasters-evaluating-femas-housing-assistance-program-in-the-2017-hurricane-season.

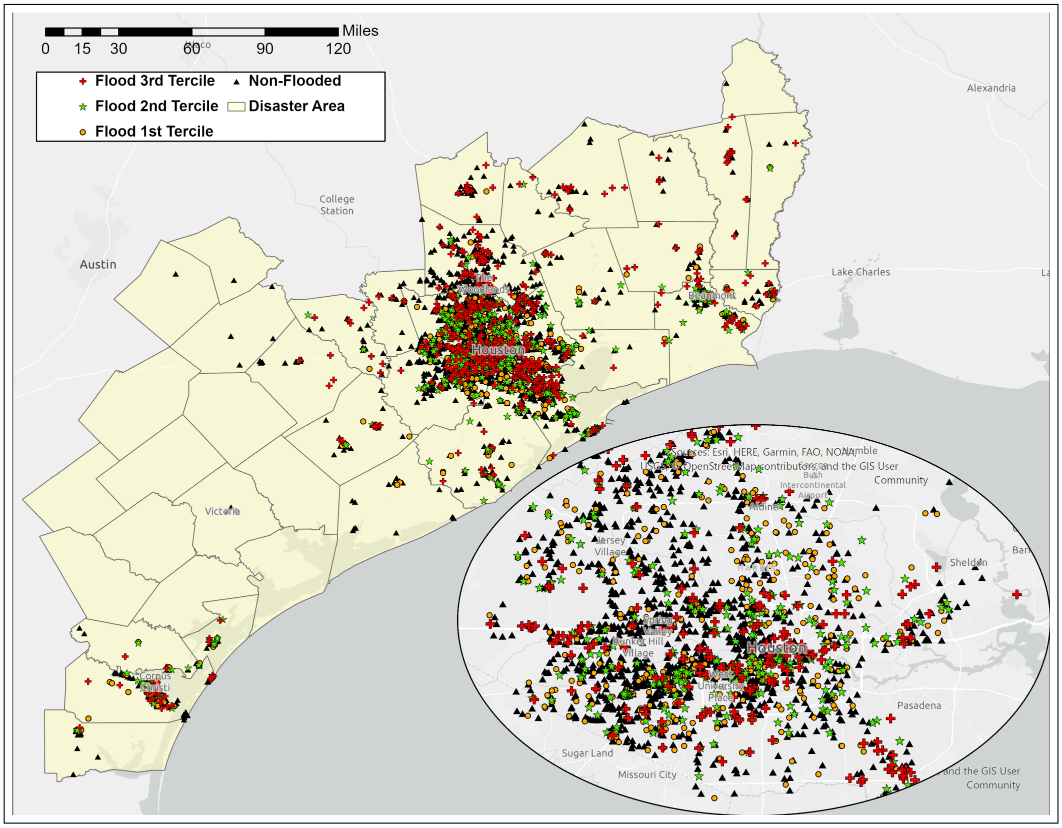
Weiss, M. A. and J.-H. Chung (2004). Us reinsurance prices, financial quality, and global capacity. *Journal of Risk and Insurance 71*(3), 437–467.

Winter, R. A. (1994). The dynamics of competitive insurance markets. *Journal of Financial Intermediation 3*(4), 379–415.

APPENDIX A

APPENDIX FOR CHAPTER 1

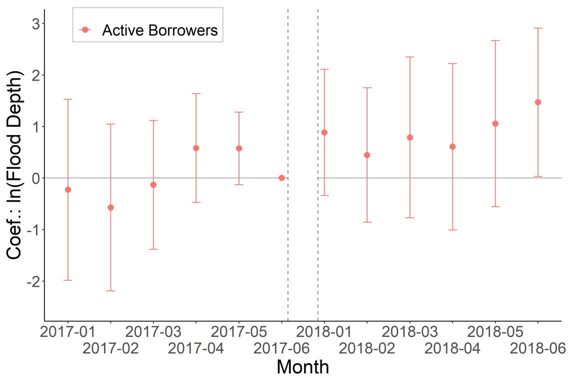
Figure A.1: Studied Firms in Disaster Area: Flood Terciles vs. Non-Flooded



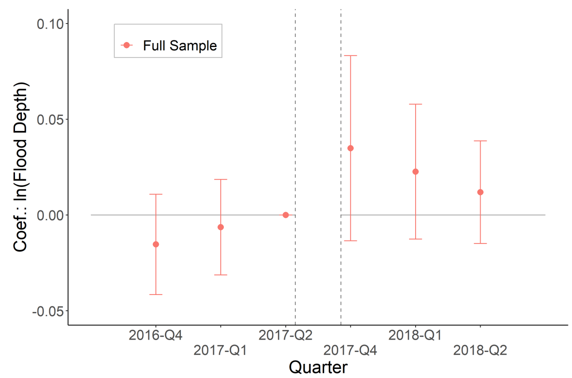
Note: Major Disaster Declaration DR-4332-TX designated 41 counties to receive federal aid (FEMA, 2017b) for Hurricane Harvey. I refer to these counties as the "disaster area" (yellow area). I group flooded firms into terciles based on the flood depth at their location estimated by FEMA ("Flood Depth"; FEMA, 2018). The first tercile (yellow circle) includes firms in areas with less than 1.7 feet of flooding. The third tercile (red cross) includes firms in areas flooded 2.7 feet or more.

Figure A.2: Evolution in DBT, Inquiries, and Balances

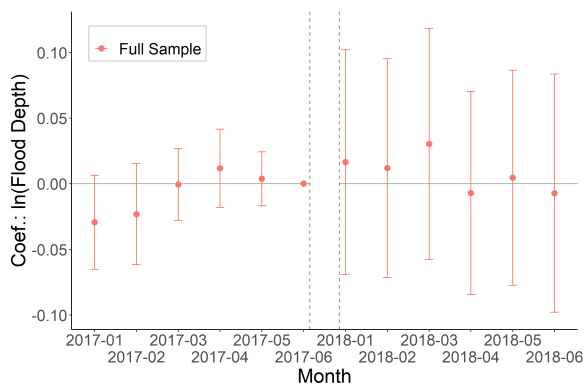
Panel A: DBT



Panel B: Inquiries



Panel C: ln(Balances)



Note: This figure provides general support for the parallel trends assumption. None of the pre-Harvey coefficients are significantly different from zero. Panels A and Panel C show the evolution of monthly DBT and monthly balances. The figures plot 95% confidence interval of event study coefficients of DBT and logged monthly credit balances on the logged flood depth. Coefficients can be interpreted as the effect of flooding relative to the firms outside the disaster area and relative to June 2017. The vertical, dashed lines mark the period July to December 2017, during which I do not observe monthly DBT and balances. Harvey occurred during that period. Panel B shows the evolution of quarterly inquiries. The figure plots 95% confidence interval of event study coefficients of quarterly inquiries on the logged flood depth. The quarterly before Harvey, Q2 2017, is the reference period. The vertical, dashed lines mark the period Q3 2017, during which I do not observe quarterly inquiries. Harvey occurred during that period. The regression models follow Equation (1.5).

Table A.1: Pre- and Post-Harvey Trends: Small Business Activities

	Establishments				
	(1) Total Number	(2) Entry Rate	(3) Exit Rate	(4) Death Rate	(5) Employment
I(Harvey Disaster Area) ×					
I(Three Years Prior)	−5,083.341	0.007	−0.002	−0.001	−7,466.615
	(6,083.709)	(0.006)	(0.004)	(0.004)	(10,986.830)
I(Two Years Prior)	−5,183.057	0.006	−0.001	0.0001	−7,524.380
	(5,980.579)	(0.005)	(0.004)	(0.003)	(10,876.900)
I(One Year Prior)	−5,946.895	0.004	0.011*	0.007	−9,269.130
	(5,976.881)	(0.005)	(0.006)	(0.005)	(10,859.710)
I(One Year After)	−4,164.090	0.004	0.007	0.006	−9,930.771
	(5,911.394)	(0.004)	(0.005)	(0.004)	(10,862.030)
State-MSA FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster by State-MSA	Yes	Yes	Yes	Yes	Yes
N	3,985	3,985	3,985	3,985	3,985
R^2	0.618	0.690	0.487	0.470	0.576

Note: Table presents county-level analysis of small business activities following Hurricane Harvey on counties we draw from for credit report analysis. We estimate:

$$\begin{aligned} y_{jt} = \gamma_0 &+ \sum_t \gamma_{1t} \mathrm{I}_t(\text{Year}) \times \mathrm{I}_j(\text{Harvey Disaster Area}) \\ &+ \sum_t \gamma_{2t} \mathrm{I}_t(\text{Year}) \times \mathrm{I}_j(\text{Outside Disaster Area, TX}) + \mathrm{FE}_m + \mathrm{FE}_t + \varepsilon_{jt}. \end{aligned} \tag{A.1}$$

where j indexes counties and t indexes year. March 2017 serves as reference period. The models include year fixed effects and state-MSA fixed effects. Disaster area represents being one of the 41 counties that were eligible for federal aid in the presidential disaster declaration DR-4332-TX for Hurricane Harvey (FEMA, 2017b). Regressions report robust standard errors clustered by state-MSA. Data are from Business Dynamics Statistics and Nonemployer Statistics of the U.S. Census Bureau (2018a,b). The data include sole proprietors with no paid employees and establishments of firms with fewer than 500 employees. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.2: Post-Sandy Trends: Small Business Activities

	Establishments				
	(1) Total Number	(2) Entry Rate	(3) Exit Rate	(4) Death Rate	(5) Employment
I(Sandy Disaster Area) ×					
I(One Year After)	2,559.999	0.0003	0.002	0.003*	2,119.587
	(11,322.770)	(0.002)	(0.001)	(0.002)	(23,747.000)
I(Two Years After)	4,251.429	−0.001	0.006***	0.005***	3,390.843
	(11,887.000)	(0.002)	(0.001)	(0.001)	(24,589.810)
I(Three Years After)	4,907.492	−0.003**	0.007***	0.005***	5,172.876
	(12,076.800)	(0.002)	(0.001)	(0.001)	(25,372.730)
State-MSA FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster by State-MSA	Yes	Yes	Yes	Yes	Yes
N	12,556	12,556	12,556	12,556	12,556
R^2	0.607	0.269	0.192	0.152	0.555

Note: Table presents county-level analysis of small business activities following Hurricane Sandy. We estimate:

$$\begin{aligned} y_{jt} = \gamma_0 &+ \sum_t \gamma_{1t} \mathrm{I}_t(\text{Year}) \times \mathrm{I}_j(\text{Sandy Disaster Area}) \\ &+ \sum_t \gamma_{2t} \mathrm{I}_t(\text{Year}) \times \mathrm{I}_j(\text{Outside Disaster Area, NY/NJ/CT}) \\ &+ \mathrm{FE}_m + \mathrm{FE}_t + \varepsilon_{jt}. \end{aligned} \tag{A.2}$$

where j indexes counties and t indexes year. March 2012 serves as reference period. The models include year fixed effects and state-MSA fixed effects. Disaster area represents counties eligible for federal aid in the presidential disaster declaration DR-4085-NY, DR-4086-NJ, and DR-4087-CT for Hurricane Sandy. Regressions report robust standard errors clustered by state-MSA. Data are from Business Dynamics Statistics and Nonemployer Statistics of the U.S. Census Bureau (2018a,b). The data include sole proprietors with no paid employees and establishments of firms with fewer than 500 employees. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.3: Post-Katrina Trends: Small Business Activities

	Establishments				
	(1) Total Number	(2) Entry Rate	(3) Exit Rate	(4) Death Rate	(5) Employment
I(Katrina Disaster Area) ×					
I(One Year After)	−247.158	0.005*	0.018	0.009	−1,656.876
	(384.206)	(0.003)	(0.015)	(0.009)	(1,012.704)
I(Two Years After)	−348.512	0.020**	−0.009***	−0.005**	−1,111.076
	(419.381)	(0.008)	(0.003)	(0.002)	(938.074)
I(Three Years After)	−116.655	0.007***	−0.003	0.001	−912.544
	(443.280)	(0.002)	(0.003)	(0.003)	(942.750)
State-MSA FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster by State-MSA	Yes	Yes	Yes	Yes	Yes
N	12,568	12,568	12,568	12,568	12,568
R^2	0.611	0.368	0.249	0.269	0.572

Note: Table presents county-level analysis of small business activities following Hurricane Katrina. We estimate:

$$
\begin{aligned}
y_{jt} = \gamma_0 &+ \sum_t \gamma_{1t} \mathrm{I}_t(\text{Year}) \times \mathrm{I}_j(\text{Katrina Disaster Area}) \\
&+ \sum_t \gamma_{2t} \mathrm{I}_t(\text{Year}) \times \mathrm{I}_j(\text{Outside Disaster Area, LA/MS/AL}) \\
&+ \mathrm{FE}_m + \mathrm{FE}_t + \varepsilon_{jt}. \qquad \text{(A.3)}
\end{aligned}
$$

where j indexes counties and t indexes year. March 2005 serves as reference period. The models include year fixed effects and state-MSA fixed effects. Disaster area represents counties eligible for federal aid in the presidential disaster declaration DR-1603-LA, DR-1604-MS, and DR-1605-AL for Hurricane Katrina. Regressions report robust standard errors clustered by state-MSA. Data are from Business Dynamics Statistics and Nonemployer Statistics of the U.S. Census Bureau (2018a,b). The data include sole proprietors with no paid employees and establishments of firms with fewer than 500 employees. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.4: Share of Balances that are Impaired, Full Sample

	PctImpaired						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(Post-Harvey) ×							
I(No Flood, Disaster Area)	0.013**	0.013**	0.010*	0.024***	0.024***	0.020***	0.029***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
I(Flood 1st Tercile)	0.020***	0.020***	0.019***	0.033***			
	(0.006)	(0.006)	(0.006)	(0.007)			
I(Flood 2nd Tercile)	0.025***	0.025***	0.024***	0.038***			
	(0.008)	(0.008)	(0.008)	(0.009)			
I(Flood 3rd Tercile)	0.030***	0.030***	0.027***	0.041***			
	(0.008)	(0.008)	(0.008)	(0.009)			
I(Flooded)					0.037***		
					(0.007)		
ln(Flood Depth)						0.024***	
						(0.005)	
I(Flooded, Remote)							0.031***
							(0.009)
I(TX)				−0.022***	−0.022***	−0.018**	−0.022***
				(0.008)	(0.008)	(0.007)	(0.008)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Firms	8,219	8,219	8,219	8,219	8,219	8,219	8,219
Firm-Year Obs	16,438	16,438	16,438	16,438	16,438	16,438	16,438
R^2	0.001	0.735	0.736	0.736	0.736	0.736	0.736

Note: Table reports the full sample estimation results of impairment analysis. Dependent variable is the share of loan balances that are not paid on time within the agreed terms for a firm's continuously reported loans ($PctImpaired_i$). Column 1 shows the results without any fixed effects; the regression in Column 2 includes firm fixed effects; Column 3 adds in control variables. Our preferred model is in Column 4, in which I also include an indicator for firms located in Texas to control for any potential systemic differences between these firms and those in other states. Disaster area represents the 41 counties that were declared the disaster area eligible for federal aid in the presidential disaster declaration. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.5: Delinquencies, Collections, and Legal Filings, Full Sample

	PctDelinquent					
	(1) 1-30 days	(2) 31-60 days	(3) 61-90 days	(4) 90+ days	(5) ln(Collection)	(6) ln(Legal)
I(Post-Harvey) ×						
I(No Flood, Disaster Area)	0.011**	0.002	0.005**	0.005	0.0005	−0.031
	(0.005)	(0.003)	(0.002)	(0.004)	(0.032)	(0.022)
I(Flood 1st Tercile)	0.011**	0.010***	0.007***	0.005	0.087**	−0.001
	(0.005)	(0.003)	(0.002)	(0.004)	(0.041)	(0.028)
I(Flood 2nd Tercile)	0.018***	0.006*	0.009***	0.005	−0.098**	−0.037
	(0.006)	(0.004)	(0.003)	(0.004)	(0.040)	(0.027)
I(Flood 3rd Tercile)	0.025***	−0.0002	0.011***	0.005	0.024	−0.042
	(0.005)	(0.004)	(0.003)	(0.006)	(0.049)	(0.035)
I(TX)	−0.012**	−0.001	−0.004	−0.005	0.024	0.011
	(0.005)	(0.003)	(0.003)	(0.005)	(0.034)	(0.029)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes
No. of Firms	8,219	8,219	8,219	8,219	8,219	8,219
Firm-Year Obs	16,438	16,438	16,438	16,438	16,438	16,438
R^2	0.656	0.545	0.660	0.798	0.919	0.946

Note: Table reports the full sample estimation results of impairment decomposition analysis. Dependent variables from Column 1 to 4 are the share of a firm's continuously reported loan balances that is delinquent ($PctDelinquent_i$) at four different levels: 1-30 days delinquent, 31-60 days delinquent, 61-90 days delinquent, and over 90 days delinquent. Dependent variable in Column 5 is the logged amount placed for collections in the last seven years ($ln(Collections_i)$). Dependent variable in Column 6 is the logged liability amount of legal filings (*i.e.*, tax liens and judgments) in the last seven years ($ln(Legal_i)$). The models include firm fixed effects, year fixed effects, and control variables. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.6: Ratio of Impaired Loans, Number of Loans

		PctDelinquent, Number of Loans				
	(1)	(2)	(3)	(4)	(5)	(6)
	PctImpaired	1-30 days	31-60 days	61-90 days	90+ days	Derogatory
I(Post-Harvey) ×						
I(No Flood, Disaster Area)	0.014	0.023	−0.013	−0.0001	0.005	−0.001**
	(0.015)	(0.016)	(0.008)	(0.008)	(0.010)	(0.001)
I(Flood 1st Tercile)	0.068***	0.044**	0.019**	0.005	−0.001	0.001
	(0.023)	(0.019)	(0.009)	(0.007)	(0.011)	(0.001)
I(Flood 2nd Tercile)	0.068***	0.041**	0.025***	−0.001	0.001	0.001
	(0.018)	(0.017)	(0.010)	(0.009)	(0.011)	(0.001)
I(Flood 3rd Tercile)	0.068***	0.042***	0.022**	0.009	−0.003	−0.002
	(0.019)	(0.016)	(0.010)	(0.007)	(0.009)	(0.002)
I(TX)	−0.002	−0.014	0.002	0.003	0.007	0.0001
	(0.019)	(0.016)	(0.008)	(0.008)	(0.011)	(0.0003)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes
No. of Firms	2,614	2,614	2,614	2,614	2,614	2,614
Firm-Year Obs	5,228	5,228	5,228	5,228	5,228	5,228
R^2	0.774	0.670	0.579	0.677	0.835	0.971

Note: Dependent variable in Column 1 is the the number of loans that are not paid on time within the agreed terms divided by the total number of loans for a firm. Dependent variables from Column 2 to 6 are the ratio of a firm's number of loans that are delinquent at five different levels: 1-30 days delinquent, 31-60 days delinquent, 61-90 days delinquent, over 90 days delinquent, and having derogatory comments. Derogatory comments here include bankruptcy, judgment, lien, etc. The mean and median number of loans for a firm is 3.5 and 2, respectively. The models include time fixed effects, firm fixed effects, and control variables. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.7: Summary Statistics: Firms with Parents

Variable	Total	Outside	No Flood, Disaster Area	Flooding 1st Tercile (1, 1.69 ft]	Flooding 2nd Tercile (1.69, 2.68 ft]	Flooding 3rd Tercile >2.68 ft
Full Sample with Parents						
No. of Firms	1,173	443	456	89	82	103
Employees	18.91	21.17	18.41	17.15	16.30	15.06
	[6]	[7]	[6]	[5]	[5]	[6]
	(43.30)	(50.61)	(40.22)	(29.05)	(44.32)	(29.83)
Total Balance ($)	37,479	69,795	14,910	7,622	6,926	52,830
	[0]	[0]	[0]	[0]	[0]	[0]
	(346,793)	(476,341)	(174,616)	(36,368)	(41,772)	(501,788)
Active Borrower Sample with Parents						
No. of Firms	313	134	125	22	13	19
Employees	37.58	39.89	36.22	29.50	42.15	36.58
	[10]	[10]	[10]	[13]	[9]	[16]
	(73.16)	(82.73)	(65.68)	(39.07)	(101.05)	(61.23)
Total Balance ($)	140,245	227,077	54,262	30,777	43,685	286,358
	[3,100]	[4,600]	[3,100]	[1,100]	[1,300]	[3,200]
	(661,295)	(847,269)	(331,257)	(69,254)	(100,191)	(1,164,573)
PctImpaired	0.16	0.18	0.15	0.07	0.11	0.18
	[0.01]	[0.05]	[0.00]	[0.00]	[0.00]	[0.00]
	(0.26)	(0.28)	(0.25)	(0.15)	(0.24)	(0.30)

Notes: Sample includes businesses with fewer than 500 employees that have parents. The values in the first, second, and third rows under each variable are means, [medians], and (standard deviations), respectively. Active borrowers include firms that have positive loan balances on both June 30, 2017 and June 30, 2018. These filters create a smaller "Active Borrower Sample". All variables are from the firm's credit report on June 30, 2017.

Table A.8: Post-Harvey Effects: DBT, Inquiries, and Balances

	Monthly DBT	Quarterly Inquiries			ln(Monthly Balances)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(Post-Harvey) ×							
I(No Flood, Disaster Area)	1.138*	0.014*	−0.002	0.064*	−0.055	0.042	−0.344***
	(0.668)	(0.009)	(0.007)	(0.034)	(0.046)	(0.037)	(0.098)
ln(Flood Depth)	0.829*	0.030**	0.002	0.122***	0.014	0.112***	−0.389***
	(0.472)	(0.013)	(0.008)	(0.030)	(0.042)	(0.040)	(0.082)
I(TX)	−0.020	−0.015	−0.0003	−0.062	0.005	−0.030	0.141
	(0.931)	(0.017)	(0.010)	(0.056)	(0.055)	(0.053)	(0.133)
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Active Borrowers	Full	Non-Borrowers	Borrowers	Full	Non-Borrowers	Borrowers
Firm-Time Obs	24,775	49,314	36,420	12,894	98,628	66,770	23,639
R^2	0.793	0.583	0.325	0.590	0.919	0.594	0.790

Note: Dependent variables from Column 1 are monthly DBT from January to June for 2017 and from January to June 2018. Dependent variables from Column 2 to 4 are quarterly inquiries from Q4 2016 to Q2 2017 and from Q4 2017 to Q2 2018. Dependent variables from Column 5 to 7 are logged monthly balances from January to June for 2017 and from January to June 2018. For DBT analysis in Column 1, I only keep firm observations in a particular month with positive total balances. For analyses of inquiries and balances from Column 2 to 7, I study the full sample and divide them into two groups: firms with zero balances as of January 2017 ("non-borrowers") and those with positive balances at that date ("borrowers"). For this reason, I drop January 2017 in Columns 6 and 7. I estimate standard treatment effects regressions as in Equation (1.4). The models include time fixed effects and firm fixed effects. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.9: Post-Harvey Effects: DBT, Inquiries, and Balances by Flood Terciles

	Monthly DBT	Quarterly Inquiries			ln(Monthly Balances)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(Post-Harvey) ×							
I(No Flood, Disaster Area)	1.616*	0.011	0.002	0.036	0.040	0.139***	−0.309**
	(0.868)	(0.012)	(0.009)	(0.042)	(0.053)	(0.038)	(0.143)
I(Flood 1st Tercile)	1.616	−0.002	0.009	−0.044	0.167**	0.179***	−0.097
	(1.325)	(0.016)	(0.016)	(0.048)	(0.072)	(0.050)	(0.178)
I(Flood 2nd Tercile)	1.389	0.006	−0.003	0.035	0.219***	0.262***	−0.052
	(0.923)	(0.020)	(0.008)	(0.070)	(0.084)	(0.082)	(0.171)
I(Flood 3rd Tercile)	1.247	0.058***	0.018	0.179***	0.074	0.270***	−0.645***
	(0.941)	(0.022)	(0.015)	(0.059)	(0.077)	(0.060)	(0.220)
I(TX)	−0.462	−0.012	−0.004	−0.035	−0.082	−0.118**	0.106
	(1.001)	(0.019)	(0.011)	(0.062)	(0.061)	(0.055)	(0.162)
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Active Borrowers	Full	Non-Borrowers	Borrowers	Full	Non-Borrowers	Borrowers
Firm-Time Obs	24,775	49,314	36,420	12,894	98,628	66,770	23,639
R^2	0.793	0.583	0.325	0.590	0.919	0.594	0.790

Note: Dependent variables from Columns 1 & 3 are monthly DBT & logged monthly balances from January to June for 2017 and from January to June 2018. Dependent variables from Column 2 to 4 are quarterly inquiries from Q4 2016 to Q2 2017 and from Q4 2017 to Q2 2018. For DBT analysis in Column 1, I only keep firm observations in a particular month with positive total balances. For analyses of inquiries and balances from Columns 2 to 7, I study the full sample and divide them into two groups: firms with zero balances as of January 2017 ("non-borrowers") and those with positive balances at that date ("borrowers"). For this reason, I drop January 2017 in Columns 6 and 7. I estimate standard treatment effects regressions as in Equation (1.4). Since the dependent variables are logged values of total loan balances from Column 5 to 7, the most accurate interpretation of the flood effect in each tercile is to exponentiate the coefficients and then subtract one. For example, the coefficient on I_t(Post-Harvey) $\times$ I_i(Flood 3rd Tercile) is 0.27 for non-borrowers, which indicates that Harvey caused an increase of $(e^{0.27} - 1) \times 100 = 31\%$ in loan balances. Similarly, Harvey caused a decrease of $|(e^{-0.645} - 1) \times 100| = 48\%$ in loan balances for borrowers in the most flooded tercile. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

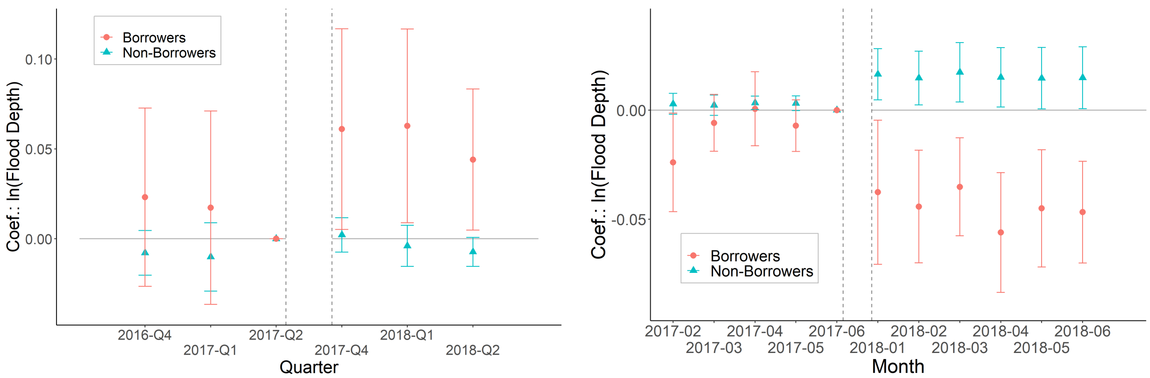
Table A.10: Post-Harvey Effects: Probability of Having Inquiries and Balances

	I(Quarterly Inquiries)			I(Monthly Balances)		
	(1)	(2)	(3)	(4)	(5)	(6)
I(Post-Harvey) ×						
I(No Flood, Disaster Area)	0.006	0.004	0.014	−0.005	0.007	−0.038***
	(0.004)	(0.004)	(0.016)	(0.007)	(0.006)	(0.013)
ln(Flood Depth)	0.013**	0.003	0.043***	0.004	0.013**	−0.037***
	(0.006)	(0.004)	(0.015)	(0.006)	(0.006)	(0.012)
I(TX)	−0.005	−0.004	−0.006	0.001	−0.004	0.016
	(0.007)	(0.005)	(0.020)	(0.007)	(0.007)	(0.017)
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by County	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Non-Borrowers	Borrowers	Full	Non-Borrowers	Borrowers
Firm-Time Obs	49,314	36,420	12,894	98,628	66,770	23,639
R^2	0.460	0.294	0.461	0.893	0.587	0.543

Note: Dependent variables from Column 1 to 3 are dummies indicating whether a firm had any quarterly inquiries from Q4 2016 to Q2 2017 and from Q4 2017 to Q2 2018. Dependent variables from Column 4 to 6 are dummies indicating whether a firm had any monthly balances from January to June for 2017 and from January to June 2018. For both analyses, I study the full sample and divide them into two groups: firms with zero balances as of January 2017 ("non-borrowers") and those with positive balances at that date ("borrowers"). For this reason, I drop January 2017 in Columns 5 and 6. I estimate standard treatment effects regressions as in Equation (1.4). The models include time fixed effects and firm fixed effects. Regressions report robust standard errors clustered by county. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

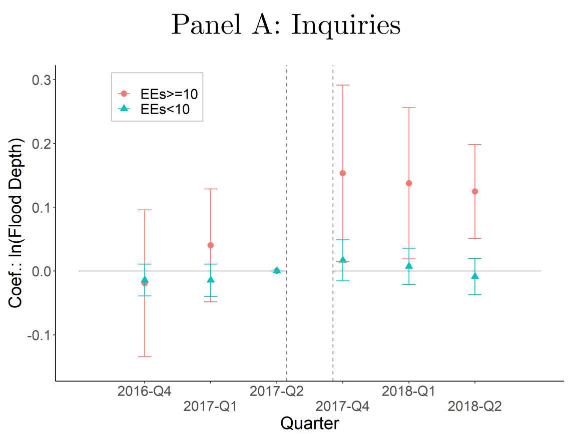
Figure A.3: Evolution in Probability of Having Inquiries and Balances

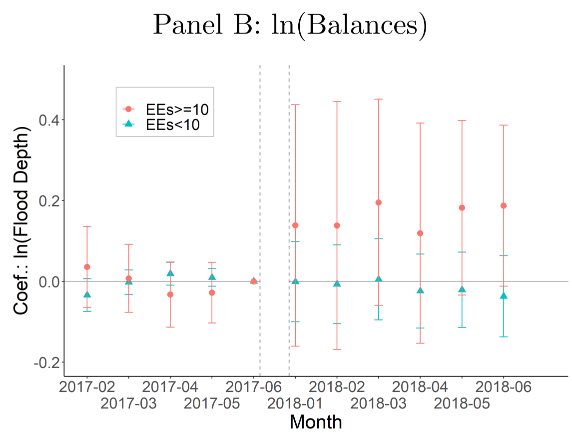
Panel A: Probability of Having Inquiries Panel B: Probability of Having Balances



Note: I explore the probability of having any inquiries and the probability of having any debt balances following Hurricane Harvey. The figure in Panel A plots 95% confidence interval of event study coefficients of the probability of having quaterly inquires on the logged flood depth. The quarterly before Harvey, Q2 2017, is the reference period. The vertical, dashed lines mark the period Q3 2017, during which I do not observe quarterly inquiries. Harvey occurred during that period. The figure in Pabel B plots 95% confidence interval of event study coefficients of the probability of having monthly credit balances on the logged flood depth. June 2017 serves as the reference period. The vertical, dashed lines mark the period July to December 2017, during which I do not observe monthly DBT and balances. Harvey occurred during that period. The regression models follow Equation (1.5).

Figure A.4: Evolution in Inquiries and Balances by Firm Size



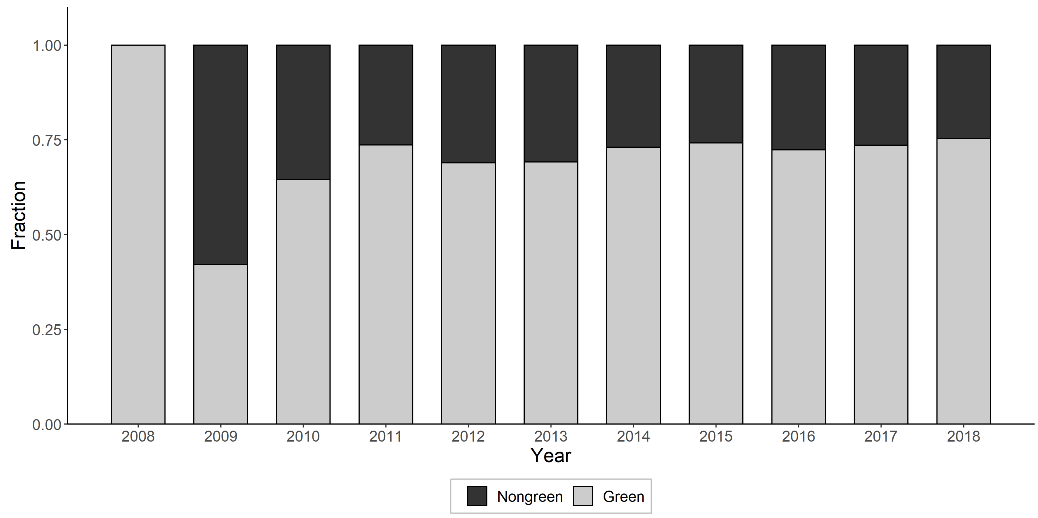


Note: I divide the full sample into two groups: firms with fewer than 10 employees and those with at least 10 employees. Panel A shows the evolution of quarterly inquiries. The figure plots the 95% confidence interval of event study coefficients of quarterly inquiries on the logged flood depth. The quarter before Harvey, Q2 2017, is the reference period. The vertical dashed lines mark the period Q3 2017, during which I do not observe quarterly inquiries. Harvey occurred during that period. Panel B shows the evolution of monthly balances. The figure plots the 95% confidence interval of event study coefficients of logged monthly credit balances on the logged flood depth. Coefficients can be interpreted as the effect of flooding relative to the firms outside the disaster area and relative to June 2017. The vertical dashed lines mark the period July to December 2017, during which I do not observe monthly DBT and balances. Harvey occurred during that period. The regression models follow Equation (1.5).

APPENDIX B

APPENDIX FOR CHAPTER 2

Figure B.1: Fraction of Plans in Nongreen Zone



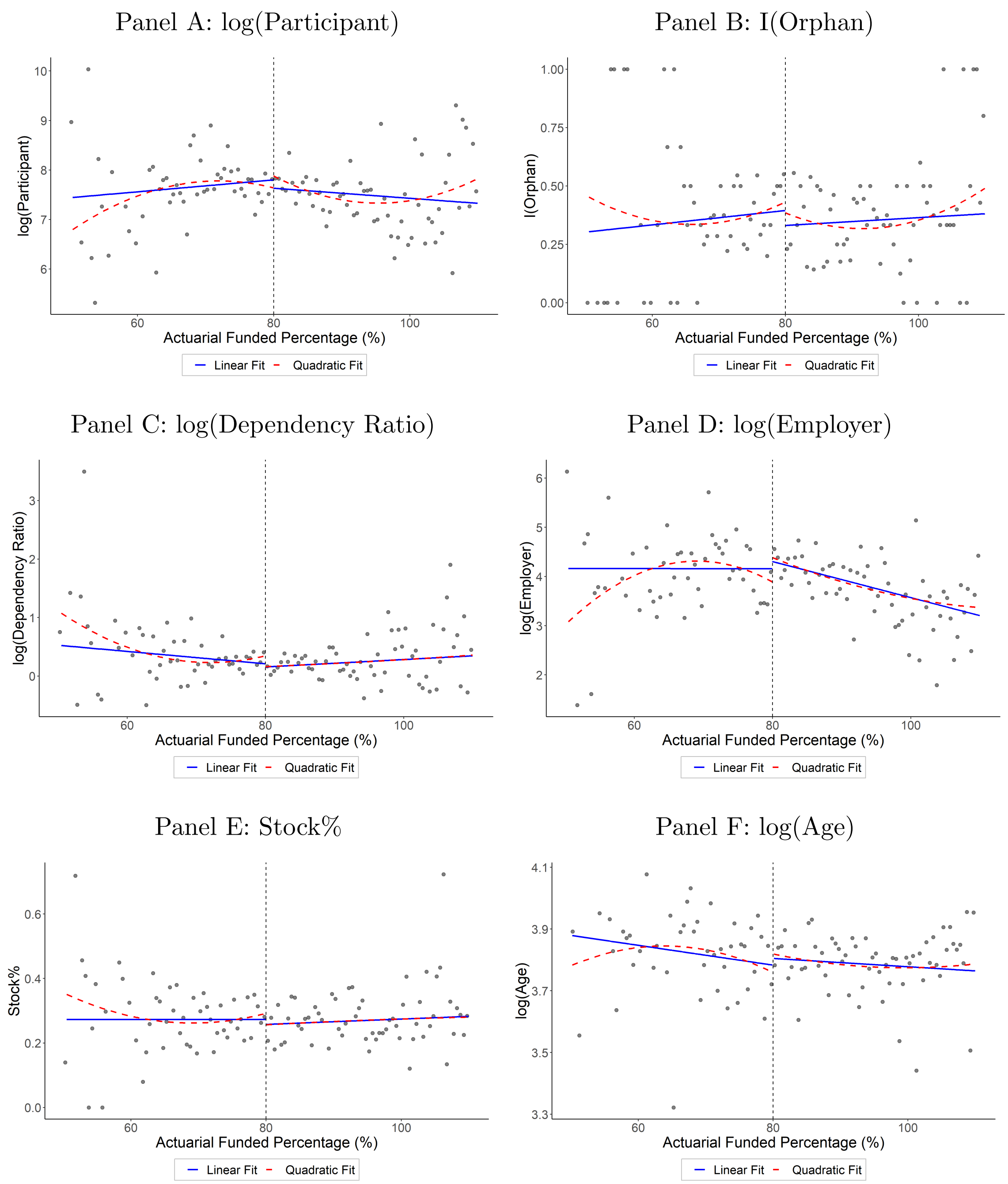
Note: This table presents the fraction of plans being certified in green zone and nongreen zone in the full sample of 971 multiemployer DB pension plans.

Table B.1: Test for Covariate Continuity Around Funded Percentage Cutoff

	2009			2010		
FP Window	[0.5, 0.8)	[0.8, 1.1]	p-value	[0.5, 0.8)	[0.8, 1.1]	p-value
Panel A. Plan Characteristics						
Participant	8,977.04	7,803.18	0.58	10,293.31	7,684.89	0.79
	(29,936.50)	(38,669.66)		(35,755.02)	(35,648.78)	
No. of ER	259.38	181.84	0.24	252.74	195.71	0.16
	(762.04)	(544.05)		(650.73)	(631.28)	
Dependency Ratio	1.80	1.80	0.13	2.82	1.89	0.08
	(2.45)	(3.06)		(6.65)	(2.36)	
I(Orphan)	0.37	0.35	0.34	0.35	0.31	0.22
	(0.48)	(0.48)		(0.48)	(0.46)	
Age	44.36	43.55	0.40	45.48	44.76	0.22
	(8.69)	(9.55)		(9.52)	(9.28)	
Stock%	0.27	0.27	0.10	0.27	0.29	0.21
	(0.19)	(0.19)		(0.21)	(0.21)	
Panel B. Industry Characteristics						
Employment	7,622,028.80	7,488,273.38	0.27	6,698,254.58	6,965,774.88	0.49
	(2,507,987.26)	(2,290,157.94)		(2,154,429.19)	(2,280,473.61)	
Union Covered Employment%	0.12	0.11	0.97	0.12	0.11	0.09
	(0.08)	(0.08)		(0.08)	(0.08)	
SUR χ^2 test			0.26			0.47
No. of Plans	361	543		194	689	

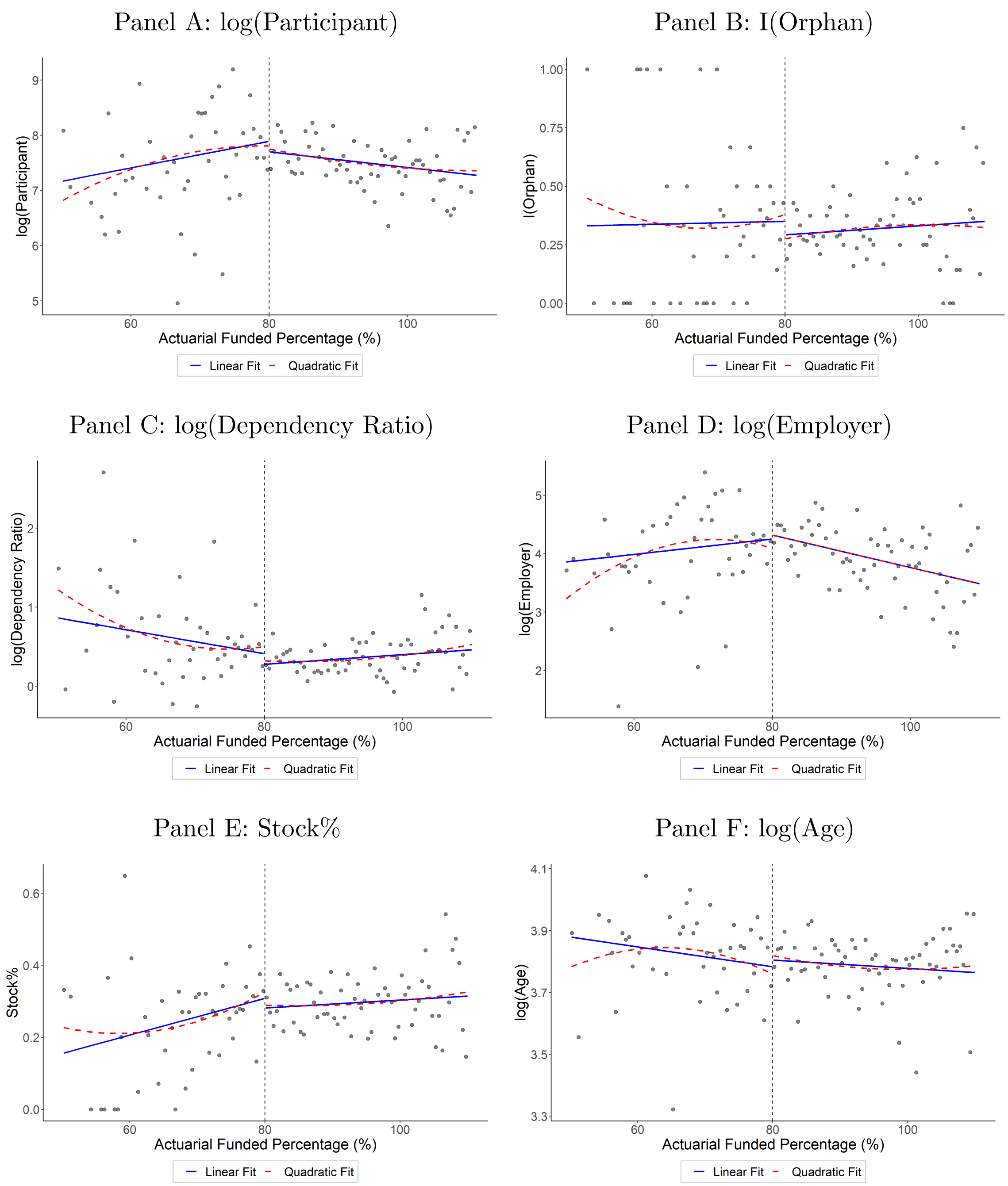
Note: Columns 1-2 and 4-5 report variable means and (standard deviations) within funded percentage windows. Columns 3 and 6 report the p-values of discontinuity estimators (α) from the following second-polynomial regressions: $y_i = \alpha I(\text{FP} < 0.8) + \theta_1(\text{FP}_i - 0.8) + \theta_2(\text{FP}_i - 0.8) \times I(\text{FP}_i < 0.8) + \theta_3(\text{FP}_i - 0.8)^2 + \theta_4(\text{FP}_i - 0.8)^2 \times I(\text{FP}_i < 0.8) + \nu_i$, where y_i represents each covariate and FP represents a plan's actuarial funded percentage. Regressions are clustered by industry. In the second last row, I conduct a Seemingly Unrelated Regression (SUR) by combining each equation with different baseline covariates as dependent variables and perform a single χ^2 testing for all α being zeros. This is to address the multiple testing issue arising from the misspecification of the functional form of the running variable (Lee and Lemieux, 2010).

Figure B.2: Covariates Around Cutoff, 2009 Plan Year



Note: Selected baseline plan characteristics around the cutoff of 80% funded percentage. Each dot corresponds to the mean value of plan characteristic in each bin. Bin size = 0.5 percentage point. Curves plot the predicted values of plan characteristic from linear and quadratic functions of the funded percentage.

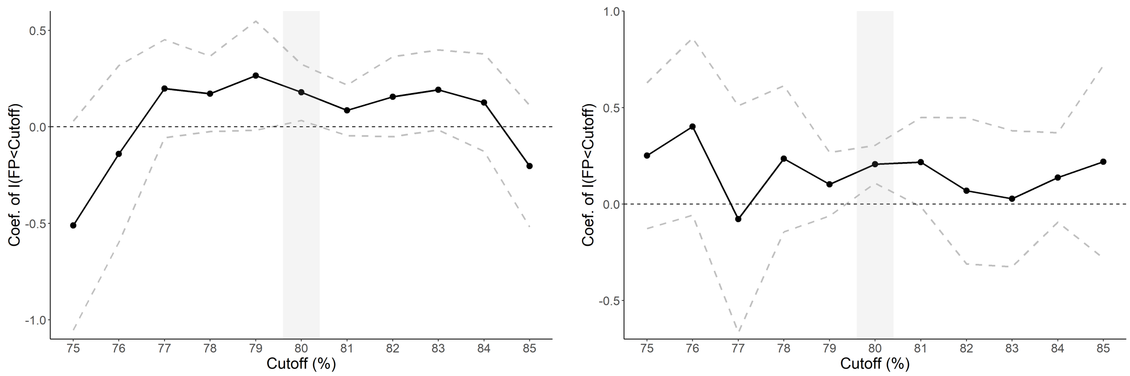
Figure B.3: Covariates Around Cutoff, 2010 Plan Year



Note: Selected baseline plan characteristics around the cutoff of 80% funded percentage. Each dot corresponds to the mean value of plan characteristic in each bin. Bin size = 0.5 percentage point. Curves plot the predicted values of plan characteristic from linear and quadratic functions of the funded percentage.

Figure B.4: Placebo Tests of Discontinuity in Nongreen Zone Certification

Panel A: 2009 Plan Year Panel B: 2010 Plan Year



Note: Figures plot the discontinuity estimates and 95% confidence intervals of γ_1 in Equation (2.2), by replacing the actual cutoff of 80% funded percentage with a series of "false" cutoffs over the range between 75% and 85%.

Table B.2: Effects of Nongreen Zone Certification on Two-Year Withdrawal Frequency

	2009		2010	
	(1)	(2)	(3)	(4)
Nongreen	0.122	0.022	0.247**	0.173*
	(0.362)	(0.254)	(0.114)	(0.091)
Controls	No	Yes	No	Yes
Observations	971	971	971	971

Note: Dependent variable is a dummy variable indicating whether a multiemployer pension plan had experienced employer withdrawals during a two-year period. Table presents the two-stage least square (2SLS) instrumental variable regression coefficient estimates. Our preferred model is in Columns 2 and 4, in which I also control for plan-level and industry-level characteristics. Regressions report robust standard errors clustered by industry. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table B.3: IV Estimates: Robustness Check

	Full		+/−7		+/−6		+/−5	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2009 Plan Year								
Parametric	0.059	−0.016	−0.058	2.511	−0.003	1.392	0.103	0.002
Quadratic	(0.292)	(0.235)	(0.710)	(11.186)	(0.979)	(5.255)	(1.143)	(0.597)
Parametric	0.089	−0.003	−0.322	−0.433	−0.194	−0.489	−0.098	−0.386
Linear	(0.203)	(0.137)	(0.502)	(0.489)	(0.584)	(0.774)	(0.716)	(1.030)
Nonparametric	0.075	−0.005	−0.193	−0.621	−0.080	−0.619	−0.068	−0.987
Linear	(0.216)	(0.148)	(0.701)	(0.900)	(0.783)	(1.137)	(0.979)	(2.104)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	971	971	423	423	371	371	302	302
Panel B: 2010 Plan Year								
Parametric	0.206*	0.140**	0.672	0.974	0.938*	0.965*	1.190*	0.843**
Quadratic	(0.111)	(0.069)	(0.669)	(0.667)	(0.533)	(0.543)	(0.635)	(0.346)
Parametric	0.167***	0.094*	0.620**	0.476**	0.528*	0.515**	0.773**	0.780***
Linear	(0.060)	(0.054)	(0.303)	(0.210)	(0.308)	(0.228)	(0.336)	(0.292)
Nonparametric	0.187***	0.120*	0.684**	0.660***	0.710**	0.713***	0.765*	0.730**
Linear	(0.066)	(0.061)	(0.312)	(0.226)	(0.344)	(0.273)	(0.402)	(0.338)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	971	971	348	348	312	312	259	259

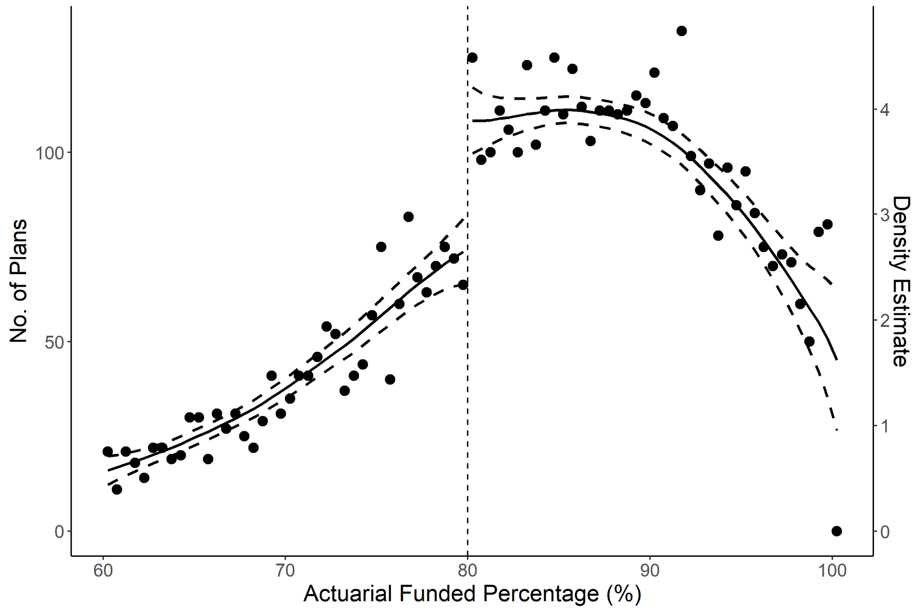
Note: Dependent variable is a dummy variable indicating whether a multiemployer pension plan had experienced employer withdrawals within the plan year. Two-stage least square (2SLS) instrumental variable regression coefficient estimates under the FRD design are presented in each column by applying to difference samples. For example, the +/-5 sample in Columns (9) & (10) only include plans with a funded percentage between 75% and 85%, a 5 percentage point on each side of the cutoff. Regressions report robust standard errors clustered by industry. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table B.4: Funding: Post-Financial Crisis

	Actuarial Funded Percentage	Current Funded Percentage
	(1)	(2)
log(Participant)	0.053***	0.024*
	(0.018)	(0.014)
log(Dependency Ratio)	−0.034***	−0.015**
	(0.009)	(0.006)
Stock%	0.041***	0.017
	(0.016)	(0.010)
Expense-to-Asset Ratio	−1.052**	−0.620**
	(0.496)	(0.275)
log(Age)	−0.027	−0.047
	(0.042)	(0.033)
I($Orphan_{2008}$)		
× I(2009)	0.003	−0.008
	(0.006)	(0.005)
× I(2010)	0.007	−0.006
	(0.007)	(0.005)
× I(2011)	0.004	−0.011**
	(0.007)	(0.006)
× I(2012)	−0.003	−0.013**
	(0.008)	(0.006)
× I(2013)	−0.009	−0.016**
	(0.009)	(0.006)
× I(2014)	−0.016*	−0.022***
	(0.009)	(0.007)
× I(2015)	−0.026***	−0.018**
	(0.010)	(0.008)
× I(2016)	−0.024**	−0.018**
	(0.011)	(0.008)
× I(2017)	−0.029**	−0.022***
	(0.011)	(0.008)
× I(2018)	−0.042***	−0.022***
	(0.012)	(0.008)
Plan FE	Yes	Yes
Year FE	Yes	Yes
Observations	10,230	10,230
R^2	0.865	0.907

Note: The reference group is multiemployer pension plan with no orphan participants as of the beginning of 2008. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Figure B.5: McCrary Test: 2011-2018



Note: Figures plot number of multiemployer pension plans (in dots) in bins of actuarial funded percentages (running variable). Bin size = 0.5 percentage point. The curve plots local linear density estimator and its 95% confidence interval following McCrary (2008).

Table B.5: Economies of Scale: Administrative Expenses

	log(Admin Expenses Per Participant)		
	(1)	(2)	(3)
I(Multiemployer)	1.922***	2.025***	
	(0.091)	(0.092)	
log(Participant)	−0.461***	−0.404***	−0.665***
	(0.016)	(0.018)	(0.041)
log(Participant) × I(Multiemployer)	−0.197***	−0.208***	−0.240***
	(0.012)	(0.012)	(0.058)
log(Dependency Ratio)	0.389***	0.358***	0.203***
	(0.025)	(0.025)	(0.030)
Plan FE	No	No	Yes
Year FE	No	Yes	Yes
Observations	73,733	73,733	73,733
R^2	0.128	0.152	0.764

Note: Sample includes both multiemployer and single-employer pension plans from 2008 to 2018. Administrative expenses consist of professional fees, contract administrator fees, premium payments to PBGC, and other plan expenditures such as salaries and other compensation and allowances. Investment expenses are excluded. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

APPENDIX C

APPENDIX FOR CHAPTER 3

There are two insurers competing in the market, insurer i and insurer j. At time 1, insurer i has assets A_1^i and preexisting liabilities (maturing at time 2) with a face value of L_1^i. Insurer i makes decisions on issuing new equities E_2^i and new policies Q^i (maturing at time 2) with price p_2^i.

The insurer's objective is:

$$\max_{p_2^i, E_2^i} C_2^i - (E_2^i + C_1^i) = Q^i[p_2^i - e^{-r} + b(x_2^i)] + L_1^i[b(x_2^i) - b(x_1^i)] \tag{C.1}$$

where $b()$ represents default put option per dollar of liabilities; $x_1^i = A_1^i/L_1^i$; $x_2^i = (A_1^i + E_2^i + p_2^i Q^i)/(L_1^i + Q^i)$.

Solving the first order conditions (FOCs) with respect to the price of new policies p_2^i and the newly issued equity E_2^i, respectively, we obtain:

$$\begin{aligned} &\left[Q^i + (Q_p^i + Q_{p^j}^i \frac{\partial p_2^j}{\partial p_2^i} + Q_{b^j}^i b_x^j x_p^j \frac{\partial p_2^j}{\partial p_2^i})[p_2^i - e^{-r} + b(x_2^i)]\right] \\ &\qquad + \left[Q_b^i[p_2^i - e^{-r} + b(x_2^i)] + Q^i + L_1^i\right] b_x^i x_e^i = 0. \end{aligned} \tag{C.2}$$

$$\left[Q_b^i[p_2^i - e^{-r} + b(x_2^i)] + Q^i + L_1^i\right] b_x^i x_p^i = 0. \tag{C.3}$$

where Q_p^i, $Q_b^i = \frac{\partial Q^i}{\partial p_2^i}, = \frac{\partial Q^i}{\partial b^i}$, respectively; b_x^i, x_p^i, $x_e^i = \frac{\partial b^i}{\partial x^i}, = \frac{\partial x^i}{\partial p_2^i}, = \frac{\partial x^i}{\partial E_2^i}$, respectively; b_x^j, x_p^j, $x_e^j = \frac{\partial b^j}{\partial x^j}, = \frac{\partial x^j}{\partial p_2^j}, = \frac{\partial x^j}{\partial E_2^j}$, respectively.

From Eq. (C.3), we know that:

$$Q_b^j[p_2^i - e^{-r} + b(x_2^i)] + Q^i + L_1^i = 0. \tag{C.4}$$

By plugging Eq. (C.4) into Eq. (C.2), we have:

$$Q^i + \left(Q^i_p + Q^i_{p^j} \frac{\partial p^j_2}{\partial p^i_2} + Q^i_{b^j} b^j_x x^j_p \frac{\partial p^j_2}{\partial p^i_2} \right) \left[p^i_2 - e^{-r} + b(x^i_2) \right] = 0. \tag{C.5}$$

Proof for Eq. (3.6)**.** Solving the FOC with respect to L^j_1 on both sides of Eq. (C.5), we have:

$$\begin{aligned} &\left[Q^i_{pb^j} + \frac{\partial p^j_2}{\partial p^i_2} \left(Q^i_{p^j b^j} + Q^i_{b^j b^j} b^j_x x^j_p \right) \right] [p^i_2 - e^{-r} + b(x^i_2)] b^j_x x^j_{L_1} \\ &\qquad + D \frac{\partial p^i_2}{\partial L^j_1} + Q^i_{b^j} b^j_x x^j_{L_1} = 0 \end{aligned} \tag{C.6}$$

where $x^j_{L_1} = \frac{\partial x^j}{\partial L^j_1}$; D is the second derivatives of the objective function with respect to insurer i's price p^i_2; to ensure there exists a local maximum, it is required that $D < 0$.

By rearranging Eq. (C.6), we thus complete the proof for Eq. (3.6):

$$\frac{\partial p^i_2}{\partial L^j_1} = \frac{Q^i_{b^j} + \left[Q^i_{pb^j} + \frac{\partial p^j_2}{\partial p^i_2} \left(Q^i_{p^j b^j} + Q^i_{b^j b^j} b^j_x x^j_p \right) \right] [p^i_2 - e^{-r} + b(x^i_2)]}{-D / b^j_x x^j_{L_1}} \tag{C.7}$$

where $Q^i_{b^j} > 0$, $\frac{\partial p^j_2}{\partial p^i_2} > 0$, $Q^i_{b^j b^j} b^j_x x^j_p > 0$, $p^i_2 - e^{-r} + b(x^i_2) > 0$, $-D/b^j_x x^j_{L_1} > 0$. As a result, the sign of Eq. (C.6) (or Eq. (3.6) in Chapter 3) depends on the sign of $Q^i_{pb^j}$ and $Q^i_{p^j b^j}$.

Table C.1: Summary Statistics: Homeowners/Farmowners Writers

	Homeowners/ Farmowners Writers		Homeowners/ Farmowners *only* Writers		Homeowners/Farmowners *and* Commercial-Line Writers ("Rivals")	
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) Mean	(6) SD
Firm-state-level Variables						
Homeowners/Farmowners Price$_t$	2.152	1.259	2.153	1.264	2.152	1.259
Homeowners/Farmowners Market Share$_t$	0.012	0.031	0.013	0.018	0.013	0.033
Homeowners/Farmowners Market Share$_{t-1}$	0.012	0.031	0.013	0.018	0.013	0.033
No. of firm-state-year obs.	79,841		5,339		74,502	
Firm-level Variables						
Commercial Shock$_{t-1}$	-0.003	0.032	0.000	0.000	-0.003	0.032
Surplus$_{t-1}$/Liability$_{t-2}$	0.756	0.399	0.841	0.508	0.715	0.363
Reinsurance Usage$_{t-1}$	0.451	0.303	0.814	0.330	0.427	0.282
Geographic HHI$_{t-1}$	0.608	0.361	0.677	0.374	0.561	0.359
Best's Rating$_{t-1}$ (A^- or above)	0.754	0.431	0.723	0.448	0.818	0.386
Log(Asset$_{t-1}$)	18.748	1.930	17.386	1.330	19.333	1.791
No. of firm-year obs.	13,942		1,139		12,803	

Note: Variables are winsorized at the 1st and 99th percentiles. To ensure that an insurer plays a non-trivial role in the product market, I only keep firms with at least 0.05 percent market share of personal lines in a state market. Commercial lines include all other lines except international and reinsurance. *Homeowners/Farmowners Price* is defined as premium earned divided by the present value of losses incurred from Homeowners/Farmowners lines. *Homeowners/Farmowners Market Share* is an insurer's share of direct premiums written among all Homeowners/Farmowners writers in a state market. *Commercial Shock* is defined as the one-year loss reserve development (LRD) from commercial lines, scaled by a firm's prior-year total liabilities. *Reinsurance usage* is defined as reinsurance ceded divided by the sum of direct premiums written and reinsurance assumed. *Geographic HHI* is defined as the sum of squares of the share of direct premiums written across 50 states plus the District of Columbia. *Best's Rating* is a dummy variable equals one of an insurer has a A^- or better rating, and zero otherwise. Stars *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.