# A Thesis on Entrepreneurship, Employment, and Investment Decisions

Xiao Cen

Submitted in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy
under the Executive Committee
of the Graduate School of Arts and Sciences

COLUMBIA UNIVERSITY

2020

# Abstract

A Thesis on Entrepreneurship, Employment, and Investment Decisions

Xiao Cen

This article investigates the incentive to pursue entrepreneurship and the impact of entrepreneurial activities on employment and investment decisions. Chapter 1 studies how household wealth affects entrepreneurial career choices, based on U.S. Census individual-level employment data and geographic information system (GIS) flood data. A regression discontinuity analysis shows that residents who were actually inundated are 7% less prone to entrepreneurial careers relative to their neighbors just outside the boundary, all within a 0.1-mile-wide band. The effect is more profound in the propensity to become startup employees than founders, attributed to a lowered self-insurance capacity against employment risks due to wealth damage. Chapter 2 investigates how the release of a mobile trading application affects retail investor and mutual fund investment. "Going mobile" raises investor attention and trading volume through aggravating behavioral biases. The shock boosts flow volatility, increases investor flow sensitivity to short-term fund returns and market sentiment, and makes fund performance suffer from heightened liquidity costs. Chapter 3 documents how intensified competition for talent from hedge fund startups affects mutual fund returns around a regulatory shock that stimulated hedge fund entry in China. The regulation leads to a decline in the performance in affected mutual funds, concentrated after managerial turnovers. The finding illustrates the adverse effect of business formation on established firms through stealing their talent and impairing their investment skills.

# Table of Contents

# List of Tables

# List of Figures

## Acknowledgements

I thank Xavier Giroud, Tomasz Piskorski, Kairong Xiao, and Vyacheslav Fos for their comments and help, and especially Wei Jiang for her encouragement, guidance, and support over the years. I also appreciate the comments of Simona Abis, Tania Babina, Geert Bekaert, Daniel Bergstresser (discussant), Olivier Dessaint (discussant), Kent Daniel, Robert Dekle (discussant) , Geoffrey Heal, Harrison Hong, Nan Li, Song Ma, Yiming Ma, Christopher J. Mayer, Ramana Nanda, Stijn G. Van Nieuwerburgh, Terrance Odean, Michaela Pagel, Jun Pan, Christopher Parsons (discussant), Giorgia Piacentino, Hongxun Ruan (discussant), Suresh Sundaresan, Paul Tetlock, Laura Veldkamp, Neng Wang, Sumudu W. Watugala (discussant), Shang-Jin Wei, and seminar participants at the USC Marshall Ph.D. Conference in Finance, HEC Paris Entrepreneurship Workshop, SFS Cavalcade conference, Northeastern University Finance Conference, University of Connecticut Financial Risk Conference, Chapman University Money and Finance Conference, Brandeis University, University of Houston, Texas A&M University, University of Pittsburgh, Northeastern University, New York University and various seminars at Columbia University. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program. The program was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. I thank the Iowa Flood Center for valuable data and assistance. The Finance Division, the Eugene Lang Entrepreneurship Center, and the W. Edwards Deming Center at Columbia Business School provided critical funding to support this research. Any opinions and conclusions expressed herein are those of the

author and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. The W. Edwards Deming Center and the Eugene Lang Entrepreneurship Center at Columbia provided critical funding to support this research.

# Chapter 1: Environmental Disasters, Household Wealth, and Entrepreneurial Career Choices

## 1.1 Introduction

New businesses play a central role in fueling innovation and productivity growth [1]. The success of a startup relies on the employees who commit themselves to an entrepreneurial career. Early employees are crucial to the formation of the organizational capital of a startup [2, 3, 4, 5, 6, 7], making hiring good employees one of the top priority for startup founders[1]. Despite their importance, the current literature has been focusing on the motives for individuals to found a startup, whereas the circumstances affecting individuals' career choices between working for an established firm and a startup have been largely unexplored. A potential first-order determinant of this career choice is household wealth, which may affect people's preference and risk-taking capacity in making employment decisions. Empirically investigating this question requires employer-employee matched data, as well as a research design that can isolate the household wealth effect from confounding factors.

In this paper, I use natural disasters as a new setting to study how household wealth affects their entrepreneurial career choices. My empirical analysis builds on an integration of flood data in geographic information system (GIS) format, the U.S. Census individual-level employment data, and deed records, and the key inputs being a set of staggered floods in the U.S. as natural disaster events. When a flood happens, there are households just above and below the inundation line. The subsequent entrepreneurial career choices made by the two groups within a 0.1-mile-wide band in the same community are informative about the causal impact of the natural disaster experience

[1] "Hiring good people" is the top concern for startup founders according to the State of Startups survey conducted by the First Round Capital from 2015 to 2017.

and, in particular, the resulting wealth damage on individuals' propensity to work in entrepreneurial firms. In fact, a regression discontinuity (RD) analysis shows that, in the two years after a flood, the group inside the inundation boundary are 7% less likely to work in a startup[2] as opposed to working in a more matured firm relative to their neighbors outside, despite the fact that their houses experience only several inches of flood and approximately $13,000 in property damage. This estimate suggests that even relatively modest flood experience can have a pronounced effect on the propensity to work in a startup as opposed to an established firm. Further analysis shows the two groups share the common labor market, which suggests the effect is likely to be driven by individuals' career choices rather than the differential impact on labor demand.

A difference-in-differences (DID) analysis on a broader sample sorted by elevation levels provides external validity to the inference made from the RD exercise. Since residents living in a low-elevation area are particularly exposed to the floods,[3] they are expected to be disproportionately affected in terms of their career choices. Indeed, the residents living in the area in the bottom-three deciles by elevation are 4.5% less likely to work in a startup after the flood, relative to their neighbors in the top three deciles within the same county.

A partitioning analysis on homeowners further attributes the adverse effect on entreprene-urial career choices to wealth damage in flood rather than purely psychological impact. Homeowners living in a low-elevation area presumably observe and experience the same flood events as their renter neighbors but suffer from more financial damage. I find the impact is mostly concentrated on homeowners, suggesting the lowered propensity to work in a startup is primarily driven by the household wealth loss as a result of a flood. Further analyses show that the effect is more profound in the propensity to become *startup employees* than founders, although the point estimates are negative for both decisions. Jointly, the above analysis shows novel evidence that impaired household wealth discourages people from becoming startup employees, which has not been documented in earlier studies.

[2]My analysis focuses on the employer startup firms up to two years old that hire at least one worker, which are likely to have higher growth potential than the subsistent forms of entrepreneurship such as self-employment [8].

[3]I find low-elevation properties do experience excessive damage to housing value and a disproportionate increase in foreclosure rates.

A battery of additional tests suggest the lowered propensity to become startup employees is driven by the impaired self-insurance capacity as a result of household wealth loss. In an incomplete market,[4] lowered wealth can reduce households' abilities to smooth consumption in the low-income scenarios and thus make them less willing to make risky entrepreneurial career choices (*self-insurance mechanism*).[5] Alternatively, household wealth can finance the pursuit of startup jobs with high non-pecuniary benefits (and relatively low pecuniary reward) if we assume the non-pecuniary benefits as luxury goods (*luxury good consumption mechanism*). Consistent with the self-insurance mechanism, I find low-elevation residents disproportionately refrain from startup jobs involving higher income risk than their previous job, and they also become less prone to changing employers and joining risky industries. The career distortions lead to a decline in individual earning power in the long run: based on a stylized property-damage model and an RD setting, I find one dollar of property damage translates to about 60 cents of labor income loss in the 6.5 years after a flood.

The paper is connected to three strands of literature. The first is the literature on the incentive for people to pursue entrepreneurial activities. A growing body of research studies how the decision to become business founders is affected by factors such as household collateral (e.g., [10], [11], [12], [13], [14]) and the regulatory environment (e.g., [15] and [16]). Although the entrepreneurial team plays a central role in the success of a venture, less attention has been paid to the motive of becoming a startup employee, who may face different trade-offs than a founder. This paper explores a potential first-order determinant of the decisions to become startup employees. Several papers examine how employees are sorted into startup firms based on their age, education, and work experience [7, 17, 18, 19]. This paper is the first to empirically investigate how the change in a household's financial condition affects the personal career choice to become an employee in a startup as opposed to an established firm. This paper also suggests a mechanism

[4]The role of wealth as self-insurance is particularly important in an incomplete market where risks cannot be efficiently hedged through financial markets and borrowing constraints exist.

[5]A startup job typically involves higher employment risk than a job in a matured firm. According to [9] and other research, about half of new businesses fail within the first four years of entry, and the unemployment due to business failure is more likely in startups than in established firms.

different from the collateral effect that has been documented to affect the business founding decision: household wealth can insure individuals against the employment risk associated with an entrepreneurial career.

Second, this study is connected to the literature that investigates how household wealth/cash windfall affects their behaviors regarding consumption and employment (e.g., [20] and [21]). There's still no consensus on the extent to which individuals smooth consumption, let alone how much the consumption-smoothing function of wealth affects their employment decisions. This paper speaks to this discussion by empirically tests the effect of negative wealth shock on risky career choices and the subsequent income. Also, by focusing on the job churning of employed people, this study complements the prior research that investigates how wealth affects the job searching of the unemployed group (e.g., [22] and [23]). The use of granular geographic variation in flood exposure further provides a relatively clean setting to study the impact of household wealth and largely disentangles the wealth effect from confounders, such as local economic conditions and personal attributes.

Third, this paper contributes to an emerging body of literature that studies how natural disaster affects individual and firm decisions. Although the increased frequency and economic relevance of natural disasters have been well-publicized,[6] their repercussion effect on broader economic activities is largely unclear. For example, how and to what extent natural disasters affect risk-taking behavior is still inconclusive. [26] and [27] find experiencing disasters can discourage firms and individuals from taking risks, whereas [28] documents excessive risk-taking by firm managers who experienced fatal disasters without extremely negative consequences. This paper adds to this discussion by showing even modest disaster experience can discourage risky career choices.[7] Also, documenting the financial effect of experiencing disasters complements the findings on the psychological impact in the prior studies. Another contribution comes from calibrating the elasticity

[6] In the U.S., the heightened environmental risk has tripled the frequency of natural disasters during the past 30 years [24], with more than $1.7 trillion in property value destroyed [25].

[7] Several related papers study the general-equilibrium labor outcomes after natural disasters, such as average wage, the labor location across industries and geographic regions [29, 30, 31], while this paper focuses on the partial equilibrium effect on individual employment decisions conditional on the local labor demand, with a unique focus on the risk-taking job choices such as working in a startup.

of long-run income loss with respect to flood damage. This calibration can potentially facilitate evaluating indirect costs of disasters and thus, inform climate-related policy decisions such as to what extent mandatory disaster insurance should be enforced.

The remainder of this chapter is organized as follows: Section 2.2 discusses the empirical setting and data; Section 1.3 details the research designs and baseline results; Section 1.4 explores the heterogeneity of the effect and disentangles mechanisms; Section 1.5 analyzes the long-run impact; Section 2.6 concludes.

## 1.2 Data and Overview

### 1.2.1 Flood Data

To understand how household wealth affects entrepreneurial career choices, I explore natural disasters as a negative shock to household wealth and integrate two key elements of data input: flood data and individual employment information.

Flood is the most devastating environmental disaster by occurrence and damage. For example, in 2017, floods caused direct financial losses of $285.1 billion according to the Center for Research on the Epidemiology of Disasters (CRED), topping the list of economic losses associated with natural disasters. The occurrence and severity of flood events have significantly increased in the last few decades, with about 60% of the counties in the US experiencing at least one significant flood from 2012 to 2016 according to Federal Emergency Management Agency (FEMA).[8]

The primary direct consequence of a flood is damage to properties and other structures. According to the CRED, a major flood can cause up to tens of billions in property damage. The damage is likely to have a real impact on the financial situation of households because flood risks cannot be fully hedged. Despite the existence of flood insurance products, the insured losses typically account for a small fraction of the total property damage in a flood event, largely due the low take-up rate and the incompleteness of the flood insurance policies.[9]

[8] According to the FEMA, all of the four costliest years for natural disasters in history occurred after 2000, of which the damage mostly arises from hydrological disasters such as floods and hurricanes.

[9] Since standard homeowner insurance policies exclude flood damage, the extent of coverage depends on the flood

My analysis focuses on five major flood events from 2005 to 2010 in five states: Tennessee, Indiana, Iowa, Pennsylvania, and California.[10] These are the five most significant flood events from 2003 to 2011 in the 17 states where the Longitudinal Employer-Household Dynamics (LEHD) data are accessible for my project.

To identify these events, I start from the sample of all federal government disaster declarations associated with a flood from 2003 to 2011 in the 17 states where the LEHD data are available.[11] I use Google trend statistics to identify the most significant flood events in each of the 17 states. In particular, I examine the dynamics of Google search frequency with the keyword "flood" combined with a state name, and locate the most significant spike in the search frequency in each state. Next, I match each of the spikes with a flood event in the CRED database to obtain their property damage. This allows me to restrict the sample further to the five flood events that cause property damage of $1 billion or more. To avoid leaving out any significant flood events, I manually check each visible spike in the search frequency history and find none of them passes the property damage threshold.

I obtain a list of counties affected by the five flood events from the county-level disaster declaration database provided by the FEMA. My baseline analysis focuses on the 220 counties subject to a federal disaster declaration associated with the five floods.

To gauge an individual's exposure to the flood, I use a battery of GIS datasets. To specify the elevation of a residential property, I obtain from the US Geological Survey (USGS) an elevation map with one-third arc-second resolution,[12] the highest-resolution seamless Digital Elevation

insurance policy purchased separately by homeowners. However, due to the priciness of such plans and the limited awareness of the potential flood risks, the take-up rate of flood insurance is typically low. For example, fewer than 10% of the households affected by Hurricane Florence in 2018 have flood insurance according to the estimates provided by Milliman Inc. Even the flood insurance plan holders face a limitation on the amount of property damage that can be reimbursed. For instance, the federal flood insurance covers no more than $250,000 of damage minus deductibles for a residential property.

[10]The five flood events include June 2008 Midwest floods in Indiana, 2010 Tennessee floods in Tennessee, June 2008 Midwest floods in Iowa, Los Angeles County flood of 2005 in California, and Mid-Atlantic United States flood of 2006 in Pennsylvania.

[11]The period of interest is determined by the sample period of the LEHD data accessible to my project. In most states, the LEHD data span from 2000 to 2014. To ensure that sufficient data are available for both pre- and post-shock periods, I require a natural disaster to occur at least three years after the starting year in the LEHD sample and three years before the ending year.

[12]Ground spacing is approximately 10 meters north/south, but variable east/west due to convergence of meridians with latitude.

Model (DEM) dataset with full coverage of the US.[13] To achieve a state-of-art identification, I extract high-resolution inundation maps for nine counties of Iowa, Indiana, and Pennsylvania from Iowa Flood Center and the National Oceanic and Atmospheric Administration (NOAA). The inundation maps allow me to pin down whether each property is inundated within a locality as granular as a block. Combining the above GIS data with individual-level residence location information from the LEHD allows me to track how much an individual is affected by a flood. I integrate the GIS data and other datasets at the individual level using ArcGIS, a software specialized in processing GIS data.

### 1.2.2 Employment Data

To obtain individual employment information, I resort to the LEHD, a large-scale confidential administrative dataset made available by the US Census Bureau. The LEHD provides a longitudinal match between employer and employee from 2000 to 2014. The core dataset tracks the income from each job (i.e., an employer-employee combination) for each quarter in a large panel at the employer-individual-quarter level. Being longitudinally linked at both the individual and firm levels, the data enable me to track quarter-to-quarter job transitions for each individual. The data covers all individuals in the unemployment insurance wage record, which accounts for over 90% of civilian wage jobs in the US. As mentioned, the batch of LEHD data currently available to my project covers 17 states including Arizona, Arkansas, California, District of Columbia, Delaware, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, New Mexico, Oklahoma, Pennsylvania, Tennessee, and Texas.

The LEHD is a good fit for my research purpose for the following reasons. First, it allows me to identify the individuals who work for a startup at a given point in time. The firm age information from the LEHD helps identify the employer startup firms at their early stage, which have been documented to be a particularly important form of entrepreneurship due to the high

[13] To confirm the financial damage arising from a flood, I obtain flood damage data for an out-of-sample flood event from the National Oceanic and Atmospheric Administration (NOAA) as an additional dataset.

growth potential.[14] In particular, I define a startup as an employer firm that has existed for up to two years using a method similar to [33].[15] Second, among other individual attributes,[16] the LEHD infrastructure provides the residence location of each person in the form of coordinates with a six-decimal-degree precision level. The residence location data allows me to link the LEHD to the GIS datasets at the individual level and in particular, to specify the extent to which an individual is affected by a flood.

In addition, I track property value and homeownership of individuals using Corelogic deeds and tax roll record, a leading dataset that covers the profiles of more than 147 million residential and commercial properties, or 99% of all properties in the US. The data provides detailed information about property transactions, property characteristics, mortgage terms and performance, and so on. The key variables include transaction price, owner's name, mortgage loan amount, loan term, appraisal price, and property location.

I merge the Corelogic data and the LEHD at the individual level, which has not been done in the literature. I match the individuals in the two datasets on name and physical address, using the Person Identification Validation System (PVS) from the Census Bureau. About 4.5% of the homeowners in the LEHD are successfully matched to Corelogic deed records in my main sample.[17] The data integration represents one of the earliest efforts to construct a comprehensive dataset that provides both the detailed homeownership information and the employment dynamics at the individual level with broad coverage of the US population.

[14] My analysis is focused on the employer startup firms that hire at least one worker, which are likely to have higher growth potential than more subsistent form of entrepreneurship such as self-employment [8]. More than 60% of these startups are incorporated, which is associated with intention to grow [32].

[15] In my main empirical analyses, the outcome variable of interest is a dummy variable that equals one if a person is working for a startup in a given quarter and zero if he/she is working for a more matured firm. In other words, all the tests will be conditional on a person being employed and are not driven by the unemployment of an individual.

[16] In addition, the LEHD provides individual-level biographical information such as gender, age, and educational attainment.

[17] About 2.9% of individuals in the LEHD in my main sample can be unambiguously matched to a homeowner in the Corelogic deed records, accounting for 4.5% of homeowners in the LEHD assuming the homeownership rate in the LEHD sample is the same as the general statistics (65.0%) provided by the U.S. Census Bureau's American Community Survey for the corresponding regions.

### 1.2.3 Summary Statistics and Overview

The main sample consists of individuals in the LEHD data residing in a county, in which the federal government declared a disaster associated with one of the five flood events specified above, and those that live in a property with an elevation in the bottom or top three deciles within the county.[18] Because the sample satisfying the above criteria is too massive to be assessed within a reasonable time frame, I use a 2% randomized subsample based on the ending digits of the unique individual identifier in the LEHD following the convention of the LEHD literature (e.g., [34]). This method reduces the sample to around 218,000 individuals. The main tests examine two years before and two years after each flood.

Table 1.1 reports the summary statistics of the main sample. $Elevation$ is the elevation of an individual's residential property in foot. $D^{startup}$ denotes a dummy variable equal to 1 if in a given quarter, an individual works in a startup up to two years old as the primary job, and 0 if he/she works for a more matured firm. $Education$ is the highest degree a person obtains with a scale of 1-4, where 1 represents a high school diploma or below, and 4 indicates a graduate degree. $Age$ is the age of an individual at the time of the flood. $Male$ denotes a dummy variable equal to 1 for male, and 0 for female. $Income$ is an individual's average quarterly wage from the primary jobs[19] over the sample period. Columns (1) to (3) report the mean, standard deviation, and pseudo median[20] of the above variables, respectively. Around 11% of the people in my sample work for a startup in a given quarter conditional on being employed. An average person in my sample is a 38 years old, who holds an undergraduate degree and earns about $7,800 per quarter. The summary statistics of individuals are comparable to those reported by the US Census Bureau.

[18] The baseline results are robust to the use of alternative thresholds such as the top and bottom quintiles, according to unreported analyses.

[19] A primary job is defined as the job that provides the highest income in a given quarter for an individual if multiple jobs emerge in the same quarter.

[20] The US Census Bureau does not allow disclosing the median of any variable. To accommodate the disclosure requirement, I report the pseudo median as measured by the mean of all observations between the 49th and 51st percentiles.

## 1.3 Natural Disaster Experience and Entrepreneurial Career Choices

Using floods as a negative shock to household wealth, I conduct two empirical strategies to investigate how household wealth affects entrepreneurial career choices. In an RD framework, the first strategy exploits the spatial discontinuity in property damage around the boundary of the areas actually inundated in a flood. I compare the residents living within 0.1-mile-wide bands on both sides of the boundary of inundated areas regarding their entrepreneurial career choices around a flood. To provide external validity, I further compare low- and high-elevation residents within the same county regarding their pre-post change in entrepreneurial career choices. This section elaborate on the two empirical strategies and report the results.

### 1.3.1 Regression Discontinuity Design

To start with, I employ a RD design around the boundary of the actual inundated areas in a flood to investigate how natural disaster experience and the resulting wealth damage affects the career choice of working in a startup versus in a matured firm. In particular, I compare the residents living within a 0.1-mile-wide band inside the inundation boundary (treated group) and outside the boundary (control group), in terms of their change in entrepreneurial career choices around the floods. The running variable is the distance between an individual's residence location and the boundary, which is set to be negative outside the areas, and positive inside.

To understand how the financial damage from natural disasters affect entrepreneurial career choices, a key empirical challenge is the inability to observe the counterfactual employment decisions of the flooded people in the absence of the flood damage. The RD method helps overcome this empirical challenge by providing a control group who are ex-ante indistinguishable from the treated group, and thus, make decisions that can closely mimic the counterfactual. The randomness of the treatment assignment between the treated and control groups arises from the uncertainty in the exact location, intensity, and duration of precipitation in a highly local area where residents face similar ex-ante flood risks. Also, since people living in such a local area are demonstrated to

share the common labor market, this method controls for local labor demand and isolates the effect on individuals' labor supply decisions.

**Identification Assumptions and Covariate Balance**

This design relies on the assumption that the residents within the narrow bands on both sides of the boundary have similar covariates, except for the level of property damage. The covariate test below shows the absence of significant gaps between the treated and control groups regarding a variety of predetermined attributes, in line with the assumption. The design also assumes that individuals cannot perfectly manipulate their residence location around the boundary in anticipation of the flood, which is also likely to hold in this setting. In narrow bands on both sides of the threshold (e.g., 0.1-mile-wide bands in the baseline test), the manipulation requires a precise prediction of the frontier of the inundated area. However, the inundation boundary is largely determined by the location, intensity, and duration of precipitation, of which the prediction is extremely difficult to meteorologist and well beyond the capacity of an average resident. The result of the McCrary density test is also consistent with the absence of perfect manipulation: no significant discontinuity in the population density function exists at the boundary of the inundation map.

To test the assumption that the treated and control groups are ex-ante similar around the threshold, I compare a battery of predetermined attributes for the residents living in bands of 0.1 miles width on both sides of the boundary. Table 1.2 reports the comparison on a set of demographic characteristics at the individual level, where $D_{pre}^{startup}$ denotes a dummy variable equal to 1 if an individual worked in a startup in the quarter before the shock, $Income$ is the average quarterly income in the two years leading up to the shock, and *Home Value* is the home value in US dollars at the time of the flood. *Flood Risk* captures an individual's ex-ante exposure to flood risks and is defined as a categorical variable with a scale of 0 to 2, where 2 (1) indicates the residence address is within a 100-year (500-year) pre-designated flood zone, and 0 represents the person lives out of a flood zone. *Employer Inundation Status* is a dummy variable set to 1 if a person works in

an establishment inside the flooded area, and zero if outside.[21] The treated and control groups do not exhibit significant discrepancy in any of the demographic attributes and even ex-ante flood risk exposure. Although analyzing unobservable covariates is technically impossible, the balance of observed covariates between the two groups suggests the assignment of treatment (flood) is quasi-random around the inundation boundary.

In essence, the identification arises from the comparison of two neighbors who are ex-ante identical but suffer from different levels of flood damage because one's home happens to be submerged at the peak of the flood and the other not. The employers of the treated and control groups being equally likely to be inundated suggests the labor market is not segmented around the flood boundary, and the two groups largely face the common pool of employers. This observation further strengthens my belief that the RD analysis result is driven by individual employment decisions rather than the differential effects on the two groups' current employers or potential job opportunity.

Notably, using the RD design in a DID framework requires less strict identification assumptions than what I have tested and verified. Even if the treated and control groups around the boundary of the inundated areas exhibited a significant gap in some covariates, the identification strategy would remain valid as long as the discrepancy does not drive a dispersion in employment decisions concurrently with the flood.

**Regression Analysis**

After verifying the identification assumption, I perform the regression analysis that compares the residents in the 0.1-miles-wide bands on both sides of the inundation boundary regarding their entrepreneurial career choices around the flood. In particular, I estimate a DID specification of the following form:

$$D_{i,t}^{Startup} = \alpha_i + \alpha_t + \beta_1 D_t^{Post} \times D_i^{Inundated} + D_t^{Post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}, \tag{1.1}$$

[21] $Age$, $Male$, and $Education$ are defined in Table 1.1.

where $i$, $k$, and $t$ denote an individual, a county, and a quarter. $D_t^{post}$ is a dummy variable equal to 1 if quarter $t$ is within two years after the shock (including the quarter of the flood), and 0 if quarter $t$ is within two years before the shock. $D_i^{Inundated}$ is a dummy variable equal to 1 if individual $i$ lived inside the inundated zone before the shock, and 0 otherwise. The outcome variable of interest $D_{i,t}^{startup}$ is a dummy variable set to 1 if individual $i$ works for a startup either as a founder or an employee during quarter $t$, and 0 if he/she is working for a more matured firm. As mentioned above, I define a startup as a firm up to two years old. Due the the definition of the outcome variable and the nature of LEHD individual employment data, all the tests hereafter are conditional on a person being employed.

To allow for the property-specific heterogeneity and macro-level trends, I incorporate property fixed effects $\alpha_i$ and quarter fixed effects $\alpha_t$. To compare the residents on both sides of the same flood line, I include the interaction terms of $D_t^{post}$ and the full set of county fixed effects. In particular, $D_i^k$ is a dummy variable set to 1 if a person lives in county $k$, and 0 otherwise. The key coefficient $\beta_1$ captures how much more likely an inundated resident is to choose an entrepreneurial career after the shock, relative to a close neighbor whose home is marginally unflooded. I would expect $\beta_1$ to be significantly negative if natural disaster experience discourages people from working in startups.

Table 1.3 reports the estimate of Equation 1.1. The significantly negative coefficient on $D^{Post} * D^{Inundated}$ shows after a flood, the residents in inundated areas are less likely to choose an entrepreneurial career over employment in an established firm, relative to their neighbors outside the regions. This finding supports the hypothesis that natural disasters make people refrain from entrepreneurial jobs.

In terms of the magnitude, after the shock, the residents living inside the boundary are about 7% less likely to work in a startup as opposed to working in an established firm, using the pre-post change of the neighbors outside the boundary as a benchmark. Combining the inundation depth information from the inundation maps and the foundation height information for the areas of interest, the treated groups, on average, see a peak of eight inches of water in their houses or

equivalently, about $13,000 in property damage based on my imputation.[22] Together, the estimate shows that even relatively modest flood damage can have a pronounced effect on entrepreneurial career choices.

**Graphical Evidence**

Figure 1.1 shows the visual evidence of the RD analysis. Panel A reports the inundation depth in a flood around the boundary of inundated areas, where the inundation depth is defined as the height of water above ground. The horizontal axis represents the distance between a residential location and the boundary, which is set to be negative outside the inundated area, and positive inside. Each point represents the average footage of inundation for all residential properties within a band of 0.005 miles width. Due to the continuous slope around the boundary, I expect a kink rather than an immediate jump in inundation depth at the boundary. Consistent with the prediction, the inundation depth remains 0 outside the boundary and increases with the distance from the boundary inside the inundation zone.

Panel B shows the pre-post change in entrepreneurial career choices around the inundation boundary in a similar format. Each point represents the average increase in the probability of pursuing an entrepreneurial job around the shock for all individuals residing in a band of 0.005 miles width. As predicted, a kink emerges at the boundary. In contrast to the residents outside the boundary whose entrepreneurial decisions do not change significantly, people inside are less likely to work for startups, and the impact is more pronounced for those living in the central inundated areas and far away from the boundary. Together, Panels A and B lend support to the hypothesis that the inundation in a flood discourages people from working in startups.

Given the presence of kinks at the boundary, a Regression Kink (RK) design is also applicable to this setting. Accordingly, I estimate the kink at the threshold as detailed in Appendix A.1.1, which confirms my findings from the RD analysis.

[22]The imputation method is detailed in Section 1.4.1.

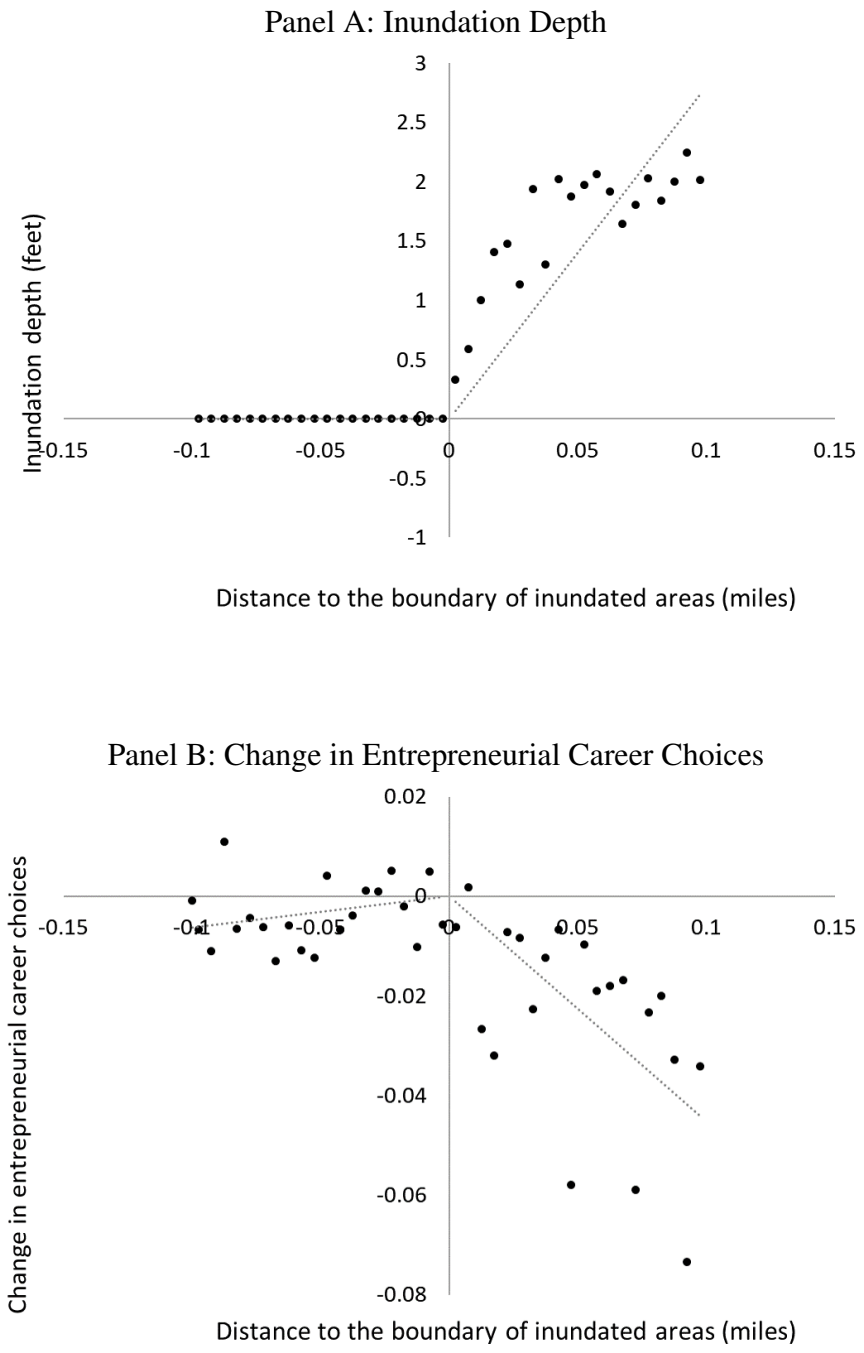



Figure 1.1: **Regression Discontinuity Analysis: Graphical Evidence.** Panel A shows the inundation depth in a flood around the boundary of the inundated areas. The horizontal axis represents the distance between the residence location and the boundary, which is set to be negative outside the inundated area and positive inside. Each point represents the average inundation depth of all residential properties in a band of 0.005 miles width. Panel B shows the change in entrepreneurial career choices around the boundary in a similar format to Panel A. Each point represents the average increase in the probability of working for a startup after the shock in the corresponding 0.005-mile-wide band.

### 1.3.2 Difference-in-Differences Design

To verify whether the above finding holds beyond the local area around the inundation boundary, I perform a DID analysis based on a broader sample that covers most residents in the flooded counties. The first difference is pre- versus post-flood. The second difference is the elevation of an individual's residence location. I assume people living in a low-elevation area are more likely to be affected by a flood than their neighbors in a high-elevation area. In other words, the DID analysis examines how people more exposed to a flood react differently than less-exposed neighbors in their entrepreneurial career choices.

A key empirical challenge is to disentangle the impact on labor supply from the effect on labor demand, given that a flood may shift both the demand and supply curve in a local labor market. For example, the flood damage could hinder the operation of entrepreneurial firms and make them cease hiring. To control for the potential labor demand shock associated with a flood, I exploit the within-county variation in elevation, assuming the residents in the same county share a labor market and face a similar development in labor demand around a flood. This method allows me to identify how a flood affects individuals' labor supply decisions, and in particular, the decision to pursue an entrepreneurial career in a relatively large sample.

The DID design serves two purposes. First, as discussed above, the analysis based on a broad sample provides external validity to the finding in the RD analysis. Second, the resulting enhanced power of the test enables me to conduct a variety of partitioning analyses, which help understand the underlying mechanism through which the effect is operated.

**First-Stage Analysis: Elevation and Flood Exposure**

Before elaborating the DID strategy, I try to verify the low-elevation residents are more affected by the flood than high-elevation neighbors. To start with, I run the cross-sectional regression:

$$Outcome_i = \beta_1 Elevation_i + \sum_{k=1}^{K} \gamma_k D_i^k + \varepsilon_i, \tag{1.2}$$

where $i$ denotes a property, $k$ denotes a county, and $Elevation_i$ is defined as the elevation of property $i$. Two outcomes of interest emerge: $InunDepth_i$ and $Damage_i$. $InunDepth_i$ is the footage of property $i$ submerged in a flood. $Damage_i$ denotes the flood damage of property $i$ with a scale of 1 to 5, where 1 represents no damage, and 5 represents destroyed. The key coefficient $\beta_1$ captures how much the property elevation determines the level of damage/inundation in a flood. Because damage data are unavailable for the main sample, I estimate the above regression using an out-of-sample flood event in Texas in 2017.

Columns (1) to (4) of Panel A in Table 1.4 report the results, where Columns (1) and (2) examine $InunDepth$ and Columns (3) and (4) examine $Damage$. The specifications in Columns (1) and (3) (Columns (2) and (4)) incorporate county fixed effects (zip code fixed effects). The significantly negative coefficients on $Elevation$ suggest lower-elevated properties are more likely to be inundated and damaged due to a flood. Regarding the magnitude, a one-standard-deviation decrease in elevation corresponds to a 0.2-standard-deviation increase in property damage.

To test a flood's impact on home value and households' financial situations more directly, I run a regression of the following form at the property-year level:

$$Outcome_{i,t} = \alpha_i + \alpha_t + \beta_1 D_t^{post} \times Elevation_i + D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}. \tag{1.3}$$

where the outcome variable is either $HomeValue_{i,t}$ or $Foreclosure_{i,t}$. $HomeValue_{i,t}$ is the value of property $i$ in year $t$ measured by the latest assessor pricing of the property. $Foreclosure_{i,t}$ is a dummy variable equal to 1 if property $i$ experiences a foreclosure transaction in year $t$, and 0 otherwise. Similar to Equation 1.1, the specification includes individual fixed effects $\alpha_i$, year fixed effects $\alpha_t$, and the interaction terms between $D_t^{post}$ and county fixed effects $D_i^k$. The coefficient $\beta_1$ represents a flood's impact on the value (the foreclosure rate) of a high-elevation property relative to low-elevation properties in the same county. I would expect a negative estimate of $\beta_1$ if a flood leads to a disproportionate impairment on low-elevation housings.

Columns (5) and (6) of Panel A in Table 1.4 report the estimate of Equation 1.3 using $HomeValue$

as the outcome variable. Column (5) examines the dollar value of a property, and Column (6) examines the standardized housing price denominated by the pricing level four years before the shock. Because Tennessee is the only state where assessor pricing is available for both the pre- and post-flood periods, the estimate builds on the subsample in Tennessee. In particular, I focus on one year before and one year after the shock to capture the immediate change in home value at the time of the flood.[23] For both measures of home value, the coefficient on $D^{Post} \times Elevation$ remains significantly positive, confirming the hypothesis that residents in a low-elevation area suffer more from home value losses than high-elevation neighbors.[24]

Panel B corresponds to Equation 1.3 using $Foreclosure$ as the outcome variable, based on two years before the shock and one year, two years, and three years after the shock for Columns (1) and (4), Columns (2) and (5), and Columns (3) and (6), respectively. Columns (4) to (6) (Columns (1) to (3)) incorporate post $\times$ county (post $\times$ state) fixed effects. The significantly negative coefficient on $D^{Post} \times Elevation$ is consistent with an excessive increase in foreclosure rate among the low-elevation residents relative to high-elevation neighbors. I interpret the finding as a symptom of the disproportionate deterioration of the financial situations among low-elevation residents.[25]

Put together, low-elevation properties are more likely to be inundated in a flood and thus experience higher home-value loss than high-elevation neighbors in the same county. These findings confirm the legitimacy of elevation as a proxy for exposure to flood damage.

**Do Lower-Elevation Residents Become Less Entrepreneurial After a Flood?**

After verifying the excessive exposure to flood for the low-elevation residents, I perform the DID analysis by comparing the change in entrepreneurial career choices for individuals residing in low- and high-elevation areas around the flood. In particular, I run an individual-quarter-level

[23] Since some homeowners may choose to repair their home immediately after the flood and before the next appraisal, the assessor pricing may include the value of repairment and underestimate the damage arising from a flood. In this sense, the estimate in Columns (5) and (6) should be interpreted as a lower bound of the flood damage.

[24] In particular, a one-standard-deviation decrease in elevation translates to a 16.0% loss in home equity. The estimation is based on the assumption that the average LTV of properties is 80%.

[25] A one-standard-deviation decrease in elevation translates to a 34.6% increase in the foreclosure rate after a flood.

regression of the following form:

$$D_{i,t}^{startup} = \alpha_i + \alpha_t + \beta_1 D_t^{post} \times D_i^{LowElev} + D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + D_t^{post} \times (\sum_{j=1}^{J} \tau_j D_{i,t-1}^j) + \varepsilon_{i,t}, \tag{1.4}$$

where $i$, $j$, $k$, and $t$ denote an individual, a firm (an employer), a county, and a quarter, respectively. $D_i^{LowElev}$ is defined as a dummy variable equal to 1 (0) if the elevation of the property where individual $i$ lives falls in the bottom (top) three deciles within a county.[26] Similar to Equation 1.1, the specification includes individual fixed effects $\alpha_i$, year fixed effects $\alpha_t$, and the interaction terms between $D_t^{post}$ and county fixed effects $D_i^k$.[27]

One may be concerned that the divergence in employment decisions for high- and low-elevation residents after the shock may reflect the flood's differential impact on their employers rather than the residents if the two groups worked for different pools of firms before the flood. To address this concern, I incorporate the interaction terms of $D_t^{post}$ and the full set of firm fixed effects $D_{i,t-1}^j$ in the specification, where $D_{i,t-1}^j$ is a dummy variable equal to 1 if individual $i$ works for employer $j$ in quarter $t-1$, and 0 otherwise.

Essentially, the analysis compares the pre-post change in entrepreneurial career choices for two individuals living in the same county and employed by the same firm in the previous quarter, one of whom resides in a low-elevation area and the other in a high-elevation area. If a natural disaster discourages people from pursuing entrepreneurial careers, I would expect a negative estimate of $\beta_1$ in this equation.

Table 1.5 reports the estimate of Equation 1.4 and its variants using the data from two years before to two years after the shock. Besides the quarter fixed effects and individual fixed effects included in all specifications, Column (2) (Column (5)) incorporates interaction terms between $D^{Post}$ and county fixed effects ( year-county fixed effects), and Column (1) (Column (4)) includes both $D^{Post} \times$ County and $D^{Post} \times$ Firm fixed effects (both year-county and year-firm fixed effects).

[26] The results are robust to the use of alternative thresholds such as the top and bottom quintiles.

[27] $D_t^{post}$ and $D_{i,t}^{startup}$ are as defined in Table 1.1.

As expected, the coefficient on $D^{Post} \times D^{LowElev}$ loads significantly negative regardless of the specifications. According to the coefficient estimate in Column (1), after a flood, an individual living in a low-elevation area becomes 4.3%[28] less likely to work in a startup relative to a neighbor who resides in a high-elevation area within the same county and works for the same employer in the previous quarter. Given that low-elevation residents are more exposed to the floods, the above finding is consistent with an adverse effect of natural disasters on people's entrepreneurial career choices.

**Parallel Trends Analysis**

The above empirical strategy assumes that the entrepreneurial career choices of high- and low-elevation residents should have experienced *similar trends* around the shock absent the floods. To test this assumption, I examine how the elevation gap in entrepreneurial career choices evolves around the shock by estimating the regression

$$D_{i,t}^{startup} = \alpha_i + \alpha_t + \sum_{\tau=-8}^{7} \beta_\tau D_t^\tau \times D_i^{LowElev} + D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}, \tag{1.5}$$

where $D_t^\tau$ equals 1 if quarter $t$ is the $\tau^{th}$ quarter following the shock, and 0 otherwise. The coefficient of interest $\beta_\tau$ measures the gap between the low- and high-elevation residents in terms of their probability of working in a startup during the $\tau^{th}$ ($-\tau^{th}$) quarter after (before) the shock. Likewise, the specification incorporates individual fixed effects $\alpha_i$, quarter fixed effects $\alpha_t$, and county $\times$ post fixed effects. Figure 2.1 reports the results, where the horizontal axis represents the quarter relative to the flood events, and the vertical axis represents the estimated $\beta_\tau$ for the corresponding quarter. No significant differences emerge between the low- and high-elevation residents for all quarters before the shock, which lends support to the parallel-trend assumption. The graph also confirms the differential impact of the flood on low- and high-elevation individuals: in each quarter after the floods, low-elevation residents are less likely to work in startups than high-elevation

[28] I obtain the magnitude through dividing the coefficient -0.487 in Column (1) by the baseline probability of entrepreneurship 0.113 as shown in Table 1.1.

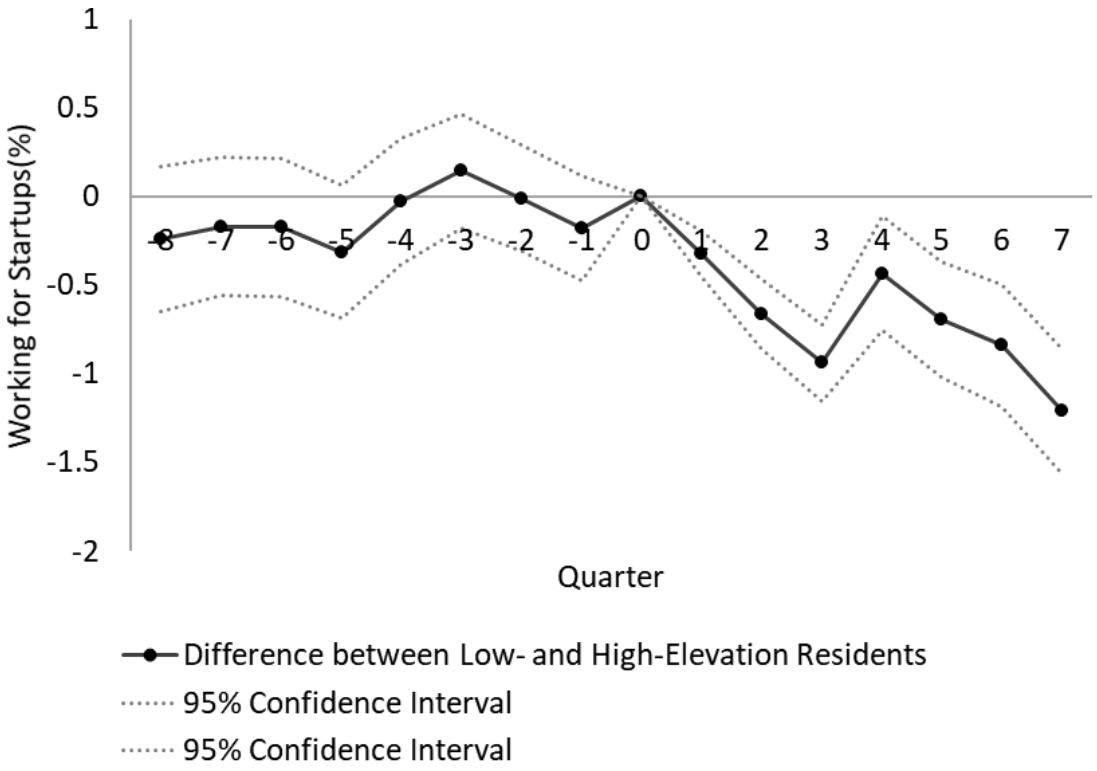



Figure 1.2: **The Test of Parallel Trends: Low- Versus High-Elevation Individuals.** The figure plots the difference between low- and high-elevation residents on their entrepreneurial activities around the shock. The horizontal axis represents the quarter relative to the flood events and the vertical axis reflects the differential probability of working for startups between low- and high-elevation groups. Each point corresponds to an estimate of $\beta_\tau$ in Equation 2.4. The dotted lines represent the upper and lower bounds of the 95% confidence interval of the estimates.

neighbors in the same county, and the differences are significant at the 5% confidence level for all post periods. Besides testing the parallel-trend assumption, I further mitigate the concern about confounder by exploring the cross-county variation in flood exposure and carefully analyzing the effect of observed discrepancy between the treated and control groups, as detailed in Appendix A.1.2

Overall, low-elevation residents are less likely to work in a startup relative to the high-elevation neighbors in the same county, and the effect is unlikely to be driven by a confounder. This finding confirms that the dampening effect of natural disaster experience on entrepreneurial career choices is not only concentrated on the areas around the inundation boundary but hold in a broader sample.

To be noted, the above analysis examines the entrepreneurial career choices in all industries of the private sector appearing in the LEHD. Further analysis as detailed in Appendix A.2.1 suggests the effect holds for most major industries including the technology industry, in which entrepreneurial activities have been believed to have a particularly profound economic implications. Another important observation is that the effect is primarily driven by the lowered propensity to join a startup from a matured firm. My analysis shows that the lowered transition rate from established firms into startups accounts for about 79.5% of the effect, while the remaining is explained by the lowered retainment rate within startups. (See Appendix A.2.2.)

## 1.4 Mechanism Analysis

### 1.4.1 Wealth Effect or Psychological Impact?

The adverse effect of natural disaster experiences on entrepreneurial career choices could be driven by different potential mechanisms. On the one hand, since property damage accounts for most of the direct cost of a flood, the resulting decrease in housing wealth may be the reason why people refrain from working in a startup. On the other hand, experiencing a flood could change people's career decisions through pure psychological impact. For example, witnessing a natural disaster may traumatize people and thus make them reluctant to venture in a career [26, 35].

To test whether the above finding is driven by financial damage or pure psychological effect, I

explore the individual-level, cross-sectional variation on homeownership. Since the homeowners are disproportionately vulnerable to housing damage and financial loss due to a flood, I would expect an exceptionally pronounced effect on homeowners if the financial damage drives the dampening effect on entrepreneurial career choices. On the contrary, if the disaster exerts the effect through a purely psychological channel, I would expect the homeowners to be affected no more than an average person in my sample.

In particular, I examine whether the dampening effect on entrepreneurial career choices is particularly pronounced for homeowners by estimating Equation 1.4 on the subsample consisting only of homeowners. I define homeowners as the people who purchase a residential property at least one year before the flood and keep owning and living in the property at the time of the flood. Tracking the employment decisions of homeowners requires the individual-level matching between the LEHD and Corelogic deeds record. The algorithm provided by the US Census Bureau enables me to match the deed records to an individual or more in the LEHD. [29]

Table 1.6 reports the result. The estimates build on the subsample of homeowners identified through the above matching. All variables are defined in Table 1.5. As expected, the coefficients on $D^{Post} * D^{LowElev}$ are significantly negative,[30] and the magnitude of the coefficients consistently exceeds that in the baseline analysis, as shown in Table 1.5. In other words, the dampening effect of natural disasters on entrepreneurial career choices is particularly pronounced for homeowners. Given that about 65.0% of residents in the affected counties own their home, the comparison of the coefficients in Tables 1.5 and 1.6 suggests the baseline effect is primarily driven by homeowners' reluctance to pursue entrepreneurial careers.

The partitioning test on homeowners provides evidence consistent with the wealth effect. The dampening impact of natural disasters on entrepreneurial careers is likely to be connected to di-

[29]Since multiple owners may exist in a property transaction, a property in the deeds record can match to more than one individual in the LEHD in a given quarter.

[30]Since only about 5% of the homeowners in the sample can be unambiguously identified based on the matching between the LEHD and the Corelogic deed records, the power of the test based on the matched subsample of homeowners is presumably lower than using the entire sample. Despite the reduced power of the test, The significant estimates of the key coefficient despite the limited power of the test suggests the effect on homeowners is likely to be highly pronounced.

rect wealth damage from the floods, as opposed to the pure psychological impact of observing a disaster.

One caveat is that the concentration of the effect on homeowners can also be consistent with the hypothesis that the home repair effort discourages people from working in a startup. If this is the case, I would expect the effect to be relatively short-lived since the repair work after a flood typically takes only a few weeks. However, as shown in Figure 2.1, the effect is largely progressive over the two years after a flood and does not revert in at least three years based on an unreported test. Together, the observations suggest the distracting effect arising from the repair effort is unlikely to be a driving force.

### 1.4.2 Heterogeneity: Business Founders versus Startup Employees

The above analysis shows that natural disasters such as floods can discourage people from working in startups by impairing household wealth. This subsection further explores the heterogeneity of this effect on a fundamental aspect, namely, the role of a person in a startup. In particular, I examine whether the above effect is driven by the lowered propensity to become startup founders or entrepreneurial employees. This exercise can have important policy implications for encouraging entrepreneurial activities. Considering aspiring startup founders and employees may have different motives to work in a startup, and thus respond to distinct stimulating policies, showing the heterogeneity in these two groups can potentially facilitate the design of policies that target at each group separately.

Table 1.7 reports the partitioning test that examines how natural disasters affect the propensity to become startup founders and entrepreneurial employees, respectively. In Columns (1), (3), and (5), the outcome variable is $D^{Startup}_{Founder}$, a dummy variable equal to 1 if an individual is a startup founder in a given quarter, and 0 if he/she works as a startup employee or in a matured firm. Following [9], I use being the top three earners in a startup as a proxy for startup founders.[31] The outcome variable in Columns (2), (4), and (6) is $D^{Startup}_{Employee}$, a dummy variable equal to 1 if a

[31] The result is robust to using the top five earners as a proxy for startup founders.

person works as an startup employee (i.e., ranked below top three in earnings) in a given quarter, and 0 if he/she is a startup founder or works in a matured firm.[32] Columns (1), (3), and (5) show that after the shock, low-elevation residents are less likely to found a startup relative to the high-elevation residents, although the effect is statistically insignificant. Columns (2), (4), and (6) show a significantly lowered propensity to become startup employees for low-elevation residents after the shock, relative to high-elevation neighbors in the same county.

The results suggest the decreased entrepreneurial activities after experiencing natural disasters are primarily driven by the lowered incentive to work as startup employees, more than the reluctance to become startup founders. Combined with the above findings concerning the wealth damage, my analysis provides novel evidence that the financial damage from a flood can discourage individuals from becoming startup employees. If we further view the floods as exogenous shocks to household wealth, the above finding sheds light on an important but understudied aspect of the incentive to pursue entrepreneurship: household wealth can affect the decision to become a startup employee, which has not been documented in earlier studies.

### 1.4.3 Why Does Wealth Loss Affect Aspiring Startup Employees?

A missing part of the picture is the mechanism through which the wealth damage arising from a flood affects aspiring startup employees. A potential channel is related to the risky nature of an entrepreneurial job. A startup employee typically faces higher income volatility and a greater unemployment risk than a worker in an established firm. According to [9], about half of new businesses fail within the first four years of entry, a much higher failure rate than more matured firms. In an incomplete market where income risk cannot be perfectly hedged, household wealth loss can impair their ability to smooth consumption during unemployment, and in other words, reduce their self-insurance against the employment risk associated with a startup job [36, 37, 38, 39, 40]. This consideration can in turn discourage the pursuit of entrepreneurial careers, which I refer to as the *self-insurance mechanism*.

[32] The definition of $D^{Post} \times D^{LowElev}$ is consistent with specification 1.4.

An alternative channel concerns the potential non-pecuniary benefits as a result of taking a job in a startup. A line of research shows evidence that people may enjoy additional benefit on top of the financial reward from entrepreneurship, such as the utility derived from the independence and flexibility at work, a high level of control, and a taste of variety in the work experience [41, 42, 43, 44]. Similar non-pecuniary benefits can come with working in an entrepreneurial firm even as an employee. If the non-pecuniary benefits can be viewed as a luxury good, decreased household wealth can impede one's ability to pursue these benefits. I refer to this mechanism as the *luxury good consumption mechanism*.

To disentangle the mechanisms, I start by exploring the cross-sectional variation on the level and variability of the income from a startup job. If the self-insurance mechanism prevails, the lowered risk-taking capacity should mainly discourage people from taking additional risk, i.e., pursuing high-risk startup jobs that involve a higher unemployment or income risk than their current positions. If the luxury good consumption mechanism dominates, people should particularly refrain from entrepreneurial jobs that provide a lower salary than their current employment, because these "downgrade" job transitions are more likely to be justified by the additional non-pecuniary benefit associated with the startup jobs.[33]

Table 1.8 reports the partitioning test that examines how natural disasters affect the propensity to take downgrade/upgrade and high-risk/low-risk startup jobs. In Columns (1) and (2) (Columns (3) and (4)), the outcome variable is $D^{Startup}_{LowerIncome}$ ($D^{Startup}_{HighIncome}$), a dummy variable equal to 1 if in a given quarter, an individual holds an entrepreneurial job that provides lower (higher) income than his/her previous job. The outcome variable in Columns (5) and (6) (Columns (7) and (8)) is $D^{Startup}_{LowerRisk}$ ($D^{Startup}_{HigherRiks}$), a dummy variable set to 1 if an individual holds an entrepreneurial job with lower (higher) income volatility than his/her previous job in a given quarter.

Columns (1)-(4) show that natural disasters discourage low-elevation residents from pursuing upgrade entrepreneurial jobs significantly relative to high-elevation neighbors in the same county,

[33] In other words, if a person leaves a matured firm for a startup to pursue non-pecuniary benefits, he/she is likely to face an income downgrade because the low non-pecuniary benefits of the previous job is supposed to be compensated by relatively high income assuming the human capital remains constant around the job transition.

whereas the differential impact on downgrade jobs is statistically insignificant. Columns (5)-(8) show that low-elevation residents mainly refrain from risky entrepreneurial jobs, as opposed to the jobs involving lower income risk than their current positions. The findings lend support to the self-insurance mechanism, as opposed to the laxury good consumption mechanism.[34]

To verify the self-insurance mechanism further, I investigate other forms of risk-taking employment decisions. If flood damage does impair an individual's risk-taking capacity and in turn discourage entrepreneurial careers, I would expect the affected people to refrain from other forms of risk-taking decisions after a flood as well.

Accordingly, I examine two alternative risk-taking employment decisions. The first is changing employers, which typically involves additional risk relative to remaining in the same firm. The risk may take the form of a potential mismatch between employees and employers due to information frictions, uncertainty regarding income prospects, legal and logistic risks involved in the job transition, among other potential risks. The second is the decision to work in a risky industry with a high likelihood of unemployment. I estimate how natural disasters affect the above two risk-taking decisions based on Specification 1.4.

Panel B of Table 1.8 reports the results. The outcome variable in Columns (1) and (2) is $D_{i,t}^{Churn}$, a dummy variable equal to 1 if individual $i$ is working for a different employer in quarter $t$ than in the previous quarter, and 0 if he/she remains in the same firm. The outcome variable in Columns (3) and (4), $IndustryRisk_{i,t}$, is a measure of layoff risk for the industry where individual $i$ works in quarter $t$, which is defined as the average quarterly labor outflow as a fraction of the total employment in a SIC 6-digit industry over the five years before the sample period.

The significantly negative coefficient on $D^{Post} \times D^{LowElev}$ in all Columns shows the low-elevation residents become less likely to change jobs or work in a risky industry after the shock, relative to high-elevation neighbors in the same county. In other words, natural disasters discourage not only entrepreneurial careers with high income risk, but also other forms of risk-taking employment decisions, which lends further support to the self-insurance mechanism.

[34]The findings also imply a potential income loss associated with the lowered motive to take a high-income high-risk entrepreneurial job.

Overall, the evidence thus far suggests natural disasters affect entrepreneurial career choices mainly through making people refrain from becoming startup employees, and this effect is connected to the impaired risk-taking capacity as a result of household wealth losses in a disaster.

## 1.5 Long-Run Impact on Labor Income

Section 1.4 shows that wealth damage from floods does not only discourage aspiring startup employees from pursuing well-paying, high-risk entrepreneurial jobs but also makes people refrain from other risk-taking decisions such as changing employers and working in high-risk industries. Since career risks are generally positively associated with income, turning away from risk-taking decisions can potentially limit one's earnings power in the long run.[35] This section aims to analyze whether the career choice distortions as a result of natural disasters have a long-term impact on earning power. In particular, I calibrate the *elasticity* of long-term income loss with respect to the dollar value of property damage in a flood. This exercise can facilitate quantifying the long-term effect of natural disasters on labor markets and thus shed light on the indirect cost of natural disasters, which can potentially inform policy decisions on environmental-risk abatement.

Two key challenges emerge in performing this exercise. First, the dollar-value property damage from a flood is unavailable at the individual level. To tackle this challenge, I build a simplified model of property damage based on the models used in the hydrological literature, which allows me to impute the property damage using observed variables such as inundation depth and housing value. Second, the counterfactual, long-term income without the property damage in a flood is unobservable. To address this issue, I exploit an RD setting similar to the one in Section 1.3 and use the income of the neighbors whose properties are marginally unflooded as the counterfactual.

[35] Furthermore, the real-option features of risky employment decisions such as entrepreneurial career choices can imply a higher sensitivity of long-run income to risk-taking decisions than the naive "risk-return" trade-off associated with a job.

### 1.5.1 Quantifying the Elasticity of Income Loss with Respect to Flood Damage

The goal of the quantification exercise is to estimate $\beta_1$ in the following model:

$$IncomeLoss_i = \alpha_1 + \beta_1 PropertyDamage_i + \varepsilon_i, \tag{1.6}$$

where $IcomeLoss_i$ is defined as the net present value (NPV) of the future income loss for individual $i$ at the time of the flood event. Due to the limitation of data coverage, I focus on the income loss in the 6.5 years after the flood. $PropertyDamage_i$ denotes the nominal property damage of individual $i$. Because property damage is unobservable at the personal level, I build a model of property damage in a flood using a method reminiscent of [45]. In particular, I assume that the property damage arising from a flood is proportional to the production of housing value and inundation depth. In other words,

$$PropertyDamage_i = \gamma InunDepth_i \times HousingValue_i, \tag{1.7}$$

where $InunDepth_i$ denotes the footage of a property submerged in a flood, and $HousingValue_i$ indicates the nominal home value of individual $i$ at the year end before the flood. I estimate $\gamma$ by matching the average property damage within a county in my sample to the county-level per-property damage statistics for three counties where the statistics are available:

$$\overline{PropertyDamage}_k = \gamma \frac{\sum_{i=1}^{I_k} HousingValue_i \times InunDepth_i}{I_k}. \tag{1.8}$$

$\overline{PropertyDamage}_k$ represents the average property damage as a result of a flood in county $k$. $I_k$ denotes the total number of flooded properties in county $k$ in my sample. The availability of $\overline{PropertyDamage}_k$, $HousingValue_i$, and $InunDepth_i$ in my data allows me to estimate $\gamma$ by taking a weighted average of the corresponding estimates for each county.

Taking Equation 1.7 into Equation 1.6, I obtain

$$IncomeLoss_i = \alpha_1 + \beta_2 InunDepth_i \times HousingValue_i + \varepsilon_i, \tag{1.9}$$

where $\beta_2 = \beta_1\gamma$. Because $\gamma$ can be estimated using Equation 1.8, the calibration of $\beta_1$ has been boiled down to estimating $\beta_2$ in Equation 1.9.

Since $InunDepth_i \times HousingValue_i$ can be potentially endogenous to an individuals' income, a naive ordinary least squares (OLS) method is likely to yield a biased estimate of $\beta_2$. To address the endogeneity issue, I resort to an RD design that compares the residents in the 0.1-mile-width bands on both sides of the boundary of inundated areas. In essence, the RD analysis is equivalent to using the dummy variable that indicates whether an individual is living within the inundation boundary as an instrumental variable for $InunDepth_i \times HousingValue_i$. The identification assumption is the same as that of the exercise in Subsection 1.3.1 and has been verified in Subsection 1.3.1, which allows me to jump into the estimation by running a first-stage RD regression of the following form:

$$InunDepth_i \times HousingValue_i = \beta_3 D_i^{Inundated} + \sum_{k=1}^{K} \gamma_k D_i^k + \varepsilon_i, \tag{1.10}$$

where $i$ and $k$ denote an individual and a county, respectively, and $D_i^{Inundated}$ is a dummy variable equal to 1 for the treated group, and 0 for the control. Similar to the specifications in Section 1.3.1, the treated (control) group is the individuals living within a 0.1-mile-wide band inside (outside) the inundated zone before the shock.[36] The key coefficient $\beta_3$ captures the gap between the treated and the control groups regarding the product of inundation depth and housing value.

Next, I try to estimate the reduced-form RD specification of the following form:

$$IncomeLoss_i = \beta_4 D_i^{Inundated} + \sum_{k=1}^{K} \gamma_k D_i^k + \varepsilon_i, \tag{1.11}$$

[36]The other variables are defined as in Equation 1.4.

where $\beta_4$ captures the excessive income loss that the treated group faces relative to the residents outside the inundated area. To calibrate the NPV of all the future income losses at the time of the flood, I estimate the expected loss of quarterly income for the treated group relative to the control group, by running a DID regression resembling Equation 1.1:

$$Income_{i,t} = \alpha_i + \alpha_t + \beta_5 D_t^{Post} \times D_i^{Inundated} + D_t^{Post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}, \tag{1.12}$$

where $Income_{i,t}$ is the wage of individual $i$ in quarter $t$, and the other variables are defined in Equation 1.1. The estimate builds on two years before and 6.5 years after the floods. The key coefficient $\beta_5$ captures the average quarterly income gap between treated and control groups after a flood, relative to their predetermined income levels. In other words, the estimate of $\beta_5$ represents the income loss for the treated group in an average quarter during the 6.5 years after the floods, assuming the pre-post change in income for the control group reflects the counterfactual.

To back out $\beta_4$, I calculate the NPV of the expected quarterly income loss ($\beta_5$) throughout the post period:

$$\beta_4 = \beta_5 \sum_{t=1}^{4T} \frac{1}{(1+r)^{t/4}}, \tag{1.13}$$

where T equals 6.5, the number of years in the post period and $r$ is the discount rate of the future income losses. The estimates of first-stage (Equation 1.10) and reduced-form RD specifications (Equation 1.11) allow me to derive $\beta_2$ following the standard formula of an RD analysis:

$$\beta_2 = \frac{\beta_4}{\beta_3}. \tag{1.14}$$

Assembling different parts of the model yields the expression of the elasticity of long-term income

loss with respect to property damage:

$$\beta_1 = \frac{\beta_5}{\beta_3 \gamma} \sum_{t=1}^{4T} \frac{1}{(1+r)^{t/4}}. \tag{1.15}$$

### 1.5.2 Estimation Results

According to Equation 1.15, the estimation of $\beta_1$ relies on the calibration of three parameters: $\beta_3$, $\beta_5$, and $\gamma$. Table 1.9 shows the estimates of $\beta_3$ and $\beta_5$. Panel A corresponds to Equation 1.10, where the outcome variable is the interaction term of $InunDepth$ and $HousingValue$. As expected, the coefficient on $D^{Inundated}$ loads significantly positive regardless of the specifications. According to Column (1), $\beta_3$ is estimated to be 257,100.

Panel B reports the estimation of Equation 1.12 at the individual-quarter level, based on two years before and 6.5 years after the shock. The outcome variable is the wage of an individual in a given quarter, and the other variables are defined in Equation 1.1. The significantly negative coefficient on $D^{post} \times D^{Inundated}$ is consistent with the hypothesis that the career choice distortion as a result of natural disasters has a lasting negative impact on labor income. According to Column (2), $\beta_5$ is estimated to be 330.3.

Next, I estimate $\gamma$ using Equation 1.8 based on three counties where the county-level per-property damage in the floods of interest are available. I obtain an estimate of $\gamma$ for each county by matching the average property damage in my sample with the county-level statistics, and take an average of the three estimates weighted by the number of flooded properties in each county. Based on this method, $\gamma$ is estimated to be 5.03%, indicating one foot of inundation at the peak of a flood can translate to a 5.03% decline in property value. My estimate of $\gamma$ is comparable to the parameter in [45].

Finally, I set the discount rate $r$ in Equation 1.15 to the average five-year treasury rate from 2008 to 2014, which is 2.325%.Taking $\beta_3$, $\beta_5$, $\gamma$, and $r$ into Equation 1.15 yields an estimate of $\beta_1$ equal to 0.62. In other words, this quantification exercise shows one dollar of property damage in

a natural disaster can lead to more than 60 cents of income loss in the 6.5 years after the disaster.[37] This finding speaks to the shadow value of wealth and suggests natural disaster experience has a long-term impact on earning power beyond the direct effect on household wealth.

## 1.6 Conclusion

This paper investigates how wealth damage affects entrepreneurial career choices using a set of staggered floods in the US as an exogenous shock to household wealth. The analysis builds on an individual-level matched dataset combining the LEHD data from the US Census Bureau, Corelogic deeds records, and a battery of GIS data. Within the 0.1-mile-wide bands on both sides of the boundary of areas actually inundated, the individuals whose properties are inundated are less prone to entrepreneurial careers relative to their unaffected neighbors after the flood. Within a county after a flood, individuals residing in low-elevation areas refrain from entrepreneurial jobs relative to the high-elevation neighbors, which further confirms the adverse effect of natural disaster experience on entrepreneurial career choices. This effect is mainly attributed to the impaired household wealth due to the floods, and manifests itself in the lowered propensity to become entrepreneurial employees more than startup founders. Further mechanism analysis shows the wealth effect on entrepreneurial career choices is likely to be driven by the lowered self-insurance capacity as a result of the financial loss. In particular, flood damage makes people refrain from high-income, high-risk startup jobs as well as other risk-taking employment decisions, leading to a long-run income loss of more than 60 cents for each dollar of property damage in a flood.

Overall, I find even relatively modest wealth loss can have a pronounced discouraging effect on the pursuit of entrepreneurial careers. My analysis further the understanding of the motive to become startup employees, which can potentially inform the policy designs aiming to stimulate entrepreneurial activities. The mechanism analysis reveals the importance of the self-insurance provided by household wealth in motivating the risky career choice to become a startup employee,

[37] Alternatively, using the compounded annual return of the S&P 500 index from 2008-2014 as the discount rate suggests one dollar of property damage translates to more than 50 cents of income losses in the 6.5 years after the disaster.

which has not been documented before. In addition, documenting the career distortion and income loss as a result of natural disasters facilitates evaluating the indirect cost of natural disasters, potentially contributing the policy discussion regarding environmental-risk abatement and natural disaster insurance.

Table 1.1: **Summary Statistics.** The table reports the summary statistics of the main sample. $Elevation$ is the elevation of an individual's residential property in feet. $D^{startup}$ denotes a dummy variable equal to 1 if an individual worked in a startup in a given quarter, and 0 if he/she worked for a more matured firm. $Education$ is the highest degree a person obtains with a scale of 1-4, where 1 represents a high school diploma or below, and 4 indicates a graduate degree. $Age$ is the age of an individual at the time of the flood. $Male$ denotes a dummy equal to 1 for male, and 0 for female. $Income$ is an individual's average quarterly wage from the primary jobs over the sample period. Columns (1) to (3) report the mean, standard deviation, and pseudo median.

| | Mean [1] | Standard Deviation [2] | Pseudo Median [3] |
|---|---|---|---|
| Elevation | 168.2 | 151.6 | 153.2 |
| $D^{startup}$ | 0.113 | 0.317 | 0 |
| Education | 2.713 | 0.992 | 3 |
| Age | 37.80 | 15.48 | 37 |
| Male | 0.535 | 0.499 | 1 |
| Income | 7,836 | 17,670 | 4,587 |

Table 1.2: **RD Design: Covariate Balance Analysis.** The table compares the treated and control groups in the RD strategy regarding several biographical attributes at the individual level. $Income$ is defined as the average quarterly income of an individual in the two years leading up to the shock. $D_{pre}^{startup}$ denotes a dummy variable equal to 1 if an individual worked in a startup before the shock, and 0 if he/she worked for a more matured firm. $Age$ is defined as the age of an individual at the time of the flood. $Male$ denotes a dummy equal to 1 for male, and 0 for female. $Education$ is the highest degree a person obtains with a scale of 1-4, where 1 represents a degree below high school, and 4 indicates a graduate degree. *Home value* is the home value in US dollars at the time of the flood. Columns (1)-(3) reports the mean of the treated group, the control group, and the difference between the two groups, of which the T-statistics are reported in Column (4). The treated (control) group is residents living within the 0.1-mile-wide band inside (outside) the inundation boundary before the shock. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| Predetermined Attributes | Treated [1] | Control [2] | Difference [3] | T-statistics [4] |
|---|---|---|---|---|
| Income | 6,540 | 6,665 | -124.8 | (-0.57) |
| $D_{pre}^{startup}$ | 10.1 | 10.1 | 0.007 | (0.94) |
| Age | 38.91 | 38.95 | -0.036 | (-0.10) |
| Male | 0.583 | 0.591 | -0.008 | (-0.64) |
| Education | 2.583 | 2.599 | -0.016 | (-0.68) |
| Home Value | 243,800 | 238,200 | 5,600 | (1.23) |
| Flood Risk | 1.173 | 1.159 | 0.014 | (1.53) |
| Employer Inundation Status | 0.160 | 0.142 | 0.018 | (0.48) |

Table 1.3: **RD Design: Regression Analysis.** This table reports the estimates of Equation 1.1 at the individual-quarter level, based on the individuals living in the 0.1-mile-wide bands on both sides of the boundary of the inundated area in a flood. The outcome variable is $D^{startup}$, a dummy variable set to 1 if an individual works for a startup as a primary job during a given quarter, and 0 if he/she works for a more matured firm. $D^{Inundated}$ is a dummy variable equal to 1 if an individual lived inside the inundation boundary before the shock, and zero otherwise. The definitions of other explanatory variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{startup}$ | | $D^{startup}$ | |
|---|---|---|---|---|
| | [1] | [2] | [3] | [4] |
| $D^{Post} \times D^{Inundated}$ | -0.780*** | -0.750** | -0.775** | -0.743** |
| | (-2.59) | (-2.55) | (-2.57) | (-2.53) |
| Year FE | Yes | Yes | No | No |
| Individual FE | Yes | Yes | Yes | Yes |
| Quarter FE | No | No | Yes | Yes |
| County × Post FE | Yes | No | Yes | No |
| N | 80,500 | 80,500 | 80,500 | 80,500 |
| R-sq | 0.51 | 0.509 | 0.51 | 0.51 |

Table 1.4: **First-Stage Analysis of DID Design: Elevation and Flood Exposure.** Columns (1)-(4) of Panel A report the estimates of Equation 1.2 at the property level, and Columns (5)-(6) of Panel A and Panel B report the estimates of Equation 1.3 at the property-year level. The outcome variable in Columns (1)-(2) of Panel A is $InunDepth$, the footage of a property submerged in a flood. The outcome variable in Columns (3)-(4) of Panel A is $Damage$, which denotes the damage level a flood inflicts on a property with a scale of 1 to 5. The outcome variable in Columns (5)-(6) of Panel A is $HomeValue$, the value of a property in a given year measured by the assessor pricing. The outcome variable in Panel B is $Foreclosure$, a dummy variable equal to 1 if property $i$ experiences a foreclosure transaction in year $t$, and 0 otherwise. $Elevation$ is defined as the elevation of a property. $D^{post}$ is a dummy variable equal to 1 for all years after the shock, and 0 otherwise. T-statistics are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

Panel A: Elevation, Inundation, and Home Value Loss

| | Inundation Depth | | Damage Level | | Housing Value | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| | | | | | Nominal | Standardized |
| Elevation | -0.033*** | -0.505*** | -0.008*** | -0.131*** | | |
| | (-22.22) | (-64.99) | (-17.53) | (-53.76) | | |
| $D^{Post}\times$ Elevation | | | | | 3.478*** | 0.053*** |
| | | | | | (4.01) | (6.32) |
| County FE | Yes | No | Yes | No | No | No |
| Zipcode FE | No | Yes | No | Yes | No | No |
| Post × County FE | | | | | Yes | Yes |
| Year FE | | | | | Yes | Yes |
| Property FE | | | | | Yes | Yes |
| N | 35,609 | 35,452 | 35,609 | 35,452 | 1,491,419 | 1,487,066 |
| R-sq | 0.110 | 0.471 | 0.102 | 0.452 | 0.993 | 0.943 |

Panel B: Elevation and Foreclosure Rate

| | Foreclosure | | | Foreclosure | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| | [-2,1] | [-2,2] | [-2,3] | [-2,1] | [-2,2] | [-2,3] |
| $D^{Post}\times$ Elevation | -0.104*** | -0.091*** | -0.094*** | -0.094*** | -0.076*** | -0.077*** |
| | (-4.39) | (-4.38) | (-4.79) | (-3.46) | (-3.18) | (-3.44) |
| Post × State FE | Yes | Yes | Yes | No | No | No |
| Post × County FE | No | No | No | Yes | Yes | Yes |
| Property FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 10,012,312 | 12,515,390 | 15,018,468 | 10,012,312 | 12,515,390 | 15,018,468 |
| R-sq | 0.250 | 0.202 | 0.168 | 0.250 | 0.202 | 0.168 |

Table 1.5: **DID Design: Do Lower-Elevation Residents Become Less Entrepreneurial After a Flood?** This table reports the estimates of Equation 1.4 at the individual-quarter level. The outcome variable is $D^{startup}$, a dummy variable set to 1 if an individual works for a startup as a primary job during a given quarter, and 0 if he/she works for a more matured firm. $D^{LowElev}$ is defined as a dummy variable equal to 1 if the elevation of the property where an individual lives before the flood falls in the bottom three deciles within a county, and 0 if the elevation falls in the top three deciles. $D^{post}$ is a dummy variable equal to 1 for all years after the shock, and 0 otherwise. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{startup}$ | | | | |
|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] |
| $D^{Post} \times D^{LowElev}$ | -0.487*** | -0.498*** | -0.524*** | -0.486*** | -0.500*** |
| | (-3.39) | (-3.18) | (-3.36) | (-3.55) | (-3.19) |
| Quarter FE | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes |
| Post × County FE | Yes | Yes | No | No | No |
| Post × Firm FE | Yes | No | No | No | No |
| Year × County FE | No | No | No | Yes | Yes |
| Year × Firm FE | No | No | No | Yes | No |
| N | 2,144,000 | 2,179,000 | 2,179,000 | 2,144,000 | 2,179,000 |
| R-sq | 0.77 | 0.564 | 0.564 | 0.849 | 0.565 |

Table 1.6: **Wealth Effect Versus Pure Psychological Impact: Are Homeowners Particularly Affected?** This table reports the estimates of Equation 1.4 at the individual-quarter level, based on the subsample of homeowners in the flooded counties. The definitions of other explanatory variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.The quantitative results are currently under review by US Census Bureau and will be added to tables as soon as the disclosure request is approved.

| | $D^{startup}$ | |
|---|---|---|
| | [1] | [2] |
| $D^{Post} \times D^{LowElev}$ | -0.710** | -0.760** |
| | (-2.24) | (-2.26) |
| Quarter FE | Yes | Yes |
| Individual FE | Yes | Yes |
| Post × County FE | Yes | No |
| N | 63,000 | 63,000 |
| R-sq | 0.477 | 0.477 |

Table 1.7: **Heterogeneity in the Effect: Startup Founders Versus Employees.** This table reports the estimates of Equation 1.4 at the individual-quarter level with alternative dependent variables. In Columns (1), (3), and (5), the outcome variable is $D^{Startup}_{Founder}$, a dummy variable equal to 1 if an individual is a startup founder in a given quarter, and 0 if he/she works as a startup employee or in a matured firm. Following [9], I use being among the top three earners in a startup as a proxy for startup founders. The outcome variable in Columns (2), (4), and (6) is $D^{Startup}_{Employee}$, a dummy variable equal to 1 if a person works as an startup employee (i.e., ranked below top three in earnings) in a given quarter, and 0 if he/she is a startup founder or works in a matured firm. The definitions of other explanatory variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{Startup}_{Founder}$ [1] | $D^{Startup}_{Employee}$ [2] | $D^{Startup}_{Founder}$ [3] | $D^{Startup}_{Employee}$ [4] | $D^{Startup}_{Founder}$ [5] | $D^{Startup}_{Employee}$ [6] |
|---|---|---|---|---|---|---|
| $D^{Post} \times D^{LowElev}$ | -0.019 | -0.468*** | -0.010 | -0.489*** | -0.009 | -0.515*** |
| | (-0.33) | (-3.42) | (-0.11) | (-3.61) | (-0.11) | (-3.81) |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County × Post FE | Yes | Yes | Yes | Yes | No | No |
| Firm × Post FE | Yes | Yes | No | No | No | No |
| N | 2,144,000 | 2,144,000 | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 |
| R-sq | 0.818 | 0.755 | 0.622 | 0.546 | 0.621 | 0.545 |

Table 1.8: **Why Does Wealth Loss Affect Aspiring Startup Employees?** Panel A reports the estimates of Equation 1.4 at the individual-quarter level with alternative dependent variables. In Columns (1) and (2) (Columns (3) and (4)), the outcome variable is $D^{Startup}_{LowerIncome}$ ($D^{Startup}_{HighIncome}$), a dummy variable equal to 1 (0) if in a given quarter, an individual holds an entrepreneurial job that provides lower (higher) average income than his/her previous job. The outcome variable in Columns (5) and (6) (Columns (7) and (8)) is $D^{Startup}_{LowerRiks}$ ($D^{Startup}_{HigherRiks}$), a dummy variable set to 1 (0) if an individual holds an entrepreneurial job with lower (higher) income volatility than his/her previous job in a given quarter. Panel B reports the estimates of Equation 1.4 at the individual-quarter level with alternative dependent variables. The outcome variable in Columns (1) and (2) is $D^{Churn}$, a dummy variable equal to 1 if an individual is working for a different employer in a given quarter than in the previous quarter, and 0 he/she remains in the same firm as in the previous quarter. The outcome variable in Columns (3) and (4), $IndustryRisk$, is defined as the average quarterly labor outflow as a percentage of the employment for all firms in the SIC 6-digit industry of an individual's employer over the five years before the sample period. The definitions of other explanatory variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

Panel A: Luxury Good Consumption or Self-Insurance Mechanism?

| | $D^{Startup}_{LowerIncome}$ | | $D^{Startup}_{HigherIncome}$ | | $D^{Startup}_{LowerRisk}$ | | $D^{Startup}_{HigherRisk}$ | |
|---|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| $D^{Post} \times D^{LowElev}$ | -0.171 | -0.159 | -0.352*** | -0.339*** | -0.138 | -0.125 | -0.374*** | -0.386*** |
| | (-1.54) | (-1.43) | (-2.98) | (-2.86) | (-1.26) | (-1.14) | (-3.10) | (-3.22) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County × Post FE | No | Yes | No | Yes | No | Yes | No | Yes |
| N | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 |
| R-sq | 0.547 | 0.548 | 0.533 | 0.533 | 0.537 | 0.537 | 0.539 | 0.539 |

Panel B: Further Evidence on Self-Insurance Mechanism

| | $D^{Churn}$ | $D^{Churn}$ | $IndustryRisk$ | $IndustryRisk$ |
|---|---|---|---|---|
| | [1] | [2] | [3] | [4] |
| $D^{Post} \times D^{LowElev}$ | -0.329*** | -0.329*** | -0.084** | -0.078** |
| | (-3.41) | (-3.42) | (-2.11) | (-1.96) |
| Quarter FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| County × Post FE | Yes | No | Yes | No |
| N | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 |
| R-sq | 0.255 | 0.255 | 0.742 | 0.742 |

Table 1.9: **The Elasticity of Income Loss with Respect to Property Damage** Panel A reports the estimation of Equation 1.10 at the individual level. The outcome variable is the interaction term of $InunDepth$ and $HousingValue$, where $InunDepth$ denotes the footage of a property submerged in flood, and $HousingValue$ indicates the nominal home value of an individual at the year end before the flood. Panel B reports the estimation of Equation 1.12 at the individual-quarter level. The outcome variable is the wage of an individual in a give quarter. The definitions of other explanatory variables are consistent with Table 1.3. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

Panel A: First-Stage RD Analysis

| | InunDepth × HousingValue | | Property Damage | |
|---|---|---|---|---|
| | [1] | [2] | [3] | [4] |
| $D^{Inundated}$ | 267,100*** | 257,100*** | 13,350*** | 12,860*** |
| | (23.55) | (22.20) | (23.45) | (22.21) |
| County FE | No | Yes | No | Yes |
| N | 6,700 | 6,700 | 6,700 | 6,700 |
| R-sq | 0.077 | 0.087 | 0.077 | 0.087 |

Panel B: Reduced-Form RD Analysis

| | Income | |
|---|---|---|
| | [1] | [2] |
| $D^{post} \times D^{Inundated}$ | -330.3** | -328.0** |
| | (-2.00) | (-1.99) |
| Year FE | Yes | No |
| Quarter FE | No | Yes |
| Individual FE | Yes | Yes |
| County × Post FE | Yes | Yes |
| N | 162,000 | 162,000 |
| R-sq | 0.533 | 0.534 |

# Chapter 2: Going Mobile, Investor Behavior, and Financial Fragility

## 2.1 Introduction

Using individual-level mutual fund trading data, this paper examines how the innovation in trading technology affects the financial fragility of mutual funds by changing investor behavior. In the past decade, the snowballing adoption of smartphones and the expanded coverage of wireless networks have revolutionized the way people access financial services. An example is the rise of mobile trading applications (hereafter, "apps") in asset management, which allow investors to monitor portfolios and place trades in a real-time manner from any location with an internet connection. While mobile trading has become increasingly mainstream, how "going mobile" influences investor behavior and financial markets remains an open question.

On the positive side, "going mobile" may benefit investors by reducing the cost of placing trades and acquiring information. Smartphone trading may also relax investor attention constraints, which can potentially facilitate informed decisions and prevent mistakes due to negligence [46, 47, 48, 49]. On the negative side, mobile trading technology may amplify the behavioral biases of retail investors. For example, access to abundant information might create an illusion of knowledge (control), which in turn boosts investors' overconfidence and spurs excessive trades [50, 51]. With the presence of systematic cognitive biases, the trades may translate into fund flow shocks. The resulting increase in flow can force mutual funds into costly adjustments to their portfolios and therefore depress fund performance and even distort the prices of the assets held by the funds [52, 53, 54].

In this paper, I document how trading technology innovation reshapes investor behavior and affects mutual fund fragility. I exploit a natural experiment, namely, the release of a new mobile

trading app by one of the largest investment advisers in China.[1] By transforming the trading app's architecture, the upgrade significantly improved the app's functionality and substantially expanded its use. This technology shock allows me to pin down the incremental effect of "going mobile" on retail investors. In particular, I compare the outcomes for the investors who switched to the app after the launch of the new app (adopters) and the investors who kept approaching the trading platform via desktops (nonadopters), both before and after the technology shock. This empirical setting, combined with individual-level proprietary data on the transaction history and biographical attributes, allows me to investigate the research question at the micro level. Also, I use a spatial-discontinuity design to strengthen the causal inference.

My analysis starts with investigating the technology's effect on investor behavior. My difference-in-differences (DID) analysis indicates that after the launch of the new app, investor attention (measured by the login to the trading platform) increased by 146% among app adopters, and their trading intensity was inflated by 37%, relative to the ex-ante equally active investors who did not adopt the app.[2] Further analysis suggests the technology-trade relationship can be operative through reinforcing investors' behavioral biases such as over-confidence and self-control problems. The triple-differences analysis shows that the increase in trading volume resulting from "going mobile" is particularly pronounced among male investors and the investors who beat the benchmark by taking excessive risks in the past. Both groups are presumably more susceptible to overconfidence than other investors [55, 56, 57]. Also, the technology's impact on trading intensity decreases with age (a proxy for self-control ability [58]) and increases with the day of the week (a proxy for the depletion of self-regulation capacity), consistent with the hypothesis that insufficient self-regulation capacity partly explains the effect of the technology. Together, the evidence above points to the notion that "going mobile" is likely to amplify investors' behavioral biases and thus spur trading activity and investor attention.

[1]The investment adviser is referred to in the paper anonymously as the Adviser.

[2]The findings are consistent with a Wall Street Journal article in January 2019. According to the article, the users of mobile trading apps such as Robinhood engage with the app the way they engage with Instagram or WhatsApp, checking the app as frequently as 10 times a day and generating a ludicrous amount of trades for Robinhood especially in low-price stocks.

The influence of "going mobile" does not stop at the individual behavior. My analysis suggests that the heightened trading volume at the individual level adds up to an increase in investor flows at the fund level. After the shock, adopters' flow becomes twice as volatile as before. The increase is significant relative to the pre-post change for nonadopters. Two specific trading patterns explain the flow effect. The first is intensified flow-performance sensitivity. After the shock, the adopters' flow becomes more sensitive to fund returns in the prior week relative to the control group, whereas investor flow is insensitive to the previous week's performance prior to the shock for both groups. The second pattern is the increase in susceptibility to market sentiment. Using nonadopters as a benchmark, the association between adopters' flow and the latest market sentiment increases by 0.14 after the technology shock. The spatial-discontinuity analysis confirms the causal inference of the above results.

The increased investor flow from "going mobile" lowers mutual fund performance. Following the shock, the risk-adjusted returns of the affected mutual funds decrease by 4% annually from the pre-period level, primarily due to poor performance subsequent to massive flows from the trading app. To strengthen the causality, I compare funds with varying degrees of exposure to the technology. I find that around the shock, the high-exposure funds witness a greater decline in abnormal returns than the low-exposure funds. Likewise, the performance gap is mainly driven by the low abnormal returns of high-exposure funds in the event of large flows from the trading app.

A likely concern about the above research design is the self-selection into technology adoption. To address the endogeneity issue, I exploit a spatial-discontinuity setting. I restrict my analysis to the investors residing in pairs of contiguous prefectures separated by the borderline between two provinces with different levels of exposure to the technology shock. In China, the geographic segmentation of the telecommunication industry and the heterogeneity of network coverage/quality arising from the variance in local carriers creates a decent amount of cross-province variation in the prevalence of smartphone use. Because possessing a smartphone is the prerequisite for app adoption, the investors ex-ante living in a province with a high smartphone penetration rate are

more likely to adopt the app and more exposed to the technology shock.[3]

Accordingly, I use the predetermined smartphone penetration rate in an investor's province of residence prior to the shock as a proxy for the state-specific policy related to mobile networks, which presumably affects app adoption. To the extent that the unobserved characteristics change continuously around the province border, this method essentially compares the behavior change of the investors with different likelihoods of adopting the app and otherwise alike.[4]

I implement the strategy by comparing the behavior of the high-exposure and low-exposure investors within each pair of neighboring prefectures at the province border, before versus after the introduction of the app. The spatial-discontinuity analysis confirms the above findings. According to the two-stage least squares (2SLS) analysis, adopting the app for exogenous reasons more than doubles investor attention, and raises their trading volume by 86%. Also, I find that the technology shock makes the high-exposure investors increasingly sensitive to the fund performance in the prior week and increasingly susceptible to market sentiment, relative to the less affected counterparts in the neighboring prefecture.

This paper contributes to three strands of literature. First, it contributes to the growing literature on financial technology. The transition from traditional online trading to mobile trading has become ubiquitous in the brokerage and asset management. The paper represents one of the earliest endeavors to understand how this transformation affects investors' behavior and financial markets. It documents detailed channels through which the technology intensifies the mutual fund fragility. The individual-level trading data allow me to explore specific behavioral biases and trading co-movement the technology reinforces to hurt fund performance. A few related papers investigate how online investment advising affects people's portfolio decisions and the financial markets (e.g., [59, 60, 61, 62]). While these papers focus on how new information affects investors, my paper fo-

[3]According to the Consumers and Mobile Financial Services 2016 report issued by the Board of Governors of the Federal Reserve System, smartphone adoption is a major driver for the use of various mobile financial services and applications.

[4]Strictly speaking, the continuity of the underlying functions of covariates against the distance to province border around the border is a sufficient but not necessary condition for the validity of the strategy. Due to difference-in-differences nature of the analysis, the design holds as long as the outcomes would have changed in parallel for the adjacent high-exposure and low-exposure prefectures in absence of the shock.

cuses on the evolution of the trading infrastructure while holding constant the information carried by the infrastructure.

Second, this paper adds to the literature on the fragility of open-end structures and the destabilizing effect of mutual fund flows. The demandable liquidity term provided by open-end structures can make their performance particularly vulnerable to investor flows. Previous studies have documented the negative association between the size of investor flows and the subsequent fund performance and interpreted the relationship as a form of liquidity cost [52, 63, 64].[5] However, establishing the causality between investor flows and fund performance is difficult because large investor flows are likely to be confounded by the change in fundamentals. My paper sheds light on the causal inference between investors' liquidity demand and fund performance by introducing an arguably exogenous shock to investor flows and examining its differential effect on mutual funds with various exposure. Also, my finding on the flow sensitivity to market sentiment suggests that, from a micro perspective, the investor flows fueled by mobile technology are not contrarian but rather a potential destabilizing force, echoing the spirit in [53], [65], and [66].

Lastly, this study illustrates how the internet shifts the allocation of investor attention among different types of information. This question is relevant given the central role of attention in investment decisions and its potential impact on asset prices [67, 68, 69, 70, 71, 72]. [73] posit that because the internet reduces the cost of real-time information more than others, short-term trends will increasingly capture individual investors' attention and unduly affect investment decisions. However, little research has been done to document the horizon shift in investor attention.[6] This article shows novel evidence that mobile trading technology draws investors' attention to the very recent trends in fund performance (and market sentiment) and induces them to "coordinate" in their trades accordingly[7], which supports the conjecture in [73].

[5] In theory, investor flows can lower fund return by forcing them into performing costly liquidity-motivated trades and preventing them from conducting profitable arbitrage [54].

[6] A handful of papers study how online social networks and online interactions among web users affect individual investment, such as [74] and [75]. These studies mainly focus on the channel via which the information is obtained rather than the horizon of the information that captures investor attention.

[7] My analysis shows that the investor flow of the app adopters is not sensitive to the past week's performance before the app upgrade and becomes significantly sensitive after the shock. In unreported analysis, the sensitivity to past month's performance does not increase significantly.

The remainder of this paper is organized as follows: Section 2.2 discusses the institutional background; section 2.3 outlines the framework; section 2.4 details the empirical design (both baseline and spatial-discontinuity strategy) and the empirical results on how "going mobile" affects retail investor behavior; section 2.5 analyzes the effect on investor flow and mutual fund performance; section 2.6 concludes.

## 2.2 Institutional Background and Data

### 2.2.1 The Mutual Fund Industry In China

As in the US, investment advisers (i.e., fund management companies) are the main players in China's mutual fund industry. A typical investment adviser manages a mutual fund family consisting of multiple mutual funds on behalf of investors. By the end of June 2017, China had 4,355 mutual funds under 117 fund families, 40% managed by stand-alone investment advisers and the rest by affiliations of financial-service conglomerates. The revenue of investment advisers primarily comes from the management fee based on a fixed percentage of the assets under management (AUM).[8] Transaction fees[9] are charged to compensate the distributors that handle the purchase and redemption of fund shares for investors. Although most of the trades of fund shares are processed by external distributors, such as banks and brokerage companies, direct sales have become increasingly common in the industry.

My analysis features the launch of a mobile trading app as a part of the direct sales infrastructure at one of the largest investment advisers (hereafter, the Adviser) in China. The Adviser has 300 billion RMB (approximately $43 billion) of AUM and provides 65 mutual funds ranging from equity, bond, and hybrid funds to money market funds. My analysis focuses on the 27 equity and hybrid mutual funds from the Adviser.

[8]The annual management fees are around 1.5% of AUM for most mutual funds.

[9]The transaction fees mostly take the form of front-end loads and occasionally of back-end loads.

### 2.2.2 The Technology Shock of "Going Mobile"

The mutual funds under the Adviser can be traded via two types of distribution channels. First, investors can trade through a third-party distributor such as a bank or a brokerage house. Second, they can use an in-house direct sales platform, which allows investors to trade fund shares directly with the Adviser. Cutting out the middleman gives an edge to the direct sales channel and makes its transaction fees[10] substantially lower than that of third-party distributors. As a result, investors broadly embrace the direct sales channel.

Prior to the launch of the new app, two methods are available for accessing the direct sales platform. The predominant method was the use of an official website mostly via an internet browser on a PC. A visit to the website enables investors to trade mutual funds from the Adviser, as well as retrieve information such as market updates, fund profile and performance, and so on. The second method is to use a "client terminal," an interactive web-based app designed for feature phones. In contrast to the popularity of the website, the adoption of the "client terminal" is negligible. Due to its backward web-based architecture, the app responded slowly and easily lost connection, which largely explains its rare adoption.[11]

In September 2013, the Adviser launched a major upgrade to the direct sales platform. Although the website remained unchanged, the Adviser replaced the web-based app with a native app. Compatible with smartphones, the new native app features a modern interface that delivers comprehensive services. The new app outclasses its predecessor along many dimensions. First, it runs faster and more smoothly than the "client terminal." The transformation of architecture empowers the new app to work in tandem with the local devices instead of the remote server, which considerably improves the speed and reliability.[12] Second, the interface of the new app is more

[10] Transaction fees (front-end loads) for trades conducted using the direct sales platform average 10-15 basis points of the transaction amounts, whereas the fee rates via the third-party distributors are 100-150 basis points.

[11] For the web-based application, the software and database reside in the central server rather than being installed on the local devices, and every action (every click) needs to be processed by the remote server through the internet. Therefore, the web-based app is much slower than the apps installed on the local devices, and its stability largely depends on the quality of the internet connection. In addition, the user interface is not user friendly, which makes the app hard to use.

[12] A web-based app is a software package one can use through a web browser or a user terminal. The software and database of a web-based app locate on a central server and can only be accessed over the internet. By contrast, a native

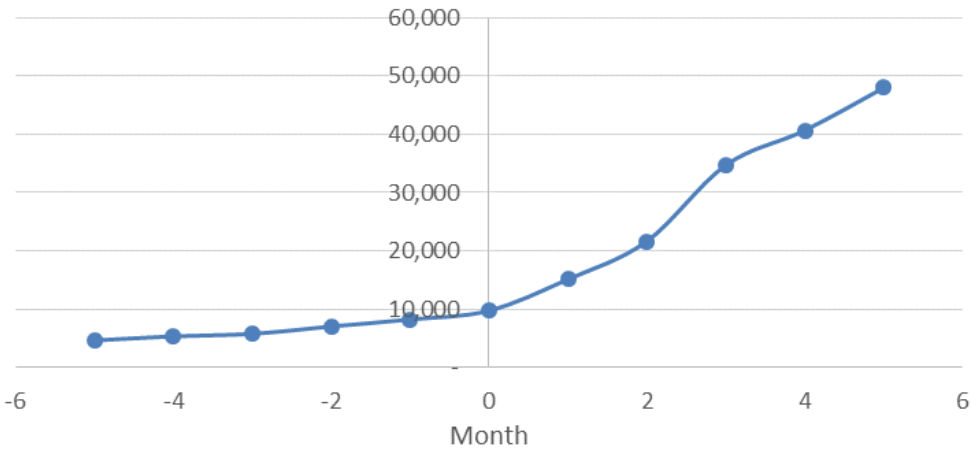



Figure 2.1: **Number of Active Users on the Direct Selling Platform.** The figure plots the total number of active users in each month around the technology shock. The horizontal axis represents the month relative to the app upgrade, and month 0 corresponds to the event month. The vertical axis is the number of mutual fund investors who are using the mobile trading app during a given month.

user-friendly and more functional, allowing users to monitor portfolios, place orders, and acquire information with a few clicks. Third, the new app streamlines the wire transfer between trading and banking accounts by retrieving related information from the mobile devices.

As a result, the new app experienced a fast pace of adoption among investors. As shown in Figure 2.1, the total number of active users on the platform had been steadily growing since early 2013 and exhibited a sharp acceleration after the debut of the new trading app. Within three months of the upgrade, the number of active users more than tripled.

### 2.2.3 Data

My sample covers 27 equity and hybrid mutual funds provided by the Adviser. I obtain the daily number of shares and net asset value (NAV) from the Adviser and calculate the daily returns of all funds based on the closing prices and the capital distribution. I construct fund returns in longer terms by compounding daily returns.

app is an application developed for a particular platform or operating system and is installed on the local devices. As a result, a native app can take advantage of various features of the particular device where it is installed and is capable of interacting with other software on the same device.

The study relies on a proprietary administrative database that documents all the transactions with retail investors on the direct sales platform during 2012-2014. The original data contain observations at the investor-fund-day level. My analysis focuses on the 51,771 pre-existing investors who had used the direct sales platform to trade prior to the technology shock. I collapse the daily data to investor-month panel data to examine the dynamics of investor trading intensity. Similarly, I use the investor-fund-week-level data to investigate how "going mobile" affects flow-performance sensitivity and susceptibility to market sentiment. I also get access to the backstage data tracking the logins on the direct sales platform through both the website and the mobile app, which allow me to construct the measure of investor attention.

The dataset also encompasses a variety of investor biographical attributes including age, gender, educational attainment, occupation, and so on. Age and gender information comes from resident identity cards, photocopied at the initiation of mutual fund trading accounts. Education and occupation are retrieved from the survey conducted during the account set-up and are self-reported by about 20% of the investors. Also, investors' residential addresses are collected and geocoded, complemented by a set of prefecture characteristics from the 2010 Population Census of China.

Table 2.1 reports summary statistics of mutual funds and investors in my sample. The definitions all all variables presented are as follows. *AUM* denotes the assets under management of a mutual fund. *Fund Age* is the number of years between the app upgrade and the fund initiation. *% Equity* denotes the value of the equity holdings as a percentage of the total assets in the portfolio. *Management Fee* is the annual management fee as a percentage of AUM. *Custodian Fee* is the annual custodian fee as a percentage of AUM. *% Retail Investors* is the fraction of the fund shares held by retail investors as of 2012. *Fund Return* is the accumulative annual NAV returns for a fund. *Net Investor Flow* is the number of shares purchased in excess of the number of shares redeemed in a given week as a percentage of the total number of shares at the end of the prior week. *Male* is a dummy variable taking a value of 1 for male investors and zero for female investors. *College Educated* denotes a dummy variable equal to 1 for an investor who has attended college and zero otherwise. *Login Frequency* is the monthly logins into the direct sales online platform before the

shock. *Account Size* is the total number of shares held by an investor at the app upgrade. *Trading Volume* is the number of shares traded per month before the shock. *Trading Frequency* is the number of trades conducted in a month. *Login Frequency* is the monthly logins into the direct sales online platform before the shock. *Account Size* is the total number of shares held by an investor at the app upgrade. *Trading Volume* is the number of shares traded per month before the shock.

As shown in Panel A, an average mutual fund has 3.2 billion RMB of AUM at the end of 2012, and common stocks account for 85% of its portfolio. The funds charge 1.3% of the investment amount for the management fee and 0.22% for custodian service on an annual basis. Retail investors outweigh institutional investors and hold 71% of fund shares prior to the shock. During the sample period, an average mutual fund delivers an annual return of 9% net of the fee. In aggregate, purchase and redemption balance out, whereas the standard deviation of weekly net flow[13] is as high as 6.21%.

Panel B reports investor-related statistics. A typical retail investor is around 40 years old and is equally likely to be male or female. Among those who self-report educational attainments, 21% have attended a college. An average investor logs into the direct sales platform (either via the app or the website) 3.8 times per month, and hold 38,000 fund shares in the trading account. Investors typically rebalance once in three months and on average trade 1,141 shares per month.

### 2.2.4 Who Adopts the Mobile Trading App?

Before the formal analysis, I investigate the determinants of app adoption. To do so, I run a cross-sectional regression to estimate a probit model of the following form:

$$Adoption_i^* = \alpha + \beta_1 Age_i + \beta_2 Education_i + \beta_3 Male_i + \beta_4 LoginFreq_i + \varepsilon_i \tag{2.1}$$

[13] Net investor flow is defined as the number of shares purchased minus the number of shares redeemed during a period as a percentage of the total number of shares in a fund at the end of the previous period.

$$D_i^{adopter} = \begin{cases} 1 & Adoption_i^* > 0 \\ 0 & Adoption_i^* <= 0, \end{cases} \tag{2.2}$$

where $D_i^{adopter}$ is a dummy variable equal to 1 for investors who adopt the app after the upgrade, and 0 otherwise. Hereafter, I refer to the investors with $D_i^{adopter} = 1$ as "adopters" and those with $D_i^{adopter} = 0$ as "nonadopters." $D_i^{adopter}$ is modeled as an indicator for whether the latent variable $Adoption_i^*$ is positive. $Adoption_i^*$ depends on a set of explantory variables: $LoginFreq_i$ denotes the average monthly login times for investor $i$ prior to the launch of the new app; $Age_i$ is investor $i$'s age as of 2013; $Education_i$ is an integer with a scale of 0 to 5, increasing with the highest educational degree investor $i$ obtained; $Male_i$ takes a value of 1 for males and 0 for females. The coefficients $\beta_1$ - $\beta_4$ capture the assocation between the latent variable and the corresponding individual attributes.

Table 2.2 reports the determinants of app adoption. Corresponding to equation 2.2, column (1) shows that young men who monitor their portfolio more frequently prior to the shock are more likely to adopt the mobile trading app. Also, the positive loading of $Education$ indicates the likelihood of adoption increases with educational attainment. To examine whether the result is robust with respect to the definition of technology adoption, I use two alternative measures: in column (2) (column (3)), $D^{adopter}$ equals 1 for the investors who use the app within 12 months (6 months) from the upgrade, and 0 otherwise. The estimates in the last two columns confirm the findings in column (1).

## 2.3 Framework

This section discusses the framework and outlines the empirical predictions. I start by discussing a set of predictions regarding the immediate impact of "going mobile" on investor behavior. The second part explores the possible effects on mutual fund operation (in particular, on the indirect liquidity costs) that follow naturally should the hypothesized effect on investor behavior

exist.

The technology's effect on investor behavior is theoretically inconclusive. One may argue "going mobile" should have a limited impact on investor behavior. Migrating from the desktop platform to the mobile app does not change the information set one can access through the platform. Also, the direct reduction of (nonpecuniary) transaction costs brought about by the improved infrastructure can be immaterial, in which case, seeing that "going mobile" has an insignificant effect on the trading behavior of investors would be unsurprising [76].

Another view is that the app adoption can significantly impact investors' decisions. By allowing portfolio managing during the fragmented time, the mobile trading app can reduce the (opportunity) cost of investor attention and spur frequent reflections on investment [77]. More attention and faster feedback may influence trading behavior [71, 73]. For example, the easy access to financial information through the app may boost investors' sense of control, create an illusion of knowledge, and therefore amplify their overconfidence and lead to excess trades [51].[14] Another possibility is the app may trigger self-control problems. Psychologists have found people with lower self-regulating capacity are more likely to become addicted to using smartphones [78]. Similarly, people with self-control problems may find particular difficulty in resisting the temptation to trade on the app, which can potentially provide an entertainment resembling what lotteries or video games bring.

To examine the impact of "going mobile" on investor behavior, I focus on two dimensions: investor attention and trading intensity. In particular, I hypothesize that using the app spurs investor attention and raises the trading volume.

**Prediction 1A:** After the launch of the new app, adopters pay more attention to their mutual fund portfolios and trade more intensively than before launch, relative to nonadopters.

Following [79] and [68], I use the login to the online trading platform as a measure of investor attention.

[14] As highlighted in a Wall Street Journal article, people are concerned that, with mobile trading apps, retail investors will "look at their portfolio balances daily or multiple times a day and make decisions based on daily market volatility. It's the equation for poor performance."

Based on the above discussion, I propose two possible channels that can potentially explain the technology's effect on investor behavior: overconfidence and self-control problems. The literature demonstrates men are more likely than women to be overconfident ([55, 56]. If the effect is indeed operative through interacting with overconfidence, the technology should elicit a stronger reaction in males than females. Also, overconfidence levels may vary with investors' experience [57]. If an investor takes on a risky portfolio while blindly using the market return as the benchmark, she is likely to perceive herself as able to beat the market even if she is unskilled in investment. The deceptively successful experience can lead to overconfidence because the investor may misattribute the good performance to her competence rather than excessive risk-taking. If the effect is operative through amplifying overconfidence, the investors who have experienced deceptive success in their trades should be disproportionately affected.

To investigate whether the technology-behavior association is related to self-control problems, I exploit two cross-sectional variations. First, I resort to the psychological findings that self-regulation capacity increases with age [80, 58]. Second, the strength model of self-regulation contends that self-regulation acts draw from a common limited pool of resources. Experiments show that self-control becomes increasingly difficult subsequent to other tasks requiring self-regulation [81, 82]. Because work (as opposed to leisure) requires self-control, I hypothesize the technology's effect on an employee-investor becomes increasingly pronounced from Monday to Friday as self-regulation capacity is depleting.

**Prediction 1B:** The effect of "going mobile" on investor behavior is more pronounced among the investors particularly prone to overconfidence and among the investors more likely to have self-control problems.

Testing Prediction 1A sheds light on the effect of "going mobile" on investor behavior, and Prediction 1B helps connect this effect to some possible explanations. Finding a significant impact on trading behavior (regardless of the mechanism behind the impact) would lead to a further inquiry concerning whether the individual behavior change induces any meaningful effects at the mutual fund level.

The effect on mutual funds is theoretically possible. The trading activity fueled by "going mobile" may translate into higher investor flows if the investors' buys and sells are cross-correlated. The responsibility to accommodate the heightened flows may, in turn, drive up the liquidity cost for mutual funds and thus depress fund performance [52, 63].[15]

Still, whether the heightened trading volume from the app can translate into materially larger flows is an empirical question. If the incremental flows of each investor are completely independent, the buys and sells should balance out and be mostly executed between investors, therefore unlikely to have a net effect on fund operation. However, if the technology amplifies the inherent behavioral patterns that bias investors' trades in the same direction, larger flows can be created.

Accordingly, I examine the technology's effect on two trading patterns related to flow comovement. The first is the sensitivity of investor flows to past fund performance. Mutual fund investors tend to invest in top performing funds and disinvest from underperformers in the previous period [83]. "Going mobile" can potentially reinforce this pattern, especially in a short horizon. The ability to track fund performance in a real-time manner may focus investor attention on short-term returns and tempt them to trade on these "signals." The increase in trade-performance sensitivity at the individual level can convert to a heightened flow-performance sensitivity at the fund level. In other words, "going mobile" should subject well-performing funds to more inflow shocks while making "losers" more vulnerable to money outflows.

The second trading pattern is the investor sentiment. When market sentiment goes down (i.e., noise traders experience a negative belief shock), noise traders tend to sell assets to rational traders and temporarily depress returns [84, 85]. The retail investors of mutual funds are typically considered uninformed noise traders and the "compliers" with the market sentiment [86]. By providing more exposure to market information, the app is likely to make investors increasingly susceptible to market sentiment. Accordingly, I predict that app adopters increasingly synchronize their trades with the market sentiment after the shock.

[15] In the past decade, ETFs have been gaining market share versus active asset managing. If the mobile trading substantially raises the cost of liquidity-driven trades for active funds, it can make active funds less attractive and accelerate the shift from active management to passive.

**Prediction 2A:** After the shock, adopters' flows become increasingly sensitive to recent fund performance and more susceptible to market sentiment, using the pre-post change of nonadopters' flows as a benchmark.

Reinforcing the trading comovement, such as flow-performance sensitivity and investor sentiment, is likely to lead to larger and more extreme investor flows.

**Prediction 2B:** Adopters' flow volatility increases around the shock, relative to that of nonadopters.

The resulting shock in the liquidity demand from mutual fund investors can hurt fund performance. [52] documents a significant liquidity cost in the form of a negative association between abnormal fund returns and investor flows.[16] Theoretically, large investor flows can dampen fund performance through at least two mechanisms. First, a massive outflow (inflow) can force a fund into fire sales (purchase) at an unfavorable price, which lowers the fund's future returns. Second, because the investor flows per se are presumably noise trading, the liquidity-motivated trades a fund manager conducts in response to investor flows are largely uninformed. In the framework of [87], the equilibrium is attained only when uninformed liquidity traders sustain losses to informed traders. For the above reason, even without a price impact, trading in accordance with investors' liquidity demands is still likely to lower fund performance.

Furthermore, if the technology indeed depresses fund performance, the funds particularly exposed to the technology should sustain disproportionately large losses. To test this idea, I exploit a cross-fund variation in the intensity of treatment, namely, the percentage of AUM retail investors hold in a fund. Because retail investors (as opposed to institutional investors) are the primary users of the app, the adverse effect, if existing, should be especially pronounced among the "retail-oriented" funds.

**Prediction 3:** The fund performance deteriorates around the shock. The funds more exposed to the shock see a greater decline in performance, and the return dispersion can be explained by incremental fund flows from the trading app.

[16]The liquidity cost is material and can fully explain the negative return performance of an average mutual fund.

## 2.4 The Impact of "Going Mobile" on Retail Investor Behavior

My analysis starts by investigating how "going mobile" affects retail investor behavior, and in particular, investor attention and trading volume. My empirical strategy compares the investors who switched from the website to the mobile trading app (adopters) and the investors who kept using the website as the exclusive method of trading (nonadopters), before versus after the launch of the mobile trading app. To make the two groups as comparable as possible, I match the adopters and nonadopters based on various biographical attributes, such as age, education attainment, gender, and county of residence, as well as the trading account size and monthly trading volume prior to the shock. Limiting my analysis to investors who traded via the website before the shock enables me to pin down the incremental effect of "going mobile," holding constant the content of the service and fee rates.[17]

Panel C of Table 2.1 shows descriptive statistics for my sample of retail investors from July 2012 to August 2013 (prior to the shock). The sample results from nearest-neighbor matching and includes 13,354 app adopters and 11,482 nonadopters. I define the adopters as the retail investors who used the website before the shock and started using the mobile trading app after the shock, and nonadopters as those who kept using the website as the exclusive means of trading. The table shows monthly statistics at the individual level. According to column (1), the average age of the adopters was 36 at the time of the technology shock, and 61% of them were men. During the pre-shock period, an average app adopter logged into the website 7.6 times per month. A typical investor who used the app traded 2,051 shares per month, out of the 40,000 shares he/she held before the shock. In comparison with the whole sample exhibited in Panel B, the adopters in the matched sample were more likely to be young men and tended to visit the direct sales platform more frequently than an average investor even before the shock, echoing the finding in Table 2.2. Column (3) presents

[17]My analysis focuses on the retail investors that traded through the website before the shock for at least three reasons. (1) This method allows us to control for the transaction fee, given that the fee structure for mobile trades is the same as the trades on the web page. (2) To justify the login frequency as a valid proxy for investor attention, the investors have to rely largely on the online trading platform to track investments. Including investors from other channels would introduce noise into this measurement. (3) Contrasting the mobile app users and the website users helps pin down the incremental effect of "going mobile" itself and fits my focus on understanding the shift on the devices where financial services are provided.

the gap between adopters and nonadopters on various predetermined attributes and column (4) presents the p-value of the estimates of the differences. As expected, after matching the adopters and nonadopters, the two groups exhibit no significant differences in any of the dimensions.

To examine the technology's effect on investor behavior, I run regressions at the individual-month level. The resulting DID specification is

$$\begin{aligned} Outcome_{i,t} = \alpha_i + \alpha_t + \beta_1 D_t^{post} \times D_i^{adopter} + D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) \\ + D_t^{post} \times (\beta_2 LoginFreq_i + \beta_3 Age_i + \beta_4 Male_i + \beta_5 Education_i + \beta_6 AccountSize_i) \\ + \varepsilon_{i,t}. \end{aligned} \tag{2.3}$$

Two outcomes of interest emerge: $Login_{i,t}$ and $Volumn_{i,t}$. $Login_{i,t}$ is defined as the total logins to the direct sales platform for investor $i$ during month $t$. Following [79] and [68], I use the times of logins to the direct sales platform as a proxy for investor attention. $Volumn_{i,t}$ is defined as the number of mutual fund shares investor $i$ trades during month $t$.

$D_t^{post}$ is a dummy variable equal to 1 if month $t$ is post the shock, and 0 otherwise. The key coefficient $\beta_1$ measures how much more attention (trading volume) the adopters pay (create) relative to the ex-ante similar investors who do not use the app, after the new app is put in place relative to the pre-shock period. $\beta_1$ is expected to be significantly positive according to Prediction 1A. To remove the effect of the individual-level time-invariant heterogeneity and macro-level factors, I include individual fixed effects $\alpha_i$ and month fixed effects $\alpha_1$ in all tests based on Regression 2.3.

A potential concern is that the dispersion in investor behavior might be driven by other observable factors correlated with app adoption rather than the technology's impact. For example, suppose males are more likely than females to use the app, and suppose the technology shock coincides with an event that makes men raise their financial attention more than women. In this case, a gender gap may contribute to the observed divergence in attention between app users and

non-users, which may bias the estimate of $\beta_1$ upward. To alleviate this concern, Regression 2.3 incorporates the interaction terms of $D_t^{post}$ and a set of control variables: $LoginFreq_i$ is the average monthly login times for investor $i$ during the pre-period; $Age_i$ is investor $i$'s age in 2013; $Male_i$ takes a value of 1 for male investor and 0 for female; $Education_i$ is an integer between 0 to 5, increasing with the level of the highest degree investor $i$ obtains; and $AccountSize_i$ is defined as the average number of shares held in investor $i$'s account over all the month ends prior to the upgrade.

Another concern is that adopters and nonadopters may cluster in different geographic areas, and the locality with more app adopters may elicit different trends than others. To address this concern, I include the interaction terms of $D_t^{post}$ and the full sets of county fixed effects ($D_i^k$) to control for the pre-post change at the county level. $D_i^k$ equals 1 if investor $i$ resides in county $k$, and zero otherwise.

A third concern is the endogeneity of the app adoption. App adopters and nonadopters might have a fundamental discrepancy on unobservable facets that drives the dispersion around the shock, which the addition of control variables cannot solve. I address this concern using a spatial-discontinuity design, exploring the discontinuity in the exposure to the technology shock at state borders. I postpone the elaboration of this identification strategy until section 2.4.4.

### 2.4.1 Graphical Evidence

The underlying assumption for the DID analysis in equation 2.3 is that, in the absence of the app upgrade, the frequency of using the direct sales platform and the trading intensity would have evolved in parallel for the adopters and nonadopters. To test this assumption, I show the dynamics of the difference in investor attention (trading volume) between adopters and nonadopters by estimating the following regression:

$$Outcome_{i,t} = \alpha_i + \alpha_t + \sum_{\tau=-5}^{7} \beta_\tau D_t^\tau \times D_i^{adopter} + \varepsilon_{i,t}, \tag{2.4}$$

where $D_t^\tau$ equals 1 if month $t$ is the $\tau^{th}$ month following the shock, and 0 otherwise. In equation 2.4, the coefficient of interest $\beta_\tau$ measures the investor attention level or trading volume of the app adopters relative to their nonadopter counterparts during the $\tau^{th}$ ($-\tau^{th}$) month after (before) the shock. Consistent with the previous specifications, I incorporate individual fixed effects $\alpha_i$ and month fixed effects $\alpha_t$ to control for the effect of individual-level heterogeneity and macro-level factors.

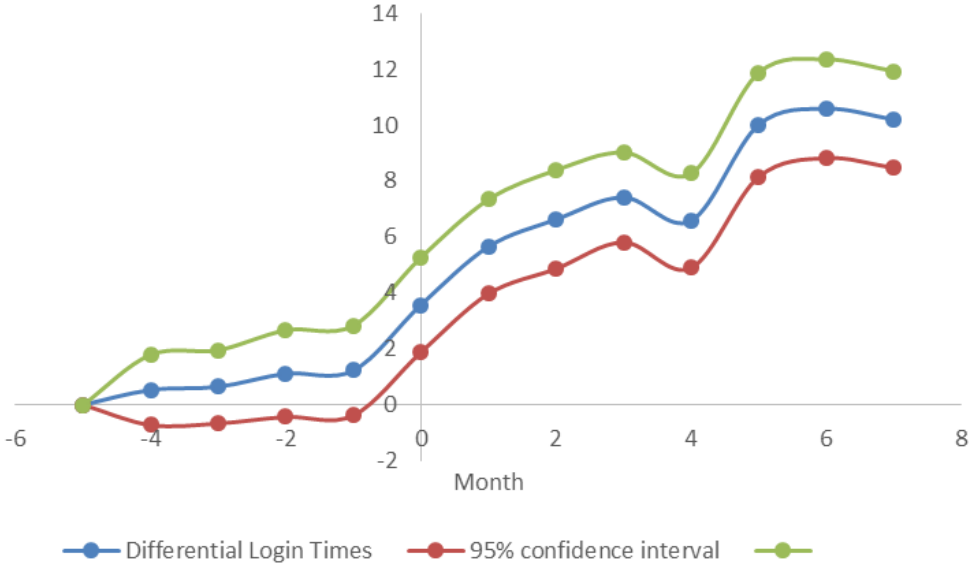
The estimation of equation 2.4 is presented in Figure 2.2, where each data point corresponds to an estimate of $\beta_\tau$ ($-5 \leq \tau \leq 7$). Panels A and B present the attention gap and trading-volume gap, respectively, comparing the adopters and the matched nonadopters for each month around the technology shock. No significant differences emerge between adopters and nonadopters on either outcome before the new app is put in place, which supports the parallel assumption. The graphs also foreshadow the first set of main results: In each of the seven months after September 2013, adopters exhibit a higher login frequency and a higher trading volume than nonadopters, and the differences are mostly significant at the 5% confidence level.

### 2.4.2 Empirical Results

Panel A of Table 2.3 reports the effect of adopting the mobile trading app on *Login*. Consistent with the graphical evidence, the coefficient on the interaction term $D^{post} \times D^{adopter}$ is significantly positive at the 1% confidence level in all the specifications. The coefficient estimate of the baseline specification in column (1) shows that the availability of the mobile trading app prompts app adopters to increase logins to the direct sales platform by 11 more times per month than the nonadopters who have similar biographic attributes and who exhibit similar trading and login patterns before the shock. The coefficient corresponds to the pre-post change in attention from the adopters relative to the same change from the nonadopters. To provide a sense of scale, the introduction of the new mobile trading app increases app adopters' attention by 146% from the pre-shock level relative to comparable nonadopters.

As shown in columns (2) and (3), the result is robust to incorporating the set of control variables

Panel A: Differential Trends of Financial Attention: App Adopters and Nonadopters




Panel B: Differential Trends of Trading Volume: App Adopters and Nonadopters

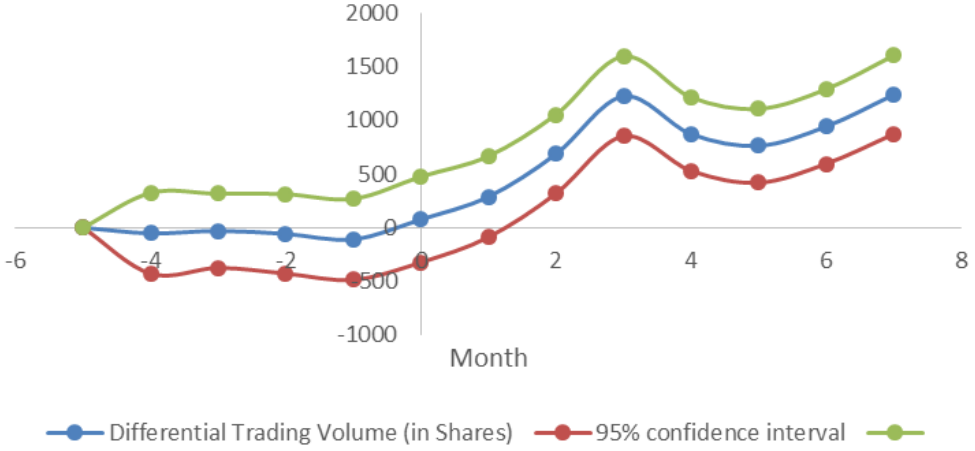



Figure 2.2: **Differential Trends of Investor Behavior: Adopters and Nonadopters.** This figure presents the behavioral difference between adopters and nonadopters in each month around the shock. Each data point corresponds to an estimate of $\beta_\tau$ ($-5 \leq \tau \leq 7$) in equation 2.4. Panel A and Panel B report the estimation on investor attention and trading volume, respectively. The horizontal axis represents the month relative to the app upgrade, and month 0 is the event month. The blue line represents the monthly logins (trading volume) of app adopters in excess of nonadopters. The green and red lines represent the 95% confidence intervals of the corresponding estimates.

specified in equation 2.3. Also, the results do not seem to be driven by the differential trends across localities, because column (4) shows that including the interaction terms of county fixed effects and $D^{post}$ does not diminish the loading of the interaction term $D^{post} \times D^{adopter}$. In addition, I investigate whether the results are robust with respect to the measurement of technology adoption by using two alternative definitions of adoption: In column (6), $D^{adopter}$ equals 1 for the investors who start using the app within 12 months of the shock, and 0 otherwise. In column (7), $D^{adopter}$ is set to 1 if and only if the investors start using the app within six months of the shock. The point estimates in columns (6) and (7) show the effect is unchanged under the alternative definitions of adoption.

Panel B of Table 2.3 demonstrates the impact of introducing the new mobile trading app on the share trading volume. All the columns mirror those in Panel A, whereas $Volume_{i,t}$ is used as the outcome variable. Similarly, the coefficient on the interaction term $D^{post} \times D^{adopter}$ is significantly positive at the 1% confidence level in all specifications. The baseline regression in column (1) shows that after the shock, an average app adopter's trading volumn increases by 750 shares per month, relative to similar investors who keep using the website exclusively. Likewise, this result is not driven by the effect of other observable characteristics based on two pieces of evidence: First, the estimates hold using the matched sample. Second, as shown in column (3), incorporating the full sets of control variables does not reduce the magnitude of the key coefficient. Also, the estimates are robust to including $D^{post} \times County$ fixed effects (column (4)) and using alternative measures of app adoption (columns (6) and (7)).

### 2.4.3 Behavioral Explanations of the Technology's Effect

Section 2.4.2 shows that using the mobile trading app is associated with higher investor attention and more trading activity. A missing part of the picture is the mechanism behind this effect. In this subsection, I try to shed light on the underlying channels by connecting the findings to the behavioral literature regarding overconfidence and self-regulation. Note that the analysis below does not rule out the presence of other potential mechanisms but only provides suggestive evidence

consistent with some possible explanations.

The first explanation I test is that the trading technology may boost investors' overconfidence. Accessing abundant information through the app may create an illusion of knowledge, which may, in turn, foster investors' overconfidence and make them increasingly prone to excessive trading [50, 51].

To test the channel, I exploit two cross-sectional variations in the tendency to be overconfident. First, prior literature shows that men tend to be more overconfident than women [55, 56]. If the technology's impact indeed operates through boosting overconfidence, the same amount of increase in information exposure should induce a greater change in the behavior of men, who ex-ante are more prone to overconfidence and thus likely to exhibit more overconfidence if the condition allows. Accordingly, I estimate the following triple-differences specification to investigate the differential impact of the technology on men and women:

$$\begin{aligned} Outcome_{i,t} = \alpha_1 &+ \beta_1 D_t^{post} \times D_i^{adopter} \times Male_i + \beta_2 D_t^{post} \times D_i^{adopter} + \beta_3 D_t^{post} \times Male_i \\ &+ D_t^{post} \times (\beta_2 LoginFreq_i + \beta_3 Age_i + \beta_5 Education_i + \beta_6 AccountSize_i) \\ &+ D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}. \end{aligned} \tag{2.5}$$

Likewise, the outcome variable is either $Login_{i,t}$ or $Volume_{i,t}$. The coefficient of interest $\beta_1$ represents how much more the technology shock affects male adopters than female adopters with respect to their trading volume (attention), using nonadopters with the same gender and similar predetermined attributes as a benchmark. A significantly positive estimate of $\beta_2$ would endorse the hypothesis that the mobile trading app tempts investors to trade more (pay more attention) by amplifying their overconfidence. The definitions of all the variables and subscripts are consistent with specification 2.3.

One may be concerned that the interpretation of the male-female comparison is contaminated by the generic difference in the response of men and women to new technology. To further verify the overconfidence story, I exploit the variation in investors' past experience, which can in turn

affect the degree of overconfidence [57].

Suppose an investor holds a risky portfolio but keeps benchmarking against the market return. She is more likely to beat the market in terms of the absolute return and therefore claim a victory even if she has no investment skills. The deceptively successful experience can potentially boost the investor's overconfidence. She might misattribute the good performance to her competence instead of risk-taking and thus become unjustifiably confident in her investment skills and the accuracy of her judgment. In other words, if the technology's impact does operate through boosting overconfidence, the effect should be particularly pronounced for the investors who outperformed the market at face value but failed to do so using risk-adjusted measures before the shock. Accordingly, I estimate the following specification:

$$\begin{aligned} Outcome_{i,t} = {} & \alpha_1 + \beta_1 D_t^{post} \times D_i^{adopter} \times Decile_i^{DeceptiveGain} \\ & + \beta_2 D_t^{post} \times D_i^{adopter} + \beta_3 D_t^{post} \times Decile_i^{DeceptiveGain} \\ & + D_t^{post} \times (\beta_2 LoginFreq_i + \beta_3 Age_i + \beta_5 Education_i + \beta_6 AccountSize_i + \beta_7 Male_i) \\ & + D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}. \end{aligned} \tag{2.6}$$

$Decile_i^{DeceptiveGain}$ is a measure for the deceptively successful trades investor $i$ performs prior to the launch of the app, constructed as follows. First, I look into each buy transaction investor $i$ conducts before the shock. I code a buy as a deceptive gain if the transacted mutual fund delivers an above-market return in the six months from the transaction but has a negative alpha estimated using a Fama-French three-factor model during the same period. Second, I construct $DeceptiveGain_i$, the number of deceptive gains as a percentage of the total number of buy transactions performed by investor $i$ prior to the shock, weighted by the value of each buy. $Decile_i^{DeceptiveGain}$ in equation 2.6 is defined as the decile of $DeceptiveGain_i$ within the matched sample.

The coefficient of interest $\beta_1$ represents how much the effect of "going mobile" increases with the past deceptively successful experience. A significantly positive estimate of $\beta_1$ would be con-

sistent with the hypothesis that the technology's effect is related to the amplification of investors' overconfidence.

The second possible channel concerns self-regulation. The mobile trading app may trigger investors' self-control problems, making them addicted to using the app and conducting excessive trades. I exploit two cross-sectional variations in the self-regulation capacity. Literature shows the growing age mitigates self-control problems [58, 80]. Accordingly, I test the self-control mechanism by running a triple-differences regression, comparing younger and older people on the extent to which they are affected by the technology:

$$\begin{aligned} Outcome_{i,t} = \alpha_1 &+ \beta_1 D_t^{post} \times D_i^{adopter} \times D_i^{senior} + \beta_2 D_t^{post} \times D_i^{adopter} + \beta_3 D_t^{post} \times D_i^{senior} \\ &+ D_t^{post} \times (\beta_2 LoginFreq_i + \beta_3 Male_i + \beta_5 Education_i + \beta_6 AccountSize_i) \\ &+ D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}. \end{aligned} \tag{2.7}$$

The outcome variable is either $Login_{i,t}$ or $Volume_{i,t}$. $D^{senior}$ denotes a dummy variable equal to 1 for an investor in the top quartile by her age in 2013, and 0 for an investor in the bottom quartile. The coefficient of interest $\beta_1$ captures how much the technology's effect on adopters in excess of nonadopters varies with age. $\beta_1$ should be significantly negative if the lack of self-regulation capacity is behind the technology's effect on trading behavior.

The second variation stems from the finding that self-regulation capacity can deplete over time. Experiments show that self-control becomes increasingly difficult subsequent to other tasks requiring self-regulation [81, 82], suggesting that self-regulation acts draw from a common limited pool of resources. Considering work requires more self-control than leisure almost by definition, the self-regulation capacity is likely to deplete from Monday to Friday for an employee with a regular work schedule. Therefore, resisting the temptation to use the app on Friday is presumably harder

than resisting using it earlier in a week. Accordingly, I estimate the following regression:

$$\begin{aligned} Outcome_{i,t} = \alpha_1 &+ \beta_1 D_t^{post} \times D_i^{adopter} \times DayofWeek_t + \beta_2 D_t^{post} \times D_i^{adopter} \\ &+ \beta_3 D_t^{post} \times DayofWeek_t + \beta_4 D_i^{adopter} \times DayofWeek_t \\ &+ \beta_5 DayofWeek_t + \beta_6 D_t^{post} + \varepsilon_{i,t}. \end{aligned} \tag{2.8}$$

Unlike the previous specifications, I estimate the above model at the individual-day level based on the subsample only including weekdays. $DayofWeek_t$ denotes the day of the week for day $t$. The coefficient of interest $\beta_1$ captures how much the technology's effect on adopters in excess of nonadopters varies with the day of the week. $\beta_1$ is predicted to be significantly positive if the technology's effect is related to the self-control problems.

Panel A of Table 2.4 reports the coefficient estimates corresponding to equation 2.5 and its variants. Columns (1)-(4) focus on the outcome *Login* and show how much the availability of the mobile trading app prompts men's attention more than women's. In all four columns, the coefficients of interest on the triple interaction term are significantly positive, and the magnitude ranges from 3.162 - 3.504 depending on the specifications. For example, the coefficient estimate in column (3) shows that in response to the technology shock, male adopters increase monthly logins by 3.5 more times than female adopters, using the ex-ante similar male and female nonadopters as "control" groups. The coefficients on the interaction term $D^{post} \times D^{adopter}$ can be interpreted as the DID effect of the mobile app on female adopters. The first two rows jointly suggest the effect on men's attention is 39% higher than the effect on women's.

Columns (5) to (8) report how much more the technology shock affects men than women with respect to their trading volume, using the nonadopters with similar attributes and comparable ex-ante trading patterns as a benchmark. Consistent with Prediction 1B, male adopters, who are presumably more prone to overconfidence, increase their monthly trading volume by 443-483 shares relative to female adopters in response to the technology shock, relative to the matched "control" group not adopting the app. Comparing the key coefficients on the triple interaction term with the coefficients on $D^{post} \times D^{adopter}$, I estimate the impact on trading volume to be nearly twice as

strong for men as for women, and the differential effect is statistically significant in all the specifications. Given that men are found more prone to overconfidence than women, the results in Panel A are overall consistent with the hypothesis that the mobile trading infrastructure affects investor behavior by reinforcing investors' overconfidence.

Panel B of Table 2.4 reports how much the app's effect on investor behavior varies with the deceptively successful experience that can boost overconfidence. The table corresponds to the estimation of equation 2.6 and its variants. The outcome variable is *Login* for columns (1)-(4) and *Volume* for columns (5)-(8). The coefficient on the triple-interaction term loads positively regardless of the specifications, suggesting that aggravated overconfidence is likely to play a role in driving the effect of "going mobile" on investor behavior.

Next, I test whether the amplified self-control problems partly explain the technology's impact on investor behavior. Panel A of Table 2.5 reports the coefficient estimates corresponding to equation 2.7 and its variants. The outcome variable is *Login* for columns (1)-(2) and *Volume* for columns (3)-(4). $D^{senior}$ denotes a dummy variable equal to 1 for investors in the top quartile by age in 2013, and zero for investors in the bottom quartile. All results are based on the matched sample in which adopters and nonadopters are similar on various attributes and predetermined account characteristics. Regardless of the inclusion of control variables, the coefficients on the triple-interaction term remain significantly negative, which is consistent with the hypothesis that the mobile trading app triggers self-control problems and therefore makes it hard to resist using the app and trading.

Panel B of Table 2.5 presents the coefficient estimates corresponding to equation 2.8. Columns (1)-(3) report the effect on the daily logins, and columns (4)-(6) on the daily trading volume. $DayofWeek$ denotes the day of the week for a given day. The coefficient on the triple interaction term captures how much the technology's effect increases with the day of the week, and in particular, the degree to which the effect is stronger on Friday versus Monday. Columns (1) and (4) are based on the entire sample of the matched investors, whereas columns (2) and (5) (columns (3) and (6) ) are based on the subsample not exceeding (exceeding) the retirement age. The results sug-

gest that as the self-regulation capacity depletes over the weekdays, the effect of the app becomes increasingly pronounced. Notably, the effect only manifests itself among the investors below the retirement age and thus likely to have a job, and not among the retired investors. This finding is consistent with the hypothesis that the technology affects investor behavior in part by triggering self-control problems.

### 2.4.4 Causal Inferences: Technology and Investor Behavior

So far, the analysis shows that "going mobile" is likely to exacerbate investors' overconfidence and self-control problems, which in turn leads to more investor attention and higher trading volume. A caveat to the above research design is that the investors can endogenously choose whether to adopt the app. If the unobserved variables that determine the self-selection into the technology adoption are associated with the change in the outcomes of interest around the shock, the above analysis may yield biased estimates of the technology's impact. Although using the matched sample and control variables can alleviate the concerns about observed confounders, the biases arising from the presence of an unobserved confounder can only be corrected using more advanced identification strategies.

To address the endogeneity issue, I explore the spatial discontinuity of the exposure to the shock. In particular, I use the discontinuity in the access to smartphones at the province border to draw causal inferences. Given that the app is designed for use on smartphones, a key premise of adopting the app is to possess a smartphone. The access to smartphones varies by province, in part due to the geographic segmentation of mobile networks in China. The network quality and coverage largely rest on the province's carriers and province-specific policies. Accordingly, I use the predetermined smartphone penetration rate of a province as a proxy for the province-specific policies regarding mobile networks, and as a source of the cross-sectional variation on the app adoption of the investors residing in the province prior to the shock. To exploit the discontinuity of the technology exposure at the state border, I restrict my analysis to the investors who reside in the pairs of neighboring prefectures separated by the province border between two provinces with

different levels of exposure to smartphones.

The spatial-discontinuity strategy relies on the assumption that the investors living in a small region around the province border are similar except for the exposure to the technology shock. In other words, covariates should exhibit no significant discontinuity at the border. The investors in a local area around the province border presumably face the same investment opportunity as well as similar economic conditions, which makes the assumption likely to hold. To test the above assumption, I compare the predetermined demographic attributes as well as investor characteristics in each pair of prefectures. The analysis below shows no significant gap between the high-exposure and low-exposure prefectures, in line with the assumption. Also, the strategy requires that the province of residence cannot be perfectly manipulated in response to the shock. Due to the household registration system in China, changing the province of residence is far from costless, which theoretically excludes the perfect manipulation of the province of residence around the shock.

In this subsection, I first test the continuity of the covariates at the province border by comparing the paired prefectures and the investors residing in them. Then I conduct the first-stage analysis to test whether app adoption jumps at the province border between the high- and low-exposure prefectures. Lastly, I detail the reduced-form and second-stage analysis that estimate the causal effect of "going mobile" on investor behavior.

**The comparison of matched prefectures**

To pinpoint the pairs of neighboring prefectures separated by a province border, I obtain the map information from the China Geo-Explorer platform provided by the University of Michigan. I manually construct 262 pairs of prefectures on the opposite sides of a province border and sharing a part of the borderline. Among them, 240 pairs are populated by mutual fund investors present in my sample. Within each pair, I code the prefecture with a relatively high smartphone penetration rate as a High-Exposure prefecture and the one with a relatively low penetration rate as a Low-Exposure prefecture. Table 2.6 displays the descriptive statistics of the High-Exposure group and the Low-Exposure group on a variety of dimensions.

Panel A of Table 2.6 exhibits the demographic attributes of the entire population for the matched High-Exposure and Low-Exposure prefectures. All variables are constructed based on the 2010 Population Census of China. Column (1) (column (2)) displays the mean of the corresponding variable for High- (Low-) Exposure prefectures, and the columns (3) and (4) show the differences between the first two columns and the standard deviations of the estimated gaps. The two groups of prefectures are extremely close in gender and age distribution according to row 2 and rows 4-6. No significant disparity is present with respect to other socioeconomic indicators such as GDP per capita, mortality rate, average years of education, the percentage who are college educated, and the housing condition (captured by the average housing area per capita).

Panel B presents the statistics of mutual fund investors residing in the matched prefectures. The first and the second row report the average monthly trading volume and the average login frequency in the pre-shock period, and the third and the fifth report the trading account size and the age at the technology shock. Along all dimensions, no significant gaps exist between the High-Exposure and Low-Exposure prefectures. Although the analysis cannot exhaust all characteristics, the similarity between the matched prefectures on various observable attributes shows that the investors and residents in the paired prefectures are highly comparable, which is consistent with the identification hypothesis that covariates change continuously around the province border.

In essence, the identification comes from the comparison of two groups of investors who are similar in all the aspects except for having different exposure to the technology shock, in terms of their behavior change before versus after the shock. Notably, using the technology shock further relaxes the identification assumption. Even if the discontinuity in covariates exists at the threshold, the identification can still be valid as long as the discrepancy between the paired prefectures does not happen to drive a dispersion in investor behavior at the time of the technology shock.

**First-stage analysis**

To implement the strategy, I start by examining whether the likelihood of receiving the treatment (i.e., adopting the app) jumps at the province border when entering a high-exposure prefecture

from the neighboring low-exposure prefecture. In particular, I run a regression of the following form:

$$D_t^{post} \times D_i^{adopter} = \alpha_i + \beta_1 D_t^{post} \times PRate_i + D_t^{post}(\gamma \cdot X_i) + \varepsilon_{i,t}. \tag{2.9}$$

$PRate_i$ represents the smartphone penetration rate of the province where investor $i$ resides in 2011, two years before the launch of the new mobile trading app. $X_i$ denotes a $1 \times n$ vector comprising a set of control variables $[x_1, x_2, \ldots, x_n]$, and $\gamma$ represents a $1 \times n$ vector containing the corresponding coefficients $[\gamma_1, \gamma_2, \ldots, \gamma_n]$. I plan to incorporate $LoginFreq_i$, $Age_i$, $Male_i$, $Education_i$, and $AccountSize_i$ as control variables. I estimate specification 2.9 based on the subsample containing retail investors in the pairs of adjacent prefectures on both sides of the province borders. The coefficient of interest $\beta_1$ captures how much more likely an investor would be to adopt the mobile trading app if the penetration rate in his/her locality rose by 100%. A significantly positive estimate of $\beta_1$ would be expected to justify the use of the discontinuity setting.

Column (1) in Table 2.7 reports the estimation of the first-stage regression. The key coefficient on $D^{post} \times PRate$ is significantly positive at the 1% confidence level in line with the prediction. To provide a sense of scale, a 50% increase in the smartphone penetration rate translates into a 25% increase in the probability of adopting the app after the technology shock.[18] Also, the design passes the Stock-Yogo critical value by a large margin.

**Reduced-form analysis: Investor attention and trading intensity**

In the reduced-form analysis, I compare the behavior change in the investors residing in the adjacent prefectures on the opposite sides of the province borders before versus after the technology shock. In light of the similarity between the high-exposure investors and low-exposure investors in each pair, the test essentially contrasts the investors who are highly comparable to each other in all aspects except the likelihood of adopting the mobile trading app. Focusing on the pair of

[18] One standard deviation increase in the penetration rate translates to an 8% increase in the probability of app adoption.

prefectures around the province borders yields a subsample including 11,752 retail investors. On this subsample, I run the following DID regression at the individual-month level:

$$Outcome_{i,t} = \alpha_i + \beta_1 D_t^{post} \times PRate_i + D_t^{post}(\gamma \cdot X_i) + D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}. \tag{2.10}$$

The outcome variable is either $Login_{i,t}$ or $Volume_{i,t}$. I incorporate the interaction terms of $D_t^{post}$ and the full set of prefecture pair fixed effects ($D_i^k$) to control for the pre-post change in the outcome for each pair of prefectures. All the other variables and subscripts are defined as above. The key coefficient, $\beta_1$, measures how much more attention (trading volume) the investors with high exposure pay (create) relative to the ex-ante similar investors with low exposure, after the shock relative to the pre-period. $\beta_1$ is predicted to be significantly positive. To remove the effect of the individual-level time-invariant heterogeneity, I incorporate individual fixed effects $\alpha_i$ in all tests based on specification 2.10. I control for the impact of various individual attributes by including the interaction terms of $D_t^{post}$ and a set of time-invariant investor characteristics as defined in equation 2.9.

Columns (2)-(5) in Table 2.7 report the estimation of the second-stage and reduced-form regressions. Columns (2)-(3) focus on the effect of mobile trading technology on the attention level, and columns (4)-(5) focus on share trading volume. Columns (3) and (5) correspond to equation 2.10, whereas columns (2) and (4) report the second-stage analysis regressing the outcome variables on the predicted value of $D^{post} \times D^{adopt}$ from the first-stage analysis.

Consistent with the hypothesis, the key coefficients on the interaction term are significantly positive at the 1% confidence level in all specifications. To provide a sense of scale, the point estimates of the coefficient on the term *Insturmented* $D^{post} \times D^{adopt}$ suggest that adopting the mobile trading app for exogenous reasons induces an investor to check his/her portfolio nine more times per month and heightens his/her trading volume by 1,748 shares per month.

## 2.5 "Going Mobile" and Liquidity Costs

The previous section shows that "going mobile" is likely to exacerbate investor behavior biases (e.g., investors' overconfidence and self-control problems), and therefore leads to a substantial increase in investor attention and trading volume. In this section, I take a step forward and investigate how the resulting behavior change at the investor level, in turn, affects the liquidity costs faced by mutual funds.

To do so, I start by examining whether the heated trading activity at the individual level converts into larger investor flows at the fund level. I test two specific patterns of trading comovement that can be reinforced due to the use of the mobile trading app and show direct evidence of how "going mobile" affects the magnitude of investor flows. In particular, my triple-differences analysis shows that "going mobile" makes investors more sensitive to past fund performance and considerably boosts investors' susceptibility to market sentiment, which links directly to the heightened investor flows among app adopters relative to nonadopters following the shock. The results are confirmed using a spatial-discontinuity analysis similar to section 2.4.4.

Heightened investor flows may drive up liquidity costs for mutual funds. In the second half of this section, I further test whether the investor flows arising from "going mobile" are large enough to lower mutual fund performance. I employ a framework similar to the DID method to understand the change in fund performance around the shock and whether it is related to heightened investor flows. Then I explore the cross-fund variation in the intensity of treatment to identify the causal effect of the technology on fund performance. My analysis shows that the funds with higher exposure to the shock experience a greater downturn after the shock, and all of the differential impact is explained by the incremental investor flows on the mobile trading app.

### 2.5.1 Technology and Investor Flows

The trading activity fueled by "going mobile" can translate into higher investor flows if the investors' buys and sells are cross-correlated. In this light, a natural starting point to test the

effect on investor flows is to investigate whether the mobile trading technology spurs investor comovement in their trading decisions.

Accordingly, I examine the technology's effect on two trading patterns related to flow comovement. The first is the sensitivity of investor flows to past fund performance. Mutual fund investors tend to invest in top performing funds and disinvest from underperformers in the previous period [83]. "Going mobile" allows investors to check fund performance in a real-time manner, which can increase the salience of the performance information and spur more response to the fund performance. This effect is likely to be particularly pronounced in a short horizon, given that the mobile technology supposedly reduces the cost of acquiring real-time information more than other types of information.

Suppose $R_t$ denotes the fund performance in period $t$ and $f_t$ the investor flow. The flow-performance sensitivity is captured by the coefficient on $R_{t-1}$ in the regression of $f_t$ against $R_{t-1}$. In the main tests, I examine how investor flow in week $t$ responds to the fund performance in week *t-1*, using a horizon shorter than what has been examined in other studies (monthly or quarterly).[19] I partition the sample into adopters and nonadopters and examine their respective change in flow-performance sensitivity before and after the app upgrade. The resulting DID specification at the individual-fund level is

$$NetFlow_{i,j,t} = \alpha_{i,j} + \beta_1 D_t^{post} \times Ret_{j,t-1} + \beta_2 D_t^{post} + \beta_3 Ret_{j,t-1} + \varepsilon_{j,t}. \tag{2.11}$$

Specification 2.11 is estimated for adopters and nonadopters separately. The outcome variable $NetFlow_{i,j,t}$ is defined as the shares of fund $j$ purchased by investor $i$ during week $t$ in excess of the number of redeemed shares from fund $j$ over the same period, as a percentage of the shares of fund $j$ investor $i$ holds at the end of the prior period. $Ret_{j,t-1}$ represents fund $j$'s performance

[19]This choice of the horizon fits the purpose of this work for the following reason. By allowing investors to track past performance and market update in a real time manner, the mobile trading technology most likely influences investors' reactions to the very recent trends, given the idea that the latest information is most likely to be missed by the webpage users but received by the app traders. Based on this logic, I choose to test the baseline specification at the weekly frequency.

over week *t-1*. My analysis deploys three different measures of weekly fund performance. $Ret^{pri}_{j,t-1}$ denotes fund *j*'s gross return in excess of the average gross return of all the funds belonging to the same primary category. $Ret^{sec}_{j,t-1}$ is defined as fund *j*'s gross return in excess of the average gross return of all funds in the same secondary category. The third performance measure, $Ret^{style}_{j,t-1}$, represents fund *j*'s gross return in excess of the average gross return of all funds belonging to the same category by investment style.

The coefficient of interest, $\beta_1$, measures how much the technology raises investor flow-performance sensitivity relative to the pre-period. In accordance with Prediction 2A, I conjecture $\beta_1$ to be significantly positive for the app adopters and insignificant for the nonadopters. The definitions of all the other variables and subscripts are consistent with the specifications in section 2.4. To control for the effect of the individual-fund-level time-invariant heterogeneity, I incorporate the individual-by-fund fixed effect, $\alpha_{i,j}$. To improve the comparability of the nonadopters and the adopters, I base the analysis on the matched sample following the method specified in section 2.4.

Next, I conduct a triple-differences analysis to confirm whether the technology effect on adopters' flow-performance sensitivity significantly exceeds the "placebo" effect on that of nonadopters. In particular, I estimate the following specification at the individual-fund level:

$$\begin{aligned} NetFlow_{i,j,t} = \alpha_{i,j} + \beta_1 D^{post}_t \times D^{adopter}_i \times Ret_{j,t-1} + \beta_2 D^{post}_t \times Ret_{j,t-1} + \beta_3 D^{post}_t \times D^{adopter}_i \\ + \beta_4 D^{adopter}_i \times Ret_{j,t-1} + \beta_5 D^{post}_t + \beta_6 Ret_{j,t-1} + \varepsilon_{i,j,t}, \end{aligned} \tag{2.12}$$

where $D^{adopter}_i$ denotes a dummy variable equal to 1 if investor *i* adopts the mobile trading app, and zero otherwise. The coefficient of interest, $\beta_1$, captures how much app adopters' flow-performance sensitivity increases pre versus post shock, using the same change in the matched nonadopters as a benchmark. A significantly positive estimate of $\beta_1$ would support the hypothesis that the trading technology makes investors more sensitive to the short-term trends of fund returns.

Next, I move on to the second flow comovement, using a similar method to test whether the technology makes investors increasingly susceptible to market sentiment. Likewise, I examine the

pre-post change in the sensitivity of investor flows with respect to the latest market sentiment for app adopters and nonadopters respectively. In particular, I run the following regression:

$$NetFlow_{i,j,t} = \alpha_{i,j} + \beta_1 D_t^{post} \times Sentiment_{t-1} + \beta_2 D_t^{post} + \beta_3 Sentiment_{t-1} + \varepsilon_{j,t}. \tag{2.13}$$

$Sentiment_{t-1}$ denotes the investor sentiment in China's stock market in week *t-1*. I borrow the investor-sentiment measure from [88], who adapt the index from [89] to the Chinese market using a similar principle component analysis. $\beta_1$ represents how much more sensitive investors become with respect to the overall market sentiment after the technology shock relative to the pre-period. According to Prediction 2A, $\beta_1$ is expected to load positively for app adopters and to be insignificant for nonadopters.

To investigate the differential effect on the two groups, I run a triple-differences regression of the following form:

$$\begin{aligned} NetFlow_{i,j,t} = {} & \alpha_{i,j} + \beta_1 D_t^{post} \times D_i^{adopter} \times Sentiment_{t-1} + \beta_2 D_t^{post} \times Sentiment_{t-1} \\ & + \beta_3 D_t^{post} \times D_i^{adopter} + \beta_4 D_i^{adopter} \times Sentiment_{t-1} + \beta_5 D_t^{post} + \beta_6 Sentiment_{t-1} \\ & + \varepsilon_{i,j,t}. \end{aligned} \tag{2.14}$$

The coefficient of interest $\beta_1$ measures how much the sensitivity to market sentiment increases among app adopters relative to nonadopters around the shock. Likewise, $\beta_1$ is predicted to be significantly positive in accordance with Prediction 2A.

**Empirical results**

Panel A of Table 2.8 presents the statistics of investor flow volatility before and after the app upgrade for adopters and nonadopters. The outcome variables in columns (1)-(2), (3)-(4), and (5)-(6) are the volatility of weekly net flow, inflow, and outflow as a percentage of the ending balance in the prior week, respectively. The first three rows show the unconditional statistics based on the

whole sample, and the next three build on the subsample of adopters and nonadopters matched on a variety of predetermined attributes. Regardless of the sample choices, the volatility of the app adopters' net flow more than doubles after the shock. The magnitude of the volatility increase for app adopters far exceeds the increase for nonadopters, suggesting the heightened trading volume from "going mobile" translates into higher volatility of investor flows at the fund level.

Panel B estimates the differential impact of the technology on adopters' and nonadopters' flow volatility. Echoing the findings in Panel A, the interaction term $D^{post} \times D^{adopter}$ loads positively in all specifications. This result suggests the mobile trading technology makes app users' flow more volatile relative to the flow for nonadopters.

Panel A of Table 2.9 reports the technology's effect on flow-performance sensitivity based on individual-fund level data.I use $Ret^{sec}_{j,t-1}$, $Ret^{pri}_{j,t-1}$, and $Ret^{style}_{j,t-1}$ to measure mutual fund weekly returns in sequence. The result in Panel A demonstrates how the adopters' flow-performance sensitivity changes around the shock relative to nonadopters. Columns (1), (3), and (5) report the estimates of equation 2.12, and columns (2), (4), and (6) show the specifications without investor-fund fixed effects. The coefficient on the triple-interaction term measures how much the technology increases adopters' flow-performance sensitivity in excess of the corresponding change for non-adopters. The coefficient of interest turns out to be significantly positive at the 5% confidence level in all specifications, echoing the hypothesis that the mobile trading technology substantially prompts investors' response to past performance in a short horizon.

Panel B of Table 2.9 presents the effect on investors' sensitivity to market sentiment. Column (1) corresponds to equation 2.14 and columns (2) to (3) to equation 2.13. Column (2) shows that the investors who switch from the desktop to the mobile trading app become more susceptible to the market optimism (and pessimism) after the shock. They buy (sell) more in response to the optimistic (pessimistic) sentiment relative to the pre-period. The coefficient estimate in column (3) suggests nonadopters do not exhibit the same shift around the technology shock. The significant positive coefficient on the triple-interaction term in column (1) further confirms the technology makes the app adopters more likely to trade in sync with the market-wise sentiment relative to the

change for nonadopters.

**Causal inferences: "Going mobile" and investor flows**

To identify the causal impact of "going mobile" on retail investors' flow-performance sensitivity, I focus on the pairs of adjacent prefectures separated by a state border and compare the change in the sensitivity for the "high-exposure" investors and the "low-exposure" investors around the shock using a triple-differences framework. In particular, I run a regression at the individual-week level of the following form:

$$Netflow_{i,j,t} = \alpha_{i,j} + \beta_1 D_t^{post} \times PRate_i \times Ret_{j,t-1} + \beta_2 D_t^{post} \times Ret_{j,t-1} + \beta_3 PRate_i \times Ret_{j,t-1} + \beta_4 D_t^{post} \times PRate_i + \beta_5 Ret_{j,t-1} + D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}. \tag{2.15}$$

The outcome variable $Netflow_{i,j,t}$ is defined as the shares of fund $j$ purchased by investor $i$ during week $t$ in excess of the number of shares redeemed from fund $j$ over the same period, as a percentage of the average shares of fund $j$ that investor $i$ holds at the end of the prior period. $Ret_{j,t-1}$ represents fund $j$'s performance over week *t-1*. My analysis deploys three different measures of weekly fund performance: $Ret^{sec}_{j,t-1}$, $Ret^{pri}_{j,t-1}$, and $Ret^{style}_{j,t-1}$, as defined in section 2.5.1. I include the individual-by-fund fixed effects, $\alpha_{i,j}$, to remove the effect of the individual-fund-level time-invariant heterogeneity. The coefficient of interest, $\beta_1$, captures the extent to which the mobile trading technology amplifies the flow-performance sensitivity of the high-exposure investors relative the "placebo" effect on low-exposure investors. Consistent with Prediction 2A, the triple-interaction term is expected to load positively.

Likewise, I conduct a triple-differences analysis to make causal inferences on the relationship between the mobile trading technology and the susceptibility of retail investors to market senti-

ment:

$$
\begin{aligned}
Netflow_{i,j,t} = \alpha_{i,j} &+ \beta_1 D_t^{post} \times PRate_i \times Sentiment_{t-1} + \beta_2 D_t^{post} \times Sentiment_{t-1} \\
&+ \beta_3 PRate_i \times Sentiment_{t-1} + \beta_4 D_t^{post} \times PRate_i + \beta_5 Sentiment_{t-1} \\
&+ D_t^{post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + \varepsilon_{i,t}.
\end{aligned} \tag{2.16}
$$

$Sentiment_{t-1}$ denotes the market-wise investor sentiment in week *t-1*. The coefficient of interest, $\beta_1$, represents how much more sensitive the high-exposure investors become to the market sentiment after the technology shock, relative to the same change for low-exposure investors. According to Prediction 2A, $\beta_1$ is expected to be significantly positive.

Panel A of Table 2.10 exhibits the estimated causal impact of "going mobile" on investor flow sensitivity with respect to the past week's fund performance. The first three columns build on the whole sample and the last three on the subsample of investors in paired adjacent prefectures at the province borders. As expected, the coefficients on the triple interaction term $D_t^{post} \times PRate_i \times Ret_{j,t-1}$ are significantly positive across all six specifications. The result suggests the access to the mobile trading technology makes the high-exposure retail investors increasingly sensitive to recent fund performance relative to the less affected investors with similar attributes and geographic locations, confirming the causal interpretation of my findings in section 2.5.1.

Panel B of Table 2.10 reports the technology's effect on investors' susceptibility to market sentiment. The first column is based on the investors in the matched prefectures and the second column is based on the whole sample. The positive coefficient on the triple interaction term suggests the high-exposure investors become more susceptible to market sentiment and trade more in sync with the sentiment after the technology shock, relative to the low-exposure investors in the neighboring prefectures across the province border. Notably, the negative loading of the term $PRate * D_t^{post}$ further confirms that after the shock, the high-exposure investors become more likely to sell when pessimism is prevalent in the market, relative to the pre-post change for low-exposure investors in the neighboring prefecture.

### 2.5.2 Technology and Mutual Fund Performance

Observing the amplified flow volatility and intensified flow comovement leads to the question of whether the new development on flows hurts fund performance. Although, in theory, accommodating large flows can drive up the costs associated with liquidity-motivated trades, whether the incremental flows stemming from mobile trading play a pivotal role in fund performance is still an empirical question.

If the investor flows fueled by the technology are detrimental to fund performance, the mutual funds available on the app are likely to see a downturn in their performance after the shock. To test this hypothesis, I run the following regression at the fund-week level:

$$Ret_{j,t} = \alpha_j + \beta_1 D_t^{post} + \varepsilon_{j,t}. \tag{2.17}$$

I employ three measures for $Ret_{j,t}$: $Ret_{j,t}^{sec}$, $Ret_{j,t}^{pri}$, and $Ret_{j,t}^{style}$. The definitions of all variables and subscripts are consistent with specification 2.11. The coefficient of interest, $\beta_1$, captures the change in fund shares' abnormal returns pre versus post the technology shock. $D_t^{post}$ is expected to load negatively, in line with Prediction 3.

The above naive regression comes with a caveat: Detecting a significant performance decline unnecessarily means the downturn is related to the heightened investor flows arising from the mobile trading app. To confirm whether the flows from the app drive the performance change, I estimate a regression of the following form:

$$\begin{aligned} Ret_{j,t} = \alpha_j &+ \beta_1 D_t^{post} \times |NetFlow|_{j,t-1}^{adopter} + \beta_2 D_t^{post} + \beta_3 D_t^{post} \times Ret_{j,t-1} \\ &+ \beta_4 D_t^{post} \times Ret_{j,t-2} + \beta_5 Ret_{j,t-1} + \beta_6 Ret_{j,t-2} + \varepsilon_{j,t}, \end{aligned} \tag{2.18}$$

where $|NetFlow|_{j,t-1}^{adopter}$ represents the absolute value of app users' net flow faced by fund $j$ during week *t-1*. Here, I focus on the impact of the flow size regardless of the direction of the flow, because both inflow and outflow shocks are likely to incur liquidity costs and affect fund perfor-

mance. The coefficient of interest, $\beta_1$, captures the fund return elasticity with respect to the size of investor flows generated by app adopters after the technology shock. Significantly positive $\beta_1$ and insignificant $\beta_2$ are expected, according to the hypothesis that the performance decline is attributed to the low return arising from the costly adjustment following massive flows from the app.

One might be concerned that the association between the flow size and fund returns merely reflects a structural break in the autocorrelation of weekly returns rather than reflecting indirect liquidity costs. For example, the combination of the reinforced short-term return reversal and the increased flow-performance sensitivity could translate into a spurious correlation between investor flows and subsequent fund returns. To alleviate this concern, I incorporate the terms $D_t^{post} \times Ret_{j,t-1}$, $D_t^{post} \times Ret_{j,t-2}$, $Ret_{j,t-1}$, and $Ret_{j,t-2}$ to control for the change in the return autocorrelation around the shock. A negative loading of $D_t^{post} \times |NetFlow|_{j,t-1}^{adopter}$ despite the inclusion of the above control variables would indicate the effect of investor flow is not driven by the return autocorrelation.

To further test Prediction 3, I use a triple-differences framework. If the technology indeed depresses fund performance, the funds with higher exposure to the shock should disproportionately suffer from the app users' flow. To test this idea, I explore a cross-fund variation in the intensity of treatment. Considering that the app is predominantly used by retail investors, a fund's exposure to the shock increases with the percentage of AUM held by retail investors. I sort the mutual funds by the share ownership of retail investors and divide the funds into two groups: funds with large retail ownership ("retail-oriented" funds) and funds with low retail ownership ("institutional-oriented" funds). My analysis compares the two groups on their performance change around the shock. In particular, I estimate a DID specification of the following form:

$$Ret_{j,t} = \alpha_j + \beta_1 D_t^{post} \times D_j^{HighRetail} + \beta_2 D_t^{post} + \varepsilon_{j,t} \tag{2.19}$$

$D_j^{HighRetail}$ is a dummy variable equal to 1 for the "retail-oriented" funds and equal to zero for the "institutional-oriented" funds. The coefficient of interest, $\beta_1$, measures how much the abnormal returns of "retail-oriented" funds increase after the shock relative to the pre-period, using the same

change in "institutional-oriented" funds as a benchmark. In accordance with the prediction, I conjecture that $\beta_1$ is significantly negative.

Furthermore, I test whether the investor flow arising from the app can explain the return dispersion between the two groups of funds. I augment equation 2.18 with another layer of difference, namely, a fund's retail ownership, to examine whether the "retail-oriented" funds are more vulnerable to the depressing effect of app users' flows. As discussed above, $\beta_1$ in equation 2.18 captures the return sensitivity with respect to app users' flow in the post-period. I expect the sensitivity to be particularly strong in the funds dominated by retail investors. Accordingly, I estimate a triple-differences specification of the following form:

$$\begin{aligned} Ret_{j,t} = \alpha_j + \beta_1 D_t^{post} \times D_j^{HighRetail} \times D_{j,t-1}^{HighFlow} + \beta_2 D_t^{post} \times D_{j,t-1}^{HighFlow} \\ + \beta_3 D_t^{post} \times D_j^{HighRetail} + \beta_4 D_t^{post} + \varepsilon_{j,t}, \end{aligned} \tag{2.20}$$

where $D^{HighFlow}$ is a dummy variable equal to zero if the magnitude of the net flow generated by app adopters falls in the bottom half, and 1 otherwise. The key coefficient, $\beta_1$, measures the extent to which the "retail-oriented" funds outperform other funds during the week following a large app user flow, using the retail-institution return gap after relatively small flows as a benchmark. Put differently, it captures the difference between "retail-oriented" and "institutional-oriented" funds in their return elasticity with respect to the size of flow from the mobile trading app after the technology shock. According to Prediction 3, the triple-interaction term is expected to load negatively.

Table 2.11 presents how fund performance changes after the shock and whether the increased investor flows arising from mobile trading explain this change. I use three measures for the abnormal fund returns: $Ret_{j,t}^{sec}$ in columns (1), (4), and (7); $Ret_{j,t}^{pri}$ in columns (2), (5), and (8); and $Ret_{j,t}^{style}$ in columns (3), (6), and (9). Columns (1)-(3) present the pre-post change in fund returns corresponding to equation 2.17. As predicted, $D^{post}$ loads negatively at the 5% confidence level in two of the three specifications. As shown in Column (1), the weekly return drops by 0.087 after the shock, which translates to a 4% annual decline. Columns (4)-(6) show that incorporating the interaction term $D^{post} \times |NetFlow|^{adopter}$ renders the coefficient on $D^{post}$ insignificant, suggesting

that during the periods after a mild app user flow, the fund performance remains unchanged from the pre-period. The negative loading of the interaction term suggests the decline in returns primarily stems from the poor performance following high investor flows from app users. Columns (7)-(9) report the estimation of specification 2.18, controlling for the possible change in the return autocorrelation around the shock. The negative loading of $D^{post} \times |NetFlow|^{adopter}$ further confirms that the observed return-flow relationship is not confounded by a structural break in the return autocorrelation.

Table 2.12 reports whether the "retail-oriented" funds suffer more from the surging investor flows brought about by the mobile trading app. As in Table 2.11, I use three measures for the abnormal fund returns: $Ret^{sec}_{j,t}$ in columns (1) and (4); $Ret^{pri}_{j,t}$ in columns (2) and (5); and $Ret^{style}_{j,t}$ in columns (3) and (6). The first three columns display the estimates of Regression 2.19. The significantly negative coefficient on the interaction term $D^{post} \times D^{HighRetail}$ suggests the performance of "retail-oriented" funds deteriorates more than other funds, in support of Prediction 3. Columns (4)-(6) present the estimates of Regression 2.20. As expected, the triple-interaction term loads negatively, and $D^{post} \times D^{HighRetail}$ does not load, which jointly indicates the underperformance of the "retail-oriented" funds primarily comes from the low returns following massive app user flows using the "institutional-oriented" funds as a benchmark. In other words, it suggests the investor flow arising from mobile trading is the underlying driver for the performance dispersion in "retail-oriented" funds and other funds after the shock.

## 2.6 Conclusion

This paper documents the change in investor behavior and the intensification of mutual funds' fragility arising from the innovation in trading technology. I exploit the launch of a new mobile trading app at one of the largest investment advisers in China as an exogenous shock to the availability of mobile trading infrastructure. The analysis shows the technology is likely to exacerbate cognitive biases such as over-confidence and self-control problems and therefore induces investors to pay more attention and trade more. The increased trading activity at the individual level con-

verts to heightened flow comovement at the fund level. In particular, using the app makes investors more sensitive to short-term trends such as the past week's fund performance and the latest market sentiment, which in turn intensify investor flows. As a result, the increased investor flows drive up the liquidity costs for the mutual funds and hurts mutual fund performance.

Table 2.1: **Summary Statistics.** Panel A reports the mean, median, and standard deviation of mutual fund characteristics for all the mutual funds in my sample. Panel B displays the mean, median, and standard deviation of investor characteristics. *Age* is the investor age at the app upgrade. In Panel C, columns (1) and (2) report the mean investor characteristics for the adopters and nonadopters matched on various predetermined attributions. Column (3) presents the difference between the two groups, and column (4) reports the p-value of the difference. All variables are defined in Section 2.2.3.

*Panel A: Summary Statistics of Mutual Funds*

| | Mean | Median | Standard Deviation |
|---|---|---|---|
| AUM (billion RMB) | 3.19 | 2.60 | 3.87 |
| Fund Age | 4.32 | 3.98 | 2.07 |
| % Equity | 85.35% | 85.51% | 7.41% |
| Management Fee | 1.28% | 1.50% | 0.38% |
| Custodian Fee | 0.22% | 0.25% | 0.06% |
| % Retail Investors | 71.34% | 80.39% | 25.08% |
| Fund Return (Annualized) | 9.03% | 13.51% | 16.50% |
| Net Investor Flow (Weekly) | -0.05% | -0.13% | 6.21% |

*Panel B: Summary Statistics of Fund Investors*

| | Mean | Median | Standard Deviation |
|---|---|---|---|
| Age | 41.82 | 40.00 | 11.55 |
| Male | 0.52 | 1.00 | 0.50 |
| College Educated | 0.21 | 0.00 | 0.41 |
| Login Frequency (Monthly) | 3.81 | 0.00 | 11.24 |
| Account Size (In thousand shares) | 37.58 | 10.71 | 144.64 |
| Trading Volume (Monthly) | 1,141 | 0 | 7,443 |
| Trading Frequency (Monthly) | 0.31 | 0.00 | 1.40 |

*Panel C: Summary Statistics of the Matched Sample*

| | Adopter [1] | Nonadopter [2] | Difference [3] | p-value [4] |
|---|---|---|---|---|
| Age | 36.00 | 37.51 | -1.51 | 0.13 |
| Male | 0.61 | 0.61 | 0.00 | 0.86 |
| Login Frequency (Monthly) | 7.60 | 6.78 | 0.82 | 0.39 |
| Account Size (In thousand shares) | 40.93 | 37.85 | 3.08 | 0.66 |
| Trading Volume (Monthly) | 2,051.26 | 2,003.82 | 47.44 | 0.94 |
| Observations | 13,354 | 11,482 | | |

Table 2.2: **Who Adopts the Mobile Trading App?** The table reports the estimates of a probit model at the individual level based on the whole sample, corresponding to equation 2.2. The outcome variable, $D^{adoptor}$, is a dummy variable taking a value of 1 if the investor adopts the app and zero otherwise. *Age* is the investor age at the app upgrade. *Education* is an integer with a scale of 0 to 5, increasing with the highest educational degree an investor obtained. *Male* is a dummy variable taking a value of 1 for male investors and zero for female investors. *LoginFreq* denotes the average monthly login times into the direct sales platform prior to the app upgrade. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | App Adoption | | |
|---|---|---|---|
| | [1] | [2] | [3] |
| *Age* | -0.027*** | -0.032*** | -0.033*** |
| | [0.001] | [0.002] | [0.002] |
| *Education* | 0.079*** | 0.059*** | 0.079*** |
| | [0.017] | [0.021] | [0.022] |
| *Male* | 0.371*** | 0.440*** | 0.453*** |
| | [0.026] | [0.032] | [0.035] |
| *LoginFreq* | 0.093*** | 0.081*** | 0.064*** |
| | [0.003] | [0.003] | [0.002] |

Table 2.3: **Technology Impact on Investor Behavior: App Adopters vs. Nonadopters.** Panels A and B report the estimates of difference-in-differences regressions at the individual-month level. The outcome variable in Panel A (Panel B) is *Login* ( *Volume*) as defined in Section 2.4.2. $D^{post}$ is a dummy variable indicating whether a given month is after the shock. In columns (1)-(5), $D^{adopter}$ is a dummy variable taking a value of 1 for investors who have used the app since the upgrade, and 0 otherwise. In column (6) ((7)), $D^{adopter}$ takes a value of 1 for the investors who start using the app within 12 (6) months of the upgrade, and 0 otherwise. *LoginFreq* is the monthly logins into the direct sales online platform before the shock. *AccountSize* is the total number of shares held by an investor at the app upgrade. The other variables are defined as in Table 1.1. Two-way clustered standard errors by individual and by month are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

*Panel A: Financial Attention*

| | Monthly Login Times | | | | Monthly Login Times | | |
|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] |
| $D^{post} * D^{adoptor}$ | 11.127*** | 11.123*** | 11.230*** | 10.850*** | 9.241*** | 10.112*** | 9.974*** |
| | [0.154] | [0.149] | [0.151] | [0.262] | [0.198] | [0.239] | [0.251] |
| $D^{post} * LoginFreq$ | | -0.337*** | -0.332*** | -0.302*** | -0.167*** | -0.194*** | -0.185*** |
| | | [0.036] | [0.036] | [0.089] | [0.039] | [0.040] | [0.040] |
| $D^{post} * Age$ | | 0.006*** | 0.067*** | 0.050*** | 0.000 | 0.000* | -0.001*** |
| | | [0.001] | [0.008] | [0.011] | [0.000] | [0.000] | [0.000] |
| $D^{post} * Male$ | | -0.187 | -0.136 | 0.059 | 0.379*** | 0.415*** | 0.501*** |
| | | [0.151] | [0.150] | [0.223] | [0.094] | [0.112] | [0.126] |
| $D^{post} * Education$ | | 0.325*** | 0.236*** | 0.428*** | 0.204*** | 0.290*** | 0.291*** |
| | | [0.054] | [0.053] | [0.081] | [0.036] | [0.044] | [0.050] |
| $D^{post} * AccountSize$ | | | 0.001*** | 0.001* | | | |
| | | | [0.000] | [0.000] | | | |
| Individual Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County Fixed Effect | No | No | No | Yes | No | No | No |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Matching | Yes | Yes | Yes | Yes | No | No | No |
| Observations | 745,080 | 745,080 | 745,080 | 745,080 | 1,553,130 | 1,284,480 | 1,147,230 |

*Panel B: Trading Intensity*

| | Monthly Trading Volume | | | | Monthly Trading Volume | | |
|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] |
| $D^{post} * D^{adoptor}$ | 749.496*** | 749.122*** | 765.072*** | 750.536*** | 389.873*** | 434.436*** | 459.009*** |
| | [72.499] | [72.246] | [73.117] | [107.444] | [44.702] | [57.308] | [63.470] |
| $D^{post} * LoginFreq$ | | -31.522*** | -31.505*** | -35.490*** | -19.681*** | -24.097*** | -24.528*** |
| | | [6.561] | [6.556] | [7.366] | [4.451] | [4.685] | [4.731] |
| $D^{post} * Age$ | | 0.946* | 7.900* | 8.430 | 0.019 | 0.011 | 0.018 |
| | | [0.535] | [4.741] | [7.200] | [0.111] | [0.146] | [0.175] |
| $D^{post} * Male$ | | -241.976*** | -239.245*** | -191.442 | -53.033 | -78.794* | -75.391 |
| | | [72.953] | [72.943] | [122.230] | [36.811] | [43.647] | [48.350] |
| $D^{post} * Education$ | | 56.810** | 51.008* | 87.882* | 1.034 | 1.874 | -3.137 |
| | | [28.253] | [29.057] | [51.724] | [12.848] | [15.391] | [17.327] |
| $D^{post} * AccountSize$ | | | -0.001* | -0.001 | | | |
| | | | [0.000] | [0.001] | | | |
| Individual Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County Fixed Effect | No | No | No | Yes | No | No | No |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Matching | Yes | Yes | Yes | Yes | No | No | No |
| Observations | 745,080 | 745,080 | 745,080 | 745,080 | 1,553,130 | 1,284,480 | 1,147,230 |

Table 2.4: **Does Mobile Trading Technology Amplify Overconfidence?** Panel A and Panel B report the estimates of equation 2.5 and equation 2.6 at the individual-month level, respectively. The outcome variable in columns (1)-(4) is *Login*, the total logins to the online trading platform for an investor in a given month, and the outcome variable in columns (5)-(8) is *Volume*, the number of mutual fund shares an investor trades in a given month. $Decile_i^{DeceptiveGain}$ is defined as the decile of $DeceptiveGain_i$, the number of deceptive gains as a percentage of the total number of buy transactions performed by investor $i$ prior to the shock, weighted by the value of each buy. The definitions of other explanatory variables are consistent with Table 2.3. Two-way clustered standard errors by individual and by month are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

*Panel A: Men vs. Women*

| | Monthly Login Times | | | | Monthly Trading Volume | | | |
|---|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| $D^{post} * D^{adopter} * Male$ | 3.461*** | 3.220*** | 3.504*** | 3.162*** | 483.288*** | 461.951*** | 482.141*** | 443.135** |
| | [0.274] | [0.288] | [0.274] | [0.473] | [138.247] | [136.724] | [138.256] | [217.692] |
| $D^{post} * D^{adopter}$ | 9.008*** | 9.163*** | 9.091*** | 9.133*** | 453.909*** | 468.211*** | 470.777*** | 509.870*** |
| | [0.178] | [0.184] | [0.179] | [0.240] | [87.509] | [87.918] | [87.600] | [147.337] |
| $D^{post} * Male$ | -1.500*** | -2.194*** | -1.465*** | -1.136*** | -425.271*** | -489.443*** | -421.990*** | -358.989** |
| | [0.219] | [0.221] | [0.218] | [0.316] | [112.503] | [117.721] | [112.343] | [180.302] |
| $D^{post} * LoginFreq$ | -0.339*** | | -0.333*** | -0.303*** | -31.771*** | | -31.740*** | -35.600*** |
| | [0.035] | | [0.036] | [0.089] | [6.587] | | [6.580] | [7.373] |
| $D^{post} * Age$ | 0.006*** | | 0.069*** | 0.051*** | 0.968* | | 8.054* | 8.595 |
| | [0.001] | | [0.008] | [0.011] | [0.535] | | [4.743] | [7.184] |
| $D^{post} * AccountSize$ | | | 0.000*** | 0.000* | | | -0.001* | -0.001 |
| | | | [0.000] | [0.000] | | | [0.000] | [0.001] |
| $D^{post} * Education$ | 0.315*** | | 0.224*** | 0.416*** | 55.335** | | 49.327* | 86.265* |
| | [0.054] | | [0.053] | [0.080] | [28.229] | | [29.037] | [51.686] |
| Individual Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County Fixed Effect | No | No | No | Yes | No | No | No | Yes |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Matching | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 |

*Panel B: The Interaction with Individual Past Deceptive Success*

| | Monthly Login Times | | | | Monthly Trading Volume | | | |
|---|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| $D^{post} * D^{adopter} * Decile^{DeceptiveGain}$ | 0.877*** | 0.825*** | 0.864*** | 1.445* | 229.882** | 233.155* | 224.375** | 317.047* |
| | [0.304] | [0.307] | [0.303] | [0.833] | [114.409] | [128.990] | [114.478] | [192.164] |
| $D^{post} * D^{adopter}$ | 8.617*** | 8.873*** | 8.802*** | 5.356 | 434.200 | -621.318 | 456.378 | 705.124 |
| | [1.269] | [1.280] | [1.261] | [3.421] | [325.925] | [413.675] | [325.743] | [496.502] |
| $D^{post} * Decile^{DeceptiveGain}$ | -0.060 | -0.004 | -0.028 | -0.178 | 25.726 | 422.006 | 20.910 | -195.482 |
| | [0.164] | [0.168] | [0.164] | [0.263] | [103.486] | [456.146] | [104.086] | [162.395] |
| $D^{post} * LoginFreq$ | -0.290*** | | -0.287*** | -0.135 | -23.848** | | -24.204** | -29.249*** |
| | [0.082] | | [0.083] | [0.207] | [9.814] | | [9.880] | [10.032] |
| $D^{post} * Age$ | 0.008*** | | 0.095*** | 0.083*** | -0.335 | | -4.962 | 41.372** |
| | [0.002] | | [0.019] | [0.028] | [1.302] | | [10.583] | [17.875] |
| $D^{post} * AccountSize$ | | | 0.000*** | 0.000* | | | -0.002* | -0.006*** |
| | | | [0.000] | [0.000] | | | [0.001] | [0.002] |
| $D^{post} * Male$ | -0.375 | | -0.385 | -0.333 | -58.996 | | -63.061 | 145.660 |
| | [0.247] | | [0.247] | [0.494] | [144.096] | | [143.563] | [264.185] |
| $D^{post} * Education$ | 0.774*** | | 0.627*** | 0.691*** | 42.991 | | 64.350 | 299.960** |
| | [0.108] | | [0.113] | [0.162] | [84.112] | | [85.654] | [143.503] |
| Individual Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County Fixed Effect | No | No | No | Yes | No | No | No | Yes |
| Month Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Matching | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 | 745,080 |

Table 2.5: **Does Mobile Trading Technology Aggravate Self-Control Problems?** Panel A reports the estimates of equation 2.7 and its variants at the individual-month level. The outcome variables in columns (1)-(2) and columns (3-(4) are *Login* and *Volume* as defined in Table 1.5. $D^{senior}$ denotes a dummy variable taking a value of 1 for investors in the top quartile by age, and zero for investors in the bottom quartile. Panel B reports the estimates of equation 2.8 and its variants at the individual-day level. The outcome variable in columns (1)-(3) is *Login*, the total logins to the online trading platform for an investor in a given day, and the outcome variable in columns (4)-(6) is *Volume*, the number of mutual fund shares an investor trades in a given day. $DayofWeek$ denotes the day of the week for a given day. The definition of all other explanatory variables are consistent with Table 2.3. All estimates are based on the matched sample of adopters and nonadopters. Two-way clustered standard errors by individual and by month are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

*Panel A: Younger vs. Older People*

| | Monthly Login Times | | Monthly Trading Volume | |
|---|---|---|---|---|
| | [1] | [2] | [3] | [4] |
| $D^{post} * D^{adoptor} * D^{senior}$ | -3.216*** | -4.611*** | -335.542*** | -463.930*** |
| | [0.308] | [0.301] | [116.393] | [120.114] |
| $D^{post} * D^{adopter}$ | 12.952*** | 13.053*** | 781.230*** | 796.653*** |
| | [0.196] | [0.191] | [65.753] | [66.293] |
| $D^{post} * D^{senior}$ | 3.782*** | 3.054*** | 350.796*** | 384.975*** |
| | [0.265] | [0.263] | [98.559] | [100.355] |
| $D^{post} * LoginFreq$ | | -0.415*** | | -31.973*** |
| | | [0.016] | | [3.922] |
| $D^{post} * Male$ | | -0.174 | | -305.823*** |
| | | [0.149] | | [56.009] |
| $D^{post} * Education$ | | 0.323*** | | 16.946 |
| | | [0.062] | | [22.521] |
| $D^{post} * TradingVolume$ | | 0.000*** | | -0.001*** |
| | | [0.000] | | [0.000] |
| Individual Fixed Effect | Yes | Yes | Yes | Yes |
| Month Fixed Effect | Yes | Yes | Yes | Yes |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes |
| Matching | Yes | Yes | Yes | Yes |
| Observations | 372,890 | 372,890 | 372,890 | 372,890 |

*Panel B: Monday vs. Friday Effect*

| | Daily Login Times | | | Daily Trading Volume | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| | All | Non-retired | Retired | All | Non-retired | Retired |
| $D^{post} * D^{adopt} * DayofWeek$ | 0.006*** | 0.008*** | 0.001 | 24.743*** | 31.340*** | 3.930 |
| | [0.002] | [0.002] | [0.005] | [8.789] | [8.721] | [44.140] |
| $D^{post} * DayofWeek$ | 0.003 | 0.002 | -0.001 | -2.058 | -4.579 | 11.557 |
| | [0.003] | [0.002] | [0.004] | [6.678] | [5.808] | [19.723] |
| $D^{adoptor} * DayofWeek$ | -0.002* | -0.001 | -0.008* | -15.358 | -14.38 | -78.601 |
| | [0.001] | [0.001] | [0.005] | [15.390] | [16.127] | [78.590] |
| $D^{post} * D^{adoptor}$ | 0.421*** | 0.416*** | 0.315*** | 64.612* | 36.613 | 144.417 |
| | [0.009] | [0.010] | [0.035] | [36.623] | [39.000] | [207.386] |
| $DayofWeek$ | -0.003 | -0.004 | 0.007 | 1.486 | 0.695 | 13.881 |
| | [0.003] | [0.004] | [0.004] | [4.047] | [4.661] | [15.839] |
| Individual Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes |
| Week Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes | Yes | Yes |
| Matching | Yes | Yes | Yes | Yes | Yes | Yes |

Table 2.6: **Summary Statistics of the Paired Prefectures on Two Sides of a Province Border.** Panel A exhibits the demographic attributes of the entire population for the matched High Exposure and Low Exposure prefectures. All variables are produced based on the 2010 Population Census of China by County and are at the prefecture level. Column (1) (column (2)) displays the mean characteristics for High (Low) Exposure prefectures, and columns (3) and (4) show the difference between the first two columns and the standard deviation of the estimate for the difference, respectively. *GDP Per Cap* denotes the total GDP of a prefecture in 2010 denominated by its population. *Male* represents the percentage of males in its population as of 2010. *Mortality Rate* is the number of deaths in 2010 as a percentage of the total population. The fourth to sixth lines present the age breakdown of the prefecture population. *Years of Education* is the year of education that an average resident in a prefecture received. *Housing Area per Cap* captures the average area of housing in square meters. Panel B reports the statistics of mutual fund investors residing in the matched prefectures. The variable definitions in Panel B are consistent with Table 2.1.

*Panel A: Prefecture Characteristics*

| | [1] High Exposure | [2] Low Exposure | [3] Difference | [4] Standard Deviation |
|---|---|---|---|---|
| GDP Per Cap (RMB) | 36,153.20 | 34,145.59 | 2,007.61 | 2,110.32 |
| Male | 51.3% | 51.2% | 0.1% | 0.1% |
| Mortality Rate | 0.6% | 0.6% | 0.0% | 0.0% |
| Age [15,65] | 73.1% | 72.8% | 0.3% | 0.4% |
| Age Above 65 | 8.7% | 8.7% | -0.1% | 0.2% |
| Age Below 15 | 18.3% | 18.5% | -0.2% | 0.4% |
| Years of Education | 8.58 | 8.45 | 0.13 | 0.10 |
| College Graduate | 3.2% | 2.8% | 0.4% | 0.3% |
| Housing Area Per Cap ($m^2$) | 30.16 | 29.92 | 0.23 | 0.60 |

*Panel B: Investor Characteristics*

| | [1] High Exposure | [2] Low Exposure | [3] Difference | [4] Standard Error |
|---|---|---|---|---|
| Trading Volume (Monthly) | 854.72 | 793.54 | 61.18 | 46.85 |
| Login Frequency (Monthly) | 2.89 | 2.88 | 0.01 | 0.18 |
| Account Size (in RMB) | 19,588.87 | 19,921.92 | -333.05 | 1,148.25 |
| Male | 51.6% | 53.3% | -1.6% | 1.3% |
| Age | 41.37 | 41.25 | 0.11 | 0.29 |

Table 2.7: **Spatial Discontinuity Analysis: Mobile Trading Adoption and Investor Behavior.** This table reports the 2SLS analysis of the technology's effect on investor behavior. Column (1) reports the first-stage regression of $D^{post} * D^{adopter}$ against the key explanatory variable $D^{post} * PRate$ and other controls. $Penetration\ rate$ represents the smartphone penetration rate of the province of residence in 2011. The outcome variable in columns (2)-(3) is $Login$, and is $Volume$ in columns (4)-(5). Columns (3) and (5) correspond to reduced-form analysis specified in equation 2.10, whereas columns (2) and (4) report the second-stage analysis taking the predicted value of $D^{post} \times D^{adopter}$ from the first-stage analysis as the main regressor. The definitions of all other variables are consistent with Table 2.3. Standard errors are clustered at the individual level and are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{post} * D^{adopter}$ | Monthly Login Times | | Monthly Trading Volume | |
|---|---|---|---|---|---|
| | First Stage | Second Stage | Reduced Form | Second Stage | Reduced Form |
| | [1] | [2] | [3] | [4] | [5] |
| $D^{post} * PRate$ | 0.536*** | | 4.930*** | | 936.061*** |
| | [0.036] | | [1.447] | | [332.374] |
| $Instrumented\ D^{post} * D^{adopter}$ | | 9.206*** | | 1,747.785*** | |
| | | [2.632] | | [620.916] | |
| $D^{post}$ * Ex-Ante Login Freq | 0.030*** | -0.170** | 0.106*** | -35.940 | 16.513** |
| | [0.000] | [0.080] | [0.011] | [59.567] | [8.184] |
| $D^{post} * Age$ | -0.006*** | -0.004 | -0.059*** | 4.981 | -5.621 |
| | [0.000] | [0.017] | [0.005] | [12.560] | [3.863] |
| $D^{post} * Male$ | 0.087*** | 0.232 | 1.037*** | -246.195 | -93.505 |
| | [0.003] | [0.251] | [0.125] | [187.209] | [90.473] |
| $D^{post} * Education$ | -0.008*** | 0.293*** | 0.219*** | -18.201 | -32.253 |
| | [0.001] | [0.054] | [0.050] | [40.303] | [35.984] |
| $D^{post} * Account\ Size$ | 0.000 | 0.000 | 0.000 | 0.007*** | 0.007*** |
| | [0.000] | [0.000] | [0.000] | [0.001] | [0.001] |
| Individual Fixed Effect | Yes | Yes | Yes | Yes | Yes |
| Clustered by Individual | Yes | Yes | Yes | Yes | Yes |
| Post × Prefecture Pair Dummy | Yes | Yes | Yes | Yes | Yes |
| Observations | 44,070 | 44,070 | 44,070 | 44,070 | 44,070 |
| Cragg-Donald Wald F statistic: | 215.702 | Stock-Yogo critical values: 10% maximal IV size: | | | 16.38 |

Table 2.8: **Change in Volatility of Investor Flow: Adopters vs. Nonadopters.** Panel A presents the statistics of investor flow volatility before and after the app upgrade for app adopters and nonadopters. The first three rows show the unconditional statistics based on the whole sample, whereas the following three rows build on the subsample of adopters and nonadopters who are matched on a variety of predetermined attributes. The outcome variables in columns (1)-(2), (3)-(4), and (5)-(6) are the volatility of weekly net flow, inflow, and outflow as a percentage of the ending balance in the prior week, respectively. Panel B reports a regression at the quarter-fund level. The outcome variable is the flow volatility derived from the weekly flow within a given quarter. $D^{adopt}$ is a dummy variable equal to 1 if the flow is created by app adopters, and zero for nonadopters' flow. Quarterly and $Fund \times D^{adopt}$ fixed effects are incorporated. The standard errors are clustered at the fund level. The standard errors of the estimates are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

*Panel A: Summary Statistics of Flow Volatility*

| | Net Flow | | Inflow | | Outflow | |
|---|---|---|---|---|---|---|
| | Full Sample | | Full Sample | | Full Sample | |
| | Adopter | Nonadopter | Adopter | Nonadopter | Adopter | Nonadopter |
| Pre-shock | 4.2% | 5.0% | 2.7% | 3.0% | 2.9% | 3.1% |
| Post-shock | 10.8% | 5.8% | 8.7% | 4.1% | 5.8% | 3.3% |
| Increase in Volatility(%) | 156.5% | 15.9% | 225.6% | 36.6% | 97.0% | 7.8% |
| | Matched Sample | | Matched Sample | | Matched Sample | |
| | Adopter | Nonadopter | Adopter | Nonadopter | Adopter | Nonadopter |
| Pre-shock | 4.2% | 4.4% | 2.7% | 3.2% | 3.0% | 2.8% |
| Post-shock | 10.2% | 5.6% | 8.2% | 4.1% | 5.8% | 3.5% |
| Increase in Volatility(%) | 140.4% | 26.9% | 202.3% | 27.5% | 97.9% | 22.2% |

*Panel B: Difference-in-difference Analysis*

| | Vol(Net Flow) | Vol(Inflow) | Vol(Outflow) | Vol(Net Flow) | Vol(Inflow) | Vol(Outflow) |
|---|---|---|---|---|---|---|
| $D^{post} * D^{adopt}$ | 0.041*** | 0.037*** | 0.015* | 0.046*** | 0.032** | 0.018** |
| | [0.012] | [0.013] | [0.009] | [0.013] | [0.013] | [0.008] |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Fund*Adoption FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Matching | Yes | Yes | Yes | No | No | No |
| Observations | 540 | 540 | 540 | 540 | 540 | 540 |

Table 2.9: **Does the Technology Affect Flow-Performance Sensitivity and Susceptibility to Market Sentiment?** This table reports the estimates of investor flow regressions at the investor-fund-week level. The outcome variable is $NetFlow$, the shares of a given fund purchased by an investor during a given week in excess of the number of shares redeemed, as a percentage of the ending balance of the prior period. Panel A reports the estimate of equation 2.12. $Ret_{t-1}$ denotes the fund performance over Week *t-1*.. $D_{post}$ is a dummy variable indicating whether a given week is after the shock. $D^{adopter}$ denotes a dummy variable taking a value of 1 for adopters, and zero otherwise. In columns (1) and (4), $Ret_{t-1}$ represents $Ret^{sec}$, whereas in columns (2) and (5) ((3) and(6)), I use $Ret^{pri}$ ($Ret^{style}$). Column (1) of Panel B reports the estimate of equation 2.14 based on the matched adopters and nonadopters. Columns (2) to (3) correspond to equation 2.13, whereas column (2) focuses on the adopters and column (3) on the nonadopters. $Sentiment_{t-1}$ denotes the investor sentiment in China's stock market in week *t-1*. Two-way clustered standard errors by individual and by month are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

*Panel A: Triple-Differences Analysis: How Flow-Performance Sensitivity Change Is Different for Adopters versus Nonadopters*

| | Net Investor Flow | | | Net Investor Flow | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| $D^{adopt} * D^{post} * Ret_{t-1}$ | 0.746** | 0.583** | 0.665** | 0.668** | 0.519* | 0.562* |
| | [0.319] | [0.292] | [0.306] | [0.312] | [0.276] | [0.337] |
| $D^{post} * Ret_{t-1}$ | -0.236 | -0.101 | -0.052 | -0.233 | -0.101 | -0.062 |
| | [0.286] | [0.262] | [0.303] | [0.271] | [0.24] | [0.283] |
| $D^{adopt} * Ret_{t-1}$ | -0.398 | -0.320 | -0.387 | -0.341 | -0.268 | -0.309 |
| | [0.277] | [0.252] | [0.289] | [0.276] | [0.248] | [0.286] |
| $D^{adopt} * D^{post}$ | -0.001 | -0.001 | -0.003 | 0.006*** | 0.006*** | 0.005** |
| | [0.003] | [0.003] | [0.004] | [0.002] | [0.002] | [0.002] |
| $Ret_{t-1}$ | 0.239 | 0.163 | 0.135 | 0.241 | 0.164 | 0.153 |
| | [0.260] | [0.236] | [0.268] | [0.253] | [0.226] | [0.257] |
| $D^{post}$ | -0.003 | -0.003 | -0.003 | -0.004** | -0.004** | -0.005** |
| | [0.002] | [0.002] | [0.003] | [0.002] | [0.002] | [0.002] |
| Individual-Fund fixed effect | Yes | Yes | Yes | No | No | No |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes | Yes | Yes |
| Matched sample | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 4,531,438 | 4,531,438 | 4,531,438 | 4,531,438 | 4,531,438 | 4,531,438 |

*Panel B: Does the App Make Investors More Susceptible to Market Sentiment?*

| | Net Investor Flow | | |
|---|---|---|---|
| | Adopter vs. Nonadopter | Adopter | Nonadopter |
| | [1] | [2] | [3] |
| $D^{adopt} * D^{post} * Sentiment_{t-1}$ | 0.140** | | |
| | [0.065] | | |
| $D^{post} * Sentiment_{t-1}$ | -0.056 | 0.084*** | -0.056 |
| | [0.056] | [0.033] | [0.056] |
| $D^{adopt} * Sentiment_{t-1}$ | -0.114 | | |
| | [0.156] | | |
| $D^{adopt} * D^{post}$ | -0.046 | | |
| | [0.043] | | |
| $Sentiment_{t-1}$ | 0.073 | -0.041 | 0.073 |
| | [0.050] | [0.025] | [0.050] |
| $D^{post}$ | 0.016 | -0.029** | 0.016 |
| | [0.020] | [0.012] | [0.020] |
| Individual-Fund Fixed Effect | Yes | Yes | Yes |
| Clustered by Individual and Month | Yes | Yes | Yes |
| Matched Sample | Yes | Yes | Yes |
| Observations | 4,531,438 | 2,278,291 | 2,253,147 |

Table 2.10: **Spatial Discontinuity Analysis: Mobile Trading Technology and Flow Patterns.** Panel A exhibits the reduced-form analysis of the technology's effect on flow-performance sensitivity. The first three columns build on the whole sample, and the last three on the subsample of investors in paired adjacent prefectures across province borders. Panel B reports the reduced-form analysis of the technology's effect on the susceptibility to market sentiment. The first column is based on the matched prefectures and, the second on the whole sample. The outcome variable in both panels is $NetFlow$. The definition of all the variables are consistent with Tables 2.7 and 2.9. Two-way clustered standard errors by individual and by month are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

*Panel A: Flow-Performance Sensitivity*

| | Net Investor Flow | | | Net Investor Flow | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| $PRate * D^{post} * Ret_{t-1}$ | 0.865** | 0.935** | 0.872** | 0.561** | 0.466* | 0.509* |
| | [0.438] | [0.375] | [0.357] | [0.282] | [0.256] | [0.301] |
| $D^{post} * Ret_{t-1}$ | -0.007 | -0.008 | 0.009 | 0.025* | 0.023 | 0.028 |
| | [0.020] | [0.019] | [0.020] | [0.013] | [0.015] | [0.024] |
| $PRate * Ret_{t-1}$ | -0.664 | -0.631 | -0.623 | -0.015 | -0.010 | -0.020 |
| | [0.462] | [0.431] | [0.472] | [0.154] | [0.171] | [0.151] |
| $PRate * D^{post}$ | -0.002 | -0.002 | -0.004 | -0.002 | -0.003 | -0.004 |
| | [0.004] | [0.004] | [0.004] | [0.002] | [0.002] | [0.003] |
| $Ret_{t-1}$ | 0.031 | 0.028 | 0.021 | -0.002 | -0.003 | -0.002 |
| | [0.027] | [0.026] | [0.016] | [0.009] | [0.012] | [0.009] |
| $D^{post}$ | 0.000 | 0.000 | 0.000 | | | |
| | [0.000] | [0.000] | [0.000] | | | |
| Individual-Fund Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered by Individual and Month | Yes | Yes | Yes | Yes | Yes | Yes |
| Post × Prefecture Pair Dummy | No | No | No | Yes | Yes | Yes |
| Observations | 1,180,731 | 1,180,731 | 1,180,731 | 268,083 | 268,083 | 268,083 |

*Panel B: Flow Sensitivity to Market Sentiment*

| | Net Investor Flow | |
|---|---|---|
| | [1] | [2] |
| $PRate * D^{post} * Sentiment_{t-1}$ | 0.181** | 0.332** |
| | [0.073] | [0.162] |
| $D^{post} * Sentiment_{t-1}$ | -0.002 | 0.001 |
| | [0.004] | [0.004] |
| $PRate * Sentiment_{t-1}$ | 0.030 | 0.018 |
| | [0.047] | [0.085] |
| $PRate * D^{post}$ | -0.066** | -0.117* |
| | [0.026] | [0.061] |
| $Sentiment_{t-1}$ | 0.003 | 0.000 |
| | [0.003] | [0.003] |
| $D^{post}$ | | 0.000 |
| | | [0.002] |
| Individual-Fund Fixed Effect | Yes | Yes |
| Clustered by Individual and Month | Yes | Yes |
| Post*Prefecture Pair Dummy | Yes | No |
| Observations | 268,083 | 1,180,731 |

Table 2.11: **Do App Users' Flows Drive Down Mutual Fund Returns after the Shock?** This table reports the estimates of fund-return regressions at the fund-week level. For the outcome variables, I use three measures for abnormal fund return: $Ret^{sec}$ in columns (1), (4), and (7); $Ret^{pri}$ in columns (2), (5), and (8); and $Ret^{style}$ in columns (3), (6), and (9). Columns (1)-(3) present the pre-post change in fund returns corresponding to equation 2.17. Columns (4)-(9) report the estimation of specification 2.18 and its variants. $|NetFlow|_{t-1}$ represents the absolute value of app users' net flow Fund *j* experiences during month *t-1*. Two-way clustered standard errors by fund and by week are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | Abnormal Return (%) | | | Abnormal Return (%) | | | Abnormal Return (%) | | |
|---|---|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] | [9] |
| $D^{post} * \|Net\ Flow\|_{t-1}$ | | | | -1.921** | -1.850** | -1.613* | -1.894** | -1.832** | -1.570* |
| | | | | [0.945] | [0.914] | [0.933] | [0.951] | [0.932] | [0.934] |
| $D^{post}$ | -0.087** | -0.087** | -0.093** | 0.054 | 0.025 | 0.011 | 0.066 | 0.031 | 0.018 |
| | [ 0.042] | [-0.043] | [0.046] | [0.057] | [0.058] | [0.059] | [0.057] | [0.058] | [0.058] |
| $D^{post} * Ret_{t-1}$ | | | | | | | 0.186 | -2.695 | 3.890 |
| | | | | | | | [5.136] | [5.354] | [5.038] |
| $D^{post} * Ret_{t-2}$ | | | | | | | -4.651 | 2.892 | -4.563 |
| | | | | | | | [5.172] | [5.317] | [4.979] |
| $Ret_{t-1}$ | | | | | | | -3.511 | -3.879 | -4.745 |
| | | | | | | | [3.185] | [3.285] | [3.357] |
| $Ret_{t-2}$ | | | | | | | 3.970 | -0.417 | 3.791 |
| | | | | | | | [3.164] | [3.205] | [3.317] |
| Fund Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered by Fund and Week | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3,272 | 3,272 | 3,272 | 3,272 | 3,272 | 3,272 | 3,272 | 3,272 | 3,272 |

Table 2.12: **Are Funds with Higher Percentage of Retail Investors More Vulnerable to App Users' Flow?** This table reports the estimate of fund-return regressions at the fund-week level. For the outcome variable, I use three measures for abnormal fund return: $Ret^{sec}$ in columns (1) and (4); $Ret^{pri}$ in columns (2) and (5); and $Ret^{style}$ in columns (3) and (6). Columns (1)-(3) present the pre-post change in fund returns corresponding to equation 2.19. Columns (4)-(6) report the estimation of specification 2.20. $D^{HighRetail}$ is a dummy variable taking a value of 1 for the "retail-oriented" funds, and zero for the "institutional-oriented" funds. $D^{HighFlow}$ is a dummy variable taking a value of zero if the magnitude of the net flow generated by the app adopters falls in the bottom half, and 1 otherwise. Two-way clustered standard errors by fund and by week are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | Abnormal Return | | | Abnormal Return | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| $D^{post} * D^{HighRetail} * D^{HighFlow}_{t-1}$ | | | | -0.215* | -0.234* | -0.241* |
| | | | | [0.122] | [0.130] | [0.131] |
| $D^{post} * D^{HighFlow}_{t-1}$ | | | | -0.029 | -0.007 | -0.006 |
| | | | | [0.088] | [0.090] | [0.099] |
| $D^{post} * D^{HighRetail}$ | -0.126** | -0.124* | -0.142** | 0.144 | 0.179 | 0.176 |
| | [0.063] | [0.064] | [0.068] | [0.097] | [0.116] | [0.113] |
| $D^{post}$ | -0.020 | -0.018 | -0.016 | -0.086 | -0.104 | -0.111 |
| | [0.053] | [0.054] | [0.055] | [0.079] | [0.081] | [0.085] |
| Fund Fixed Effect | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered by Fund and Week | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3,272 | 3,272 | 3,272 | 3,272 | 3,272 | 3,272 |

# Chapter 3: Competition for Talent and Mutual Fund Performance: Evidence from Private Fund Deregulation

## 3.1 Introduction

In recent years, many countries have seen the rise of private funds (i.e., hedge funds, private equity, and venture capital) as important players in the asset management realm. The development of the private fund industry potentially challenges the mutual fund industry through, for example, intensifying the competition for talent, investment opportunities, and investors. A particularly fierce battle has been waged on the managerial talent front [90, 91]. Anecdotal evidence shows that many high-profile mutual fund managers left their jobs to pursue higher compensation and greater flexibility at private funds.[1] To what extent has the rise of private funds stolen talent from mutual funds at the aggregate level? Does this competition for human capital affect mutual fund performance?

To answer these questions, I use China's enactment of the "Amended Securities Investment Fund Law" (hereafter, the Amendment) as an exogenous shock to the inter-industry competition faced by mutual funds. Enacted in 2013, the law was designed to legitimize private funds and reduce private fund industry entry barrier. As a result, after the Amendment became effective, the private fund industry experienced a boom in new entires. To establish a causal relationship between the inter-sector competition for talent and mutual fund performance, I exploit the fact that the Amendment affects a subset of mutual funds more than others. In particular, the mutual funds targeting foreign assets, namely, the Qualified Domestic Institutional Investor (QDII) mutual funds, are less affected by the Amendment than others because private funds do not have access to

[1] Two examples are Jeffrey Vinik, the former manager of Fidelity Magellan Fund and Yawei Wang, the former manager of the largest mutual fund (China Asset Management Growth Fund) in China.

the QDII quota, which the government requires for investing in foreign markets. Therefore, private funds cannot compete with the QDII mutual funds.[2]

In a difference-in-differences (DID) framework, I find the non-QDII mutual funds significantly underperform QDII funds after the Amendment and experience substantial outflows relative to the QDII mutual funds. These findings suggest the intensified competition from private funds can dampen mutual fund performance. To ensure these results are not driven by confounders, I examine the dynamics of the performance gap between QDII and non-QDII mutual funds around the shock and find no divergence between the two groups before the shock. Furthermore, to make the QDII and non-QDII funds more comparable, I exploit the within-fund-family variation by introducing fund-management-firm $\times$ quarter fixed effects. My findings are robust to using the alternative specifications.

To understand whether the underperformance of the non-QDII funds results from talent stealing, I perform two sets of analyses. The first compares the change in the managerial turnover rate in the treated and control groups in a DID framework. After the shock, the manger turnover rate increases by more than 50% in the non-QDII funds relative to the QDII funds. Second, I adopt a triple-differences framework that explores the cross-sectional variation in the occurrence of managerial turnovers. The adverse effect of the intensified competition on mutual fund performance is particularly pronounced after a managerial turnover in a mutual fund. Notably, managerial turnovers account for more than half of the competition effect in the baseline DID analysis, which underscores the importance of human capital loss in driving the deterioration in fund performance. This result also suggests that the quality of mutual fund managers matters for fund performance, which contributes to the debate on whether mutual fund managers have investment skills.

To further confirm the "brain drain" channel, I compare the fund performance change around a managerial turnover before versus after the shock. I find a turnover after the shock is followed by a significant decline in gross returns relative to peer funds, whereas before the shock, fund performance around a manager's departure does not change. This evidence suggests the regulatory

[2]Although some generic investment skills might apply in both QDII and non-QDII funds, it is generally not efficient for domestic private funds to "steal" QDII mutual fund managers and such cases are rarely seen.

change is associated with a structural break in the talent gap between departing and incoming managers, which is consistent with the brain drain in the mutual fund industry after the shock.

This paper is connected to two strands of literature. First, it complements the literature on human capital relocation across different sectors. To my best knowledge, this work is the first to document a causal relationship between inter-sector competition for talent and mutual fund performance. The closest paper is [90], which investigates the self-selection of mutual fund managers into hedge funds. My paper focuses on the consequence of talent stealing by the private fund industry on mutual fund performance. [91] finds younger mutual fund managers underperformed older ones during 1999–2005, a period during which the hedge fund industry grew rapidly. My paper links the competition from the private fund industry directly to mutual fund performance by exploiting the exogenous policy shock on the private fund industry and the cross-sectional variation in the exposure to the shock.

Second, this paper contributes to the debate about the role of managerial talent in the money management industry [92, 93, 94, 95, 96, 97, 98]. By showing evidence that fund managers do matter for performance, this paper helps address the puzzling lack of evidence concerning managerial skills.[3] Also, instead of inferring managerial skills from the returns, flow, or portfolio components, this paper takes a different approach by introducing a shock to the managerial talent and examining its impact on fund performance.

The rest of this paper is organized as follows. Section 2 introduces the empirical setting and the data; Section 3 discusses the research design and the empirical results; Section 4 concludes.

## 3.2 Empirical Setting and Data

### 3.2.1 The Regulatory Change and QDII Mutual Funds

I use the enactment of the *Amended Securities Investment Fund Law* as an exogenous shock to the inter-industry competition faced by mutual funds. As the first regulation on private funds (i.e., private equity, venture capital, and hedge funds) in China, the regulation is vital to stimulating the

[3]The underlying rationale is, if managerial skill does not matter, talent stealing should not affect fund performance.

development of the private fund industry by lowering barrier to entry.

The timeline of this regulatory change is as follows. Effective June 1, 2013, the Amendment legitimized private funds in China and laid out general industry principles. The principles cover a variety of aspects including the registration of private funds, filing requirements, accredited investors, and so on. On June 27, 2013, the State Commission Office for Public Sector Reform (SCOPSR) issued a notice that put private equity and venture capital under the supervision of the China Securities Regulatory Commission (CSRC). This notice ended the prolonged battle between the CSRC and the National Development and Reform Commission (NDRC) over the supervision of this industry. On February 7, 2014, the Asset Management Association of China (AMAC) issued a guideline called *Measures for the Registration and Archival Filing Regarding Private Fund Managers* to enforcethe Amendment. According to the *Measures*, all private fund management firms must register with AMAC and provide required information about the firm and its top executives.

This set of regulations, starting with the enacetment of the Amendment, considerably stimulated the private fund industry. Prior to this regulation, private funds that invested in publicly traded securities (i.e., hedge funds) were mostly banned, while underground hedge funds faced substantial legal risk regarding illegal fundraising. The only exception to this ban was several "sunshine private funds," which took the form of security investment trusts and were thus heavily regulated by the China Banking Regulatory Commission (CBRC). The trust industry is known for its high barrier to entry; the requirement for registered capital above RMB 300 million made sunshine private funds inaccessible for small players in the financial industry.[4]

In contrast, under the new Amendment, private funds could be started with registered capital of only RMB 10 million, which dramatically lowered the barrier to entry. The Amendment also streamlined the registration procedure for private funds: the AMAC must complete the registration within 20 business days of the registration request, which is substantially shorter than the time

[4]Moreover, historically, the government suppressed the trust industry and made it face great regulatory uncertainty. For example, the CBRC released a circular in August 2009, *Risk Alert on Trust Company Equity Trading Accounts*, which banned trusts from opening trading accounts and thus forced many trusts to shut down.

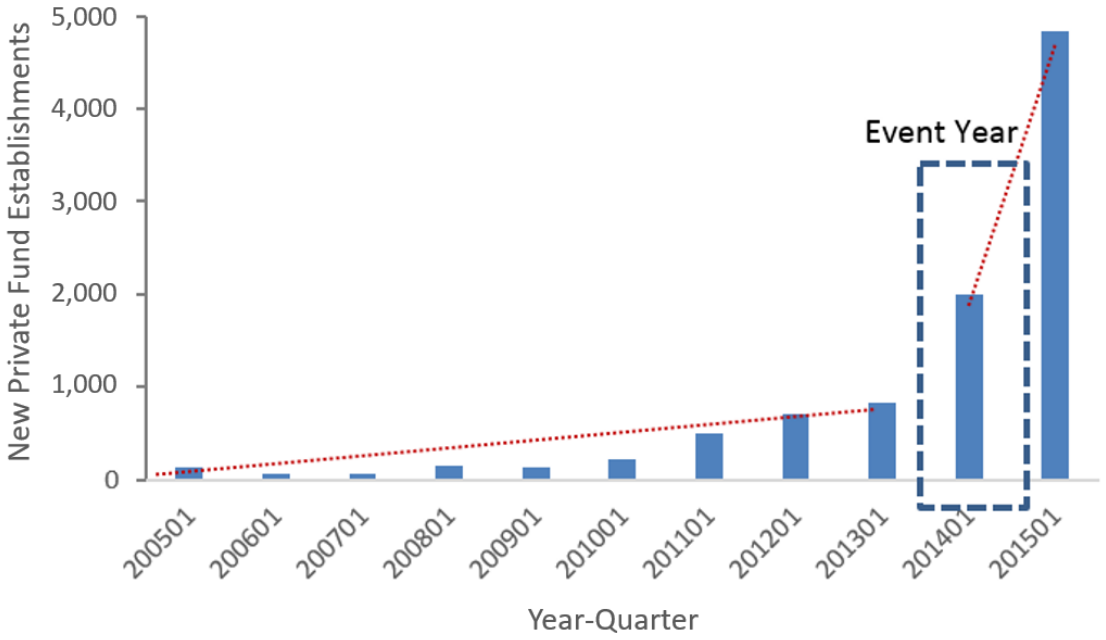



Figure 3.1: **Private fund establishments** This figure plots the annual number of new establishments in the private fund industry from 2004 to 2015. Each bar represents the number of new private fund management firms created during the 12 months by the end of the first quarter of a given year.

required to register a trust. Last but not least, the Amendment resolved the regulatory uncertainty arising from the long-time battle between the NDRC and the CSRC by putting the AMAC (delegated by CSRC) in charge of the private fund industry. This reallocaion of regulatory power paved the way for legal enforcement that would promote healthy competition in the industry.

Due to the lowered entry barrier and enhanced business environment in the private fund industry, an unprecedented boom occurred in the industry after the regulatory change. Figure 3.1 reports the number of new establishments in the private fund industry by year between 2004 and 2015. Each bar represents the number of new funds created during the 12 months leading up to March 31 each year. The number of new establishments significantly increased after the enactment of the Amendment. Considering that talent pools for private and mutual funds largely overlap, the private fund boom has the potential to increase the competition between the two industries, especially for talent.

Testing the impact of the intensified cross-sector competition on mutual funds requires a control group that is largely unaffected by the regulatory change. My baseline analysis uses Qualified

Domestic Institutional Investor (QDII) mutual funds as the control group. Unlike other types of mutual funds, QDII mutual funds (which mainly invest in overseas assets) do not face intensified competition from private funds, because private funds are not allowed to invest overseas. In China, people do not have free access to foreign financial securities and have to obtain QDII quotas from the State Administration of Foreign Exchange (SAFE) to invest in these securities. The majority of QDII quotas have been granted to large banks, mutual funds, and insurance companies; no private funds have received QDII quotas. This fact, together with other features I will discuss, makes QDII mutual funds a valid control group.

### 3.2.2 The Validity of the Empirical Setting

This study investigates how inter-industry competition for talent affects mutual fund performance. Toward that end, I use the Amendment enactment as an exogenous shock to the inter-industry competition faced by mutual funds and explore how the regulation differentially affects non-QDII and QDII mutual funds.

The validity of this empirical setting relies on the following assumptions. First, the regulatory change should intensify the intra-industry competition faced by mutual funds. Second, the shock should be exogenous to mutual fund performance and managerial turnover decisions. Third, the non-QDII and QDII mutual funds should not experience divergence in performance in the absence of the Amendment. I discuss the first two assumptions in this subsection and test the third assumption in Section 3.3.1.

The first assumption is likely to hold because my analysis shows that the regulation is associated with intensified competition for managerial talent. As reported in Table 3.2, the likelihood of managerial departure in mutual funds increases by more than 50% after the Amendment. The turnover rate becomes significantly higher across all types of mutual funds except for QDII funds, consistent with the assumption that the competition for talent becomes more severe for non-QDII funds but not for QDII funds.

In addition, anecdotal evidence shows that mutual fund managers migrated to private funds

after the shock. According to *the Wall Street Journal (WSJ)*, "Many experienced mutual-fund managers have left their jobs to start their own private-equity firms, which are more profitable," and "A lot of junior people were promoted to become fund managers." The underperformance of junior managers in the midst of the market fluctuation also received media attention.

The talent stealing hypothesis has an economic foundation. Compensation gaps in the two industries can potentially motivate the migration of capable mutual fund managers to private funds. Similar to the practice in the U.S. asset management industry, mutual fund managers' incomes largely depend on assets under management, while private fund managers' compensation primarily comes from the incentive fee. Typically, private fund managers are rewarded around 20% of excess fund returns, which can lead to the self-selection of managers with investment skills into private funds. The flexibility of private fund investment and the low requirement regarding portfolio diversification may also be appreciated, particularly by talented fund managers, who can use that flexibility to make profits.

Regarding the second assumption, the regulatory change is very likely to be exogenous to mutual fund performance and the career decisions of mutual fund managers. Since the Amendment was designed to regulate a separate sector than the mutual fund industry, the regulation is unlikely to be driven by the dynamics in the mutual funds. Fund returns are largely unpredictable and therefore unlikely to contribute to policy-making before the returns are realized. Similarly, individual fund managers are not expected to have the political power to influence regulation decisions. For the above reasons, I believe the regulatory change set forth by the Amendment is plausibly exogenous to mutual fund performance and managerial turnovers.

### 3.2.3 Data

I manually collect private fund establishment dates and geographic information from the AMAC's official website, on which all private fund management companies are required to register. The dataset includes 19,947 private fund management companies that registered by September 28, 2015. The information includes the incorporation addresses and the office addresses, number of

employees, dates of establishment and registration, fund type, information regarding top executives, etc.

The mutual fund information is obtained from the WIND Financial Dataset, one of the most used data vendors for financial and economic research in China. The dataset contains fund net values, expenses, returns, and fund manager information for each quarter. WIND compiles the data from quarterly, semiannual and annual financial reports of mutual funds. Importantly, this dataset also provides mutual funds' addresses, which enables me to link the mutual funds with the private fund management companies based on their geographic locations.

I obtain the main sample using the following filter. The sample focuses on actively managed mutual funds and does not include index funds, money market funds, or funds specializing in alternative assets. I also exclude the mutual funds that are in a class that does not contain any QDII funds. This filter yields a raw sample of 1,729 unique mutual funds. Further, I require the mutual funds to have full observations from the fourth quarter in 2012 to the third quarter in 2014, which reduces the sample to 1,004 mutual funds.[5]

Table 3.1 reports the summary statistics of the main sample. The dataset includes 12,799 fund-quarter observations spanning from 2012 to the second quarter of 2015. The average quarterly net return is 4.8% and the expense ratio is 1.1%. A fund charges 0.4% of assets under management (AUM) as the management fee, and the transaction costs average around 0.2% per quarter. Note that the turnover rate in the sample is higher than in the U.S. mutual fund industry. On average, a mutual fund experiences managerial turnover every 2.5 years.

## 3.3 Research Design and Empirical Result

### 3.3.1 Intensified Competition and Mutual Fund Performance

The objective of this paper is to understand how the inter-industry competition for talent affects mutual funds' performance. To do so, I investigate how the rising competition concomitant with the

[5]Notably, the majority of mutual funds in China follow mixed investment strategies and spread investment across different asset classes. The sample contains four classes of mutual funds (based on the primary classification): Equity, Bond, Mixed, and QDII.

Amendment affects mutual funds' performance before further examining whether the competition-performance relation arises from the talent stealing.

To test the impact of the intensified competition for fund performance, I examine how the Amendment affects the performance of QDII funds and non-QDII funds differently in a DID framework. As discussed above, the QDII mutual funds do not face intensified competition from private funds because private funds do not have access to QDII quotas and thus cannot invest overseas. Accordingly, I use non-QDII mutual funds as the treated group and QDII mutual funds as the control group. If the cross-industry competition negatively affects fund performance, I expect a decrease in excess returns for non-QDII funds after the regulatory change relative to QDII funds.

In particular, I estimate the following specification:

$$ExcessReturn_{i,t} = \beta * Treat_i * PostShock_t + \alpha_t + \alpha_i + \alpha_{j,t} + \varepsilon_{i,t}, \tag{3.1}$$

where $i$ indexes a mutual fund and $t$ indexes a quarter. $ExcessReturn$ is defined as gross returns minus the benchmark returns specified in the mutual funds' prospectuses. $Treat$ takes the value of 1 for non-QDII mutual funds and 0 for QDII mutual funds. $PostShock$ equals 1 for periods after the shock and 0 for periods before the shock. I focus on the comparison between QDII funds and non-QDII funds under the same fund management firm by including firm-quarter fixed effects $\alpha_{j,t}$ in the regression below. A negative estimate of $\beta$ would be consistent with a dampening effect of the increased competition on mutual fund performance.

The results are reported in colunms (1)–(3) in Table 3.3. $Treat$ takes the value of 1 for non-QDII mutual funds and 0 for QDII mutual funds. $Post$ is assigned the value of 1 for the periods after the shock and 0 for the periods before the shock. To avoid extreme market conditions, I focus on the sample from 2012 to 2014 and exclude the first two quarters of 2015 when many small-cap stocks had extreme returns. In columns (1)–(3), the outcome variable is excess returns, which is the return before expenses minus the passive benchmark. In columns (4)–(6), the dependent variable is percentage fund flow, as defined in Table 3.1. Columns (2) corresponds to Equation

3.1 ,while columns (1) and (3) do not include firm-quarter fixed effects. Column (1) is based on the full sample, while column (2) builds on the firms that are managing both QDII and non-QDII funds, and column (3) builds on the entire control group and the treated group matched with the control group.

The results in Table 3.3 are generally consistent with the predictions in Section 3.3.1. The key coefficient $Treat * Post$ loads negatively across columns (1)–(3), regardless of the samples and the specifications. This indicates that the increased competition stemming from the regulation shock adversely affects the performance of the treated funds as opposed to the control group. As shown in column (2), the results are robust to controlling for firm-level variation, which suggests the differential impacts of the Amendment on the QDII and non-QDII funds are not driven by the divergent trends in the fund management firms that provide non-QDII funds and those offering QDII funds.

One may be concerned that the decline in fund returns in the treated group might be driven by fund flows. There is overwhelming evidence that the mutual fund industry has a decreasing return to scale. If the turnover is taken positively by investors and the fund experiencing managerial turnovers had large inflows after a turnover, the decrease in returns would be expected even if the quality of managers does not deteriorate.

To address this concern, I repeat the above DID tests on fund flows and report the results in columns (4)–(6) of Table 3.3. The outcome variable is the fund flow of a mutual fund during a given quarter, which is defined as the percentage outflow relative to the net value at the beginning of the quarter. After the shock, the non-QDII mutual funds see slight outflows rather than inflows, which suggests the decline in returns does not arise from the change in fund flows. Overall, the above evidence suggests the intensified competition dampens mutual fund performance and the effect is unrelated to the change in fund flows.

A key assumption in the DID analysis is that the excess returns of the QDII funds and the non-QDII funds have parallel trends in the absence of the regulatory change. To verify whether the assumption holds, in Figure 3.2, I report how the returns gap between the treated and the

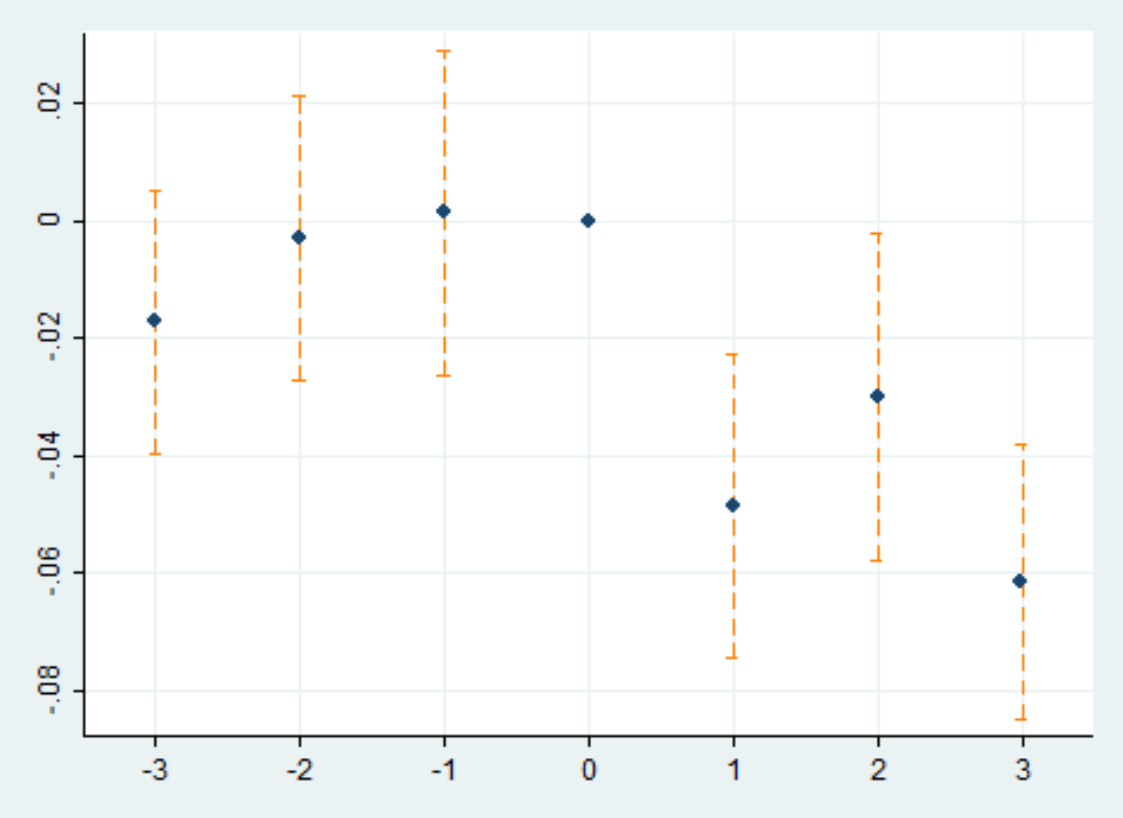



Figure 3.2: **Returns gap between domestic and QDII mutual funds** This figure shows how the returns gap between the treated group (non-QDII) and the control group (QDII) evolves over time. The horizontal axis is the timeline with quarter as the unit and the zero point refers to the third quarter of 2013. The vertical axis indicates the excess returns gap between non-QDII funds and QDII funds for each quarter, where excess returns are defined in Table 3.1. This plot is based on a within-management-firm regression similar to Equation 3.1 based on 648 funds.

control groups evolves . The horizontal axis is the quarter relative to the shock, and the zero point corresponds to the third quarter of 2013. The vertical axis indicates the excess returns gap between non-QDII funds and QDII funds. This plot corresponds to a within-firm regression similar to Equation 3.1 in a quarter-by-quarter style. The returns gap between QDII and non-QDII funds is insignificant for all the periods before the regulatory shock, which confirms the parallel-trend assumption. Note that all three point estimates after the shock are significantly negative, which lends further support to the negative effect of cross-industry competition on mutual fund returns.

### 3.3.2 Is the Performance Decline Attributable to Talent Stealing?

In this subsection, I further investigate the mechanism behind the baseline effect addressed above. I am particularly intersted in the mechanism regarding talent stealing. Due to the overlap of managerial skills desired by mutual funds and private funds, the development of the private fund industry may draw talent with return-generating skills away from the mutual fund industry and thus undermine the talent pool and the performance of mutual funds.

To test the talent-stealing hypothesis, I perform two sets of empirical analyses. First, I try to verify whether the Amendment indeed increases competition for talent by examining how the turnover rate changes in the treated group after the shock relative to the control group. In particular, I run the following regression:

$$Turnover_{i,t} = \beta * Treat_i * PostShock_t + \alpha_t + \alpha_i + \alpha_{j,t} + \varepsilon_{i,t} \tag{3.2}$$

All variables are as defined in Equation 3.1. A positive estimate of $\beta$ would be consistent with intensified competition for talent faced by mutual funds as a result of the Amendment.

To test the talent-stealing channel more directly, I exploit the cross-sectional variation in manager departures. If the brain drain in mutual funds explains the performance decline in the treated group, I would expect the competition-returns relation to be stronger in the period following a manager's departure. Since talent stealing is realized at the time of managerial turnovers, any performance deterioration should be concentrated in the firms that experienced turnovers after the regulatory change as opposed to the pre-shock period.[6]

In particular, I run the following regression:

$$\begin{aligned} ExcessReturn_{i,t} = {} & \beta_1 * D_{i,t}^{PostTurnover} * Treat_i * PostShock_t + \beta_2 * PostShock_t * D_{i,t}^{PostTurnover} \\ & + \beta_3 * PostShock_t * Treat_i + \beta_4 * D_{i,t}^{PostTurnover} * Treat_i + \alpha_t + \alpha_i + \alpha_{j,t} + \varepsilon_{i,t} \end{aligned}$$

[6]Since the talent stealing is realized only when a manager leaves a mutual fund, the competition-performance relationship should be particularly pronounced after a managerial turnover if the effect is driven by talent stealing. On the contrary, if the talent-stealing channel does not explain the effect, the Amendment's impact on fund performance should not vary with the presence of managerial turnovers.

$D^{PostTurnover}$ is a dummy equal to 1 if there is at least one "valid" turnover during the four quarters leading up to the current period. For the observations after the regulatory shock, a "valid" turnover must occur after the shock, whereas before the shock, any manager departure counts as a "valid" turnover. The other variables in Equation 3.3 remain their definitions in Equation 3.1. The coefficient $\beta_1$ should be significantly negative if the Amendment hurts mutual funds' performance by distracting their talented managers.

Table 3.4 reports the estimates of Equation 3.2. To reduce the impact of potential measurement errors on the exact timing of managerial turnovers, I exclude the turnovers occurring in Q3/2013 and Q4/2013. Columns (1) and (2) (columns (3) and (4)) are based on the sample through Q2/2015 (Q4/2014). Columns (3) and (4) build on the mutual funds offered by a firm that manages both QDII and non-QDII funds, while columns (1) and (2) are based on the full sample. Similarly, the quarter fixed effects and the fund fixed effects are included in all the specifications, and firm-by-quarter fixed effects are used in columns (3) and (4). All variables are as defined in Table 3.1. In all specifications, the coefficient before $Treat * PostShock$ is positively significant, which suggests the turnover rate in non-QDII funds increases significantly after the regulatory shock relative to the control group.

The result of the triple-differences analysis is reported in Table 3.5. Columns (1)–(3) focus on excess returns, and Columns (4)–(6) focus on fund flow. I include the quarter fixed effects and the fund fixed effects in all columns and include the firm-by-quarter fixed effects in columns (2) and (5). Columns (1) and (4) build on the full sample, while columns (2) and (5) (columns (3) and (6)) are based on the firms that manage both QDII and non-QDII funds (the matched treated and control groups).

The coefficients of $D^{PostTurnover} * Treat * PostShock$ are significantly negative for both excess return and fund flow regressions. This result implies that the increased competition arising from the regulatory shock is particularly harmful to the mutual funds when their managers depart.

Overall, the above findings are consistent with the hypothesis that the boom in the private fund industry dampens mutual funds' performance through intensifying competition for talent.

Revealing the talent-stealing channel also lends support to the relevance of managerial skills in the money management industry.

### 3.3.3 Further Evidence on Brain Drain

To further verify the talent-stealing hypothesis, I examine how the skill gap between departing managers and incoming managers changes before versus after the regulatory shock. In particular, I use the change in relative returns around a turnover as a proxy for the skill gap. If brain drain exists, I would expect the skill gap between departing and incoming managers to widen after the shock relative to the skill gap in the pre-shock period. In other words, in the post-shock period, a mutual fund's returns are expected to drop after a managerial turnover relative to the peer funds seeing no turnovers, whereas a turnover is not expected to be associated with any performance change in the pre-shock period.

In particular, I estimate the following specification:

$$Return_{i,t} = \alpha_i + \alpha_t + \beta * D_i^{turnover} * PostTurnover_{i,t} + \varepsilon_{i,t} \tag{3.3}$$

I estimate the above specification using the pre-shock period and the post-shock period respectively. The outcome variable is returns before management fees. On the right-hand side, $PostTurnover_{i,t}$ is a dummy variable equal to 1 (0) for the quarters after (before) the turnover. $D_i^{turnover}$ is a dummy indicating whether Fund $i$ belongs to the turnover or non-turnover groups. The turnover group is defined as the mutual funds experiencing manager departure, while the non-turnover group contains those not experiencing turnover and that match with the turnover group based on net values and returns. The sample period spans from three quarters before a turnover to three quarters after a turnover. If the intensified competition leads to a brain drain and weakens the talent pool in the mutual fund industry, I would expect $\beta$ to be significantly negative for the post-shock period and insignificant for the pre-shock period.

To visualize the evolution of mutual fund returns around the manager's departure, I run the

following regression:

$$Return_{i,t} = \alpha_i + \alpha_t + \sum_{k\in\{-3,-2,0,1,2,3\}} \beta_k * D_i^{turnover} * D_{i,t}^k + \varepsilon_{i,t}, \tag{3.4}$$

where $D_{i,t}^k$ is defined as a dummy variable taking the value of 1 if and only if quarter $t$ is the $k^{th}$ quarter after the turnover. If brain drain occurs after the shock in the mutual fund industry, I would predict $\beta_0$ $\beta_{-2}$ $\beta_{-3}$ to be insignificant for both the pre-shock and post-shock periods. $\beta_1$ $\beta_2$ $\beta_3$ are predicted to be negative for the post-shock subsample, and the post-turnover coefficients are predicted to be insignificant for the pre-shock period.

The results are reported in Table 3.6. Column (1) and column (3) correspond to Equation 3.3. In the post-shock regression, $D^{Turnover}$ is set to one if a mutual fund experienced a turnover during the first three quarters of 2014 and did not see other managerial departures in the seven quarters around the turnover. In the pre-shock regression, $D^{Turnover}$ is set to 1 for the mutual funds that experienced a turnover during the first two quarters of 2013 and no further managerial departures in the seven quarters around the turnover. $D^{Turnover}$ is set to 0 if a mutual fund does not see any turnovers in the seven quarters specified above and is matched with a mutual fund in the turnover group based on predetermined attributes such as net value and returns.

According to columns (1) and (3), the coefficient before $PostTurnover * D^{Turnover}$ is significantly negative after the shock and insignificant before the shock, which is consistent with the occurrence of brain drain in the mutual fund industry after the regulatory change.

Columns (2) and (4) of Table 3.6 report the estimation of Equation 3.4. For the post-shock analysis, the coefficients after a managerial turnover are negative,two of which are significant at the 5% level. In terms of magnitude, a fund seeing a turnover after the shock underperforms its peers by 2% per quarter during the three quarters after the turnover. By contrast, none of the coefficients are significant for the pre-shock analysis, which indicates a managerial turnover is not associated with a change in performance before the shock.

The above results are visualized in Figures 3.3 and 3.4, where each point corresponds to a

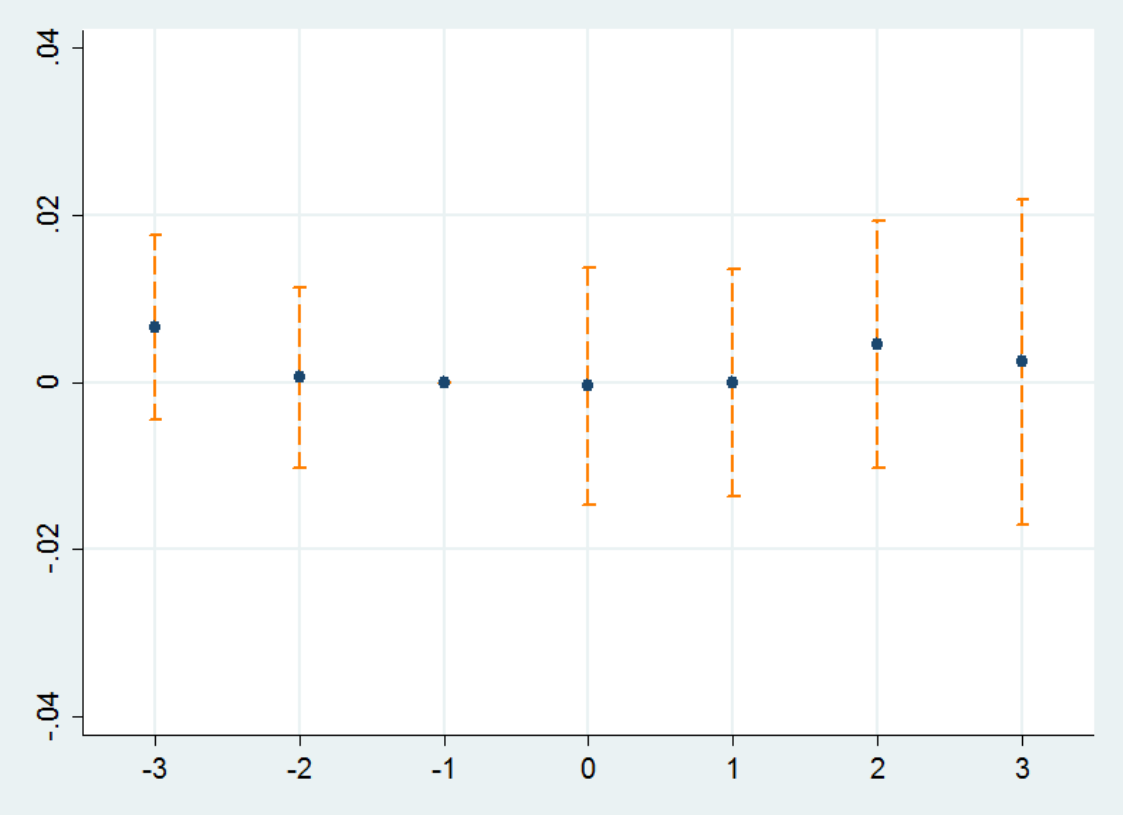



Figure 3.3: **Returns around manager turnover: before the regulatory shock** This plot exhibits the returns gap between the group with $D^{Turnover} = 1$ and the group with $D^{Turnover} = 0$ before the regulatory change. The horizontal axis indicates the timeline in quarters and the zero point represents the quarter of turnover. The blue dots represent returns before management fees of the $D^{Turnover} = 1$ group relative to the $D^{Turnover} = 0$ group, and dotted orange lines represent the 95% confidence interval of the coefficient estimate.

coefficient estimate in columns (2) and (4) of Table 3.6. The dotted orange lines represent the 95% confidence interval of the estimates. According to Figure 3.3, the returns of the fund experiencing a turnover relative to the matched peer funds remain unchanged around the turnover, whereas in Figure 3.4, the funds seeing a managerial departure significantly underperform after a turnover.

Overall, the above findings suggest the mutual fund industry is likely to experience a brain drain after the regulatory shock, and the deterioration in the talent pool is economically significant.

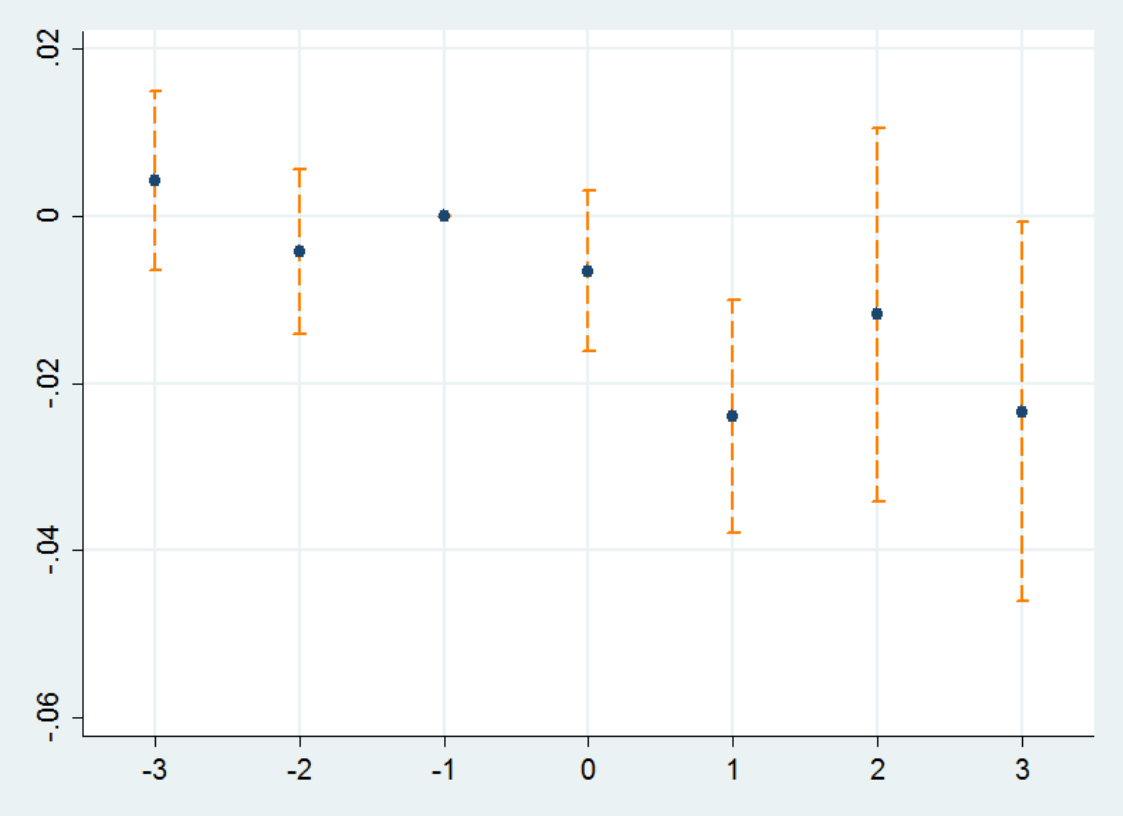



Figure 3.4: **Returns around manager turnover: after the regulatory shock** This plot exhibits the returns gap between the group with $D^{Turnover} = 1$ and the group with $D^{Turnover} = 0$ over time before the regulatory change. The horizontal axis indicates the timeline in quarters and the zero point represents the quarter of turnover. The blue dots show returns before management fees of the $D^{Turnover} = 1$ group relative to the $D^{Turnover} = 0$ group, and dotted orange lines represent the 95% confidence interval of the coefficient estimate.

### 3.3.4 Robustness Tests

**Alternative cross-sectional variation in regulation exposure**

To further confirm the causal inference of the above findings, I explore an alternative cross-sectional variation in the exposure to the shock: a mutual fund's proximity to private funds. In particular, in a framework similar to DID, I compare the performance of mutual funds neighboring a large number of private funds and those who neighbor few private funds.

Specifically, I sort the mutual funds by the number of private funds located within the same zip code by the end of 2011. I set $D^{HighExposure}$ to be 1 if a mutual fund falls in the top quintile, and assign $D^{HighExposure}$ as 0 for the mutual funds in the bottom two quintiles and that are matched with the high-exposure funds in terms of predetermined attributes such as net value, returns, and fund flow. The underlying assumption is private funds are more likely to steal talent from nearby mutual funds. The assumption is likely to hold because the proximity may help to overcome the information asymmetry and make it easier for the neighboring private funds to target the talented managers in their recruiting efforts. Moreover, the personal costs associated with changing jobs is likely to be low if the new and old employers are in the same area. Based on the above idea, I run the following regressions:

$$Turnover_{i,t} = \alpha_i + \alpha_t + \beta_1 * D^{HighExposure} * PostShock_{i,t} + \varepsilon_{i,t} \tag{3.5}$$

$$Return_{i,t} = \alpha_i + \alpha_t + \beta_1 * D^{HighExposure} * PostShock_{i,t} + \varepsilon_{i,t} \tag{3.6}$$

Equation 3.5 tests whether the regulatory shock accelerates talent outflow from mutual funds where $\beta_1$ is predicted to be positive. Equation 3.6 is designed to examine how the intensified competition affects fund performance where the outcome variable is returns before expenses as defined in Table 3.1. If the local competition between mutual funds and private funds dampens mutual fund performance and leads to brain drain in mutual funds, I would expect $D^{HighExposure} * PostShock_{i,t}$ to be significantly negative.

As discussed above, I use one-to-one matching between the high- and low-exposure funds to ensure the comparability of the high-exposure and low-exposure funds. I compare the attributes of high-exposure and low-exposure groups before the shock in Table 3.7. The two groups are comparable along all dimensions and do not exhibit a significant difference in any predetermined attributes.

Panel A of Table 3.8 reports the estimation of Equations 3.6 and 3.5. Like the specifications above, quarter fixed effects and fund fixed effects are included. The first column presents the regression of turnover on $PostShock * D^{HighExposure}$. One could decompose the turnover into two types, the regular turnover $T_r$ and talent-stealing turnover $T_s$. Based on the assumption that $T_r$ is orthogonal to the regulation, the coefficient of $PostShock * D^{HighExposure}$ can be interpreted as the association between $PostShock * D^{HighExposure}$ and the talent-stealing turnover $T_s$. In particular, the high-exposure mutual funds are 6.8% more likely to experience a talent-stealing turnover than the matched low-exposure funds, relative to their pre-shock turnover rate. Relative to the statistics in Table 3.1, the estimate translates to an 70% increase in the turnover rate after the shock.

Column (2) examines a mutual fund's returns before expenses, and column (3) focuses on the decile of returns before expenses.[7] The key coefficient before the interaction term is highly significant in both columns, which suggests that proximity to private funds results in underperformance of 1.2% per quarter relative to the pre-shock level. Panel B confirms the finding using an alternative proxy for exposure to the regulatory shock, where $\#PF\_decile_i$ is defined as the decile of the number of private funds in the same zip code as mutual fund $i$. Overall, the results show that the mutual funds neighboring private funds see a disproportionate decline in performance after the regulatory change, potentially connected to the talent-stealing channel.

One potential concern is the decline in returns might be driven by fund flows for the reasons discussed in Section 3.3.1. To address this concern, I investigate whether the high- and low-exposure funds diverge in the fund flows after the shock. The result is reported in columns (4) and

[7] I sort returns before expenses by quarter to reduce the noise in the returns data, given that the Chinese market experienced a highly volatile period starting from the end of 2014 that potentially introduced sizable noise into the data.

(5) of Table 3.8. The outcome variable is $Flow$ (defined in Table 3.1) in column (4) and decile rank of $Flow$ in column (5). In particular, I rank fund flow by quarter decile to reduce the effect of extreme values. The estimates of key coefficient are negative in both specifications and are significant at the 1% level for flow decile. The evidence suggests, after the shock, the decline in returns in the mutual funds that neighbor a large number of private funds is not driven by fund inflows and decreasing returns to scale. On the contrary, investors respond negatively to the talent stealing by withdrawing their mutual fund investment, which is consistent with the brain drain hypothesis.

**Further robustness tests**

In this subsection, I conduct a series of analyses to check the robustness of the baseline result in Sections 3.3.1 and A.1.2. First, I examine whether the results are robust to using alternative measures of turnover. Since many mutual funds have more than one manager, a managerial turnover is expected to induce a greater effect on the mutual funds with fewer managers. Accordingly, I define *Turnover2* as the number of departing managers divided by the total number of managers serving the same fund at the time of the turnover. I repeat the analysis in Table 3.5 with the alternative measure of turnover and report the result in Table 3.10.

For a given quarter within one year after the shock, $D^{PostTurnover2}$ is defined as the highest value of $Turnover2$ between the shock and the given quarter, while $D^{PostTurnover2}$ is defined as the highest value of $Turnover2$ in the previous four quarters of a given quarter, otherwise. Similarly, I obtain significantly negative estimates of the coefficient on $D^{PostTurnover2} * Treat * PostShock$ in the return regressions and negative estimates in the flow regressions. Consistent with Table 3.5, the finding further confirms the inter-sector competition for talent leads to the performance decline in affected mutual funds.

Furthermore, I am interested in whether this result is robust to using alternative return measures, and in particular, returns before management fees. Since management fees are used to compensate the fund managers on top of the net returns earned by investors, the sum of the management fee

and the net returns conceptually capture a manager's overall skill in generating returns. Table 3.9 reports the results of the robustness test based on the alternative measure of fund returns. Columns (1)–(3) match columns (1)–(3) in Table 3.5 and columns (4)–(6) mirror Table 3.3. The coefficient estimates in columns (1)–(3) are close to those in columns (1)–(3) of Table 3.5. The finding suggests the baseline result is robust to the alternative returns measure. Similarly, the key coefficient remains significantly negative in Columns (4)–(6), and the estimates are comparable to those in Table 3.3.

## 3.4 Conclusion

This paper provides evidence that inter-industry competition negatively affects mutual fund performance through the stealing of talented mutual fund managers. I try to establish causality using a regulatory change in China as an exogenous shock on the deveopment of the private fund industry. The regulatory change accelerates business entry into the private fund industry and intensifies the competition faced by a subset of mutual funds. A DID analysis shows that the excess returns of the treated funds declines after the Amendment relative to the control group. I conduct various tests to confirm that this result does not merely capture the diverging trends in the two groups of mutual funds. Further, using a triple differences design, I find the competition effect is more salient at a manager's departure, and the magnitude of the effect is twice as large as in other periods. This evidence is consistent with the notion that increased competition exerts an adverse effect on fund performance through the talent–stealing channel. The performance gap between departing and incoming managers opens up after the regulatory shock, which suggests that intensified competition creates a brain drain in the mutual fund industry by stealing capable managers and weakening the talent pool. Overall, this paper not only shows the dampening effect of inter-industry competition on mutual fund performance, but also underscores the importance of managerial talent in the money management industry.

Table 3.1: **Summary Statistics.** This table presents the summary statistics for all the explanatory variables and outcome variables used in this paper. Expense rate is defined as expenses divided by the fund net value at the beginning of the period, management fee ratio, sales expense ratio, trading expense ratio, and custodian fee ratio are defined as the corresponding expense divided by the fund net value at the beginning of the period, respectively. $Flow$ is defined as $(NetValue_t + Expense_t - NetValue_{t-1} * GrossReturn_t)/NetValue_{t-1}$. $Turnover$ is a dummy taking the value of 1 if there is a manager departure in period $t$ and 0 if there is not. Relative return is defined as the benchmark returns deducted from gross returns. All variables are winsorized for 0.5% at each side.

| | Obs | Mean | Std. Dev. | 0.5% percentile | 99.5% percentile |
|---|---|---|---|---|---|
| Net returns | 12,834 | 0.048 | 0.093 | -0.187 | 0.446 |
| Expense rate | 12,799 | 0.011 | 0.013 | 0.002 | 0.166 |
| Management fee ratio | 12,788 | 0.004 | 0.003 | 0.001 | 0.051 |
| Sales expense ratio | 3,363 | 0.001 | 0.001 | 0.000 | 0.009 |
| Trading expense ratio | 12,797 | 0.002 | 0.002 | 0.000 | 0.018 |
| Custodian fee ratio | 12,799 | 0.001 | 0.001 | 0.000 | 0.015 |
| Returns before expense | 12,799 | 0.058 | 0.094 | -0.184 | 0.612 |
| Returns before management fee | 12,788 | 0.052 | 0.094 | -0.185 | 0.497 |
| Flow | 12,613 | 0.033 | 0.840 | -0.836 | 9.851 |
| Net Value | 12,834 | 1,574 | 2,403 | 6 | 17,622 |
| Manager Turnover | 12,834 | 0.095 | 0.293 | 0.000 | 1.000 |
| Excess returns | 12,702 | 0.029 | 0.085 | -0.476 | 0.972 |

Table 3.2: **The Effect of the Regulatory Shock on Managerial Turnover.** *PostShock* is a dummy variable indicating whether a given quarter is after the regulatory change. The classes are based on the official primary classification for each mutual fund. Turnover is a dummy variable equal to 1 when a manager departs during the quarter. Fund fixed effects are included in each specification. Standard errors clustered at the fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | Full Sample | Class= Bond | Class= Equity | Class= Mix | Class= QDII |
|---|---|---|---|---|---|
| | Turnover | Turnover | Turnover | Turnover | Turnover |
| PostShock | 0.0572*** | 0.0592*** | 0.0495** | .0646*** | -0.0056 |
| | [0.0049] | [0.0079] | [0.0219] | [0.0070] | [0.0150] |
| Fund FE | Yes | Yes | Yes | Yes | Yes |
| Clustered SE | Yes | Yes | Yes | Yes | Yes |
| # Observations | 11655 | 3836 | 666 | 6358 | 795 |
| # Funds | 1004 | 337 | 59 | 541 | 70 |
| R-squared | 0.0846 | 0.0973 | 0.0630 | 0.0763 | 0.0907 |

Table 3.3: **DID Analysis: Non-QDII versus QDII Mutual Funds.** $Treat$ is a dummy equal to 1 if the mutual fund is non-QDII and 0 if it is a QDII mutual fund. $PostShock$ is a dummy indicating whether it is after the regulatory change. The outcome variable in columns (1)–(3) is Excess Returns, benchmark returns deducted from returns before expenses.The outcome variable in columns (4)–(6) is Flow, as defined in Table 3.1. Fund fixed effects and quarter fixed effects are included in all specifications. Firm-quarter fixed effects are included in columns (2) and (5). Standard errors clustered at fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | (1) Returns | (2) Returns | (3) Returns | (4) Flow | (5) Flow | (6) Flow |
|---|---|---|---|---|---|---|
| Treat*PostShock | -0.032*** | -0.032*** | -0.023*** | -0.006 | -0.013 | -0.035 |
| | [0.003] | [0.003] | [0.003] | [0.032] | [0.035] | [0.040] |
| Quarterly FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mng Firm Qrt FE | No | Yes | No | No | Yes | No |
| Clustered SE | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 11573 | 7546 | 2679 | 11431 | 7482 | 2632 |
| # Funds | 996 | 648 | 227 | 1003 | 654 | 226 |

Table 3.4: **How Does the Amendment Affect Turnover for Non-QDII and QDII Funds?** $Treat$ is a dummy variable equal to 1 if the mutual fund is non-QDII and 0 if it is a QDII mutual fund. $PostShock$ is a dummy variable indicating whether a given quarter is after the regulatory change. The outcome variable is $Turnover$, a dummy variable equal to 1 if a manager departs. Fund fixed effects and quarter fixed effects are included in all specifications. Firm-quarter fixed effects are included in columns (3) and (4). Standard errors clustered at the fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Turnover | Turnover | Turnover | Turnover |
| Treat*PostShock | 0.035** | 0.065*** | 0.036** | 0.068*** |
| | [0.017] | [0.016] | [0.018] | [0.016] |
| Quarterly FE | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes |
| Mng Firm Qrt FE | No | No | Yes | Yes |
| Clustered SE | Yes | Yes | Yes | Yes |
| # Observations | 9685 | 11655 | 6338 | 7636 |
| # Funds | 1004 | 1004 | 655 | 655 |

Table 3.5: **Triple-Differences Analysis: the Differential Effect of Turnover on QDII Funds and Domestic Funds Around the Regulatory Change.** $Treat$ is a dummy variable equal to 1 if the mutual fund is non-QDII and 0 if it is a QDII mutual fund. $D^{PostTurnover}$ is a dummy equal to 1 if there is at least one "valid" turnover during the four quarters leading up to the current period. For the observations after the regulatory shock, a valid turnover must occur after the shock, whereas before the shock, any manager departure is a valid turnover. The outcome variable in columns (1)–(3) is $ExcessReturn$, benchmark returns deducted from returns before expenses. The outcome variable in columns (4)–(6) is flow, as defined in Table 3.1. Fund fixed effects, quarter fixed effects are included in all specifications. Firm-quarter fixed effects are included in Columns (2) and (5). Standard errors clustered at the fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | (1) Returns | (2) Returns | (3) Returns | (4) Flow | (5) Flow | (6) Flow |
|---|---|---|---|---|---|---|
| $D^{PostTurnover}$* | -0.024*** | -0.022** | -0.024** | -0.049 | -0.147 | -0.141 |
| Treat*PostShock | [0.008] | [0.011] | [0.009] | [0.086] | [0.093] | [0.104] |
| $D^{PostTurnover}$*PostShock | 0.017** | 0.018* | 0.013** | 0.022 | 0.094 | 0.038 |
| | [0.007] | [0.010] | [0.006] | [0.077] | [0.082] | [0.078] |
| $D^{PostTurnover}$*Treat | 0.006*** | 0.004** | 0.002 | 0.045* | 0.047** | 0.037 |
| | [0.002] | [0.002] | [0.002] | [0.023] | [0.024] | [0.043] |
| Treat*PostShock | -0.017*** | -0.018*** | -0.011** | 0.005 | 0.015 | -0.001 |
| | [0.004] | [0.004] | [0.005] | [0.033] | [0.038] | [0.045] |
| Quarterly FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mng Firm Qrt FE | No | Yes | No | No | Yes | No |
| Clustered SE | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 11573 | 7546 | 2679 | 11431 | 7482 | 2632 |
| # Funds | 996 | 648 | 227 | 1003 | 654 | 226 |

Table 3.6: **Mutual Fund Returns Around Managerial Turnovers.** Columns (1) and (3) correspond to Equation 3.3, while columns (2) and (4) to Equation 3.4. The dependent variable is returns before management fee for mutual funds. $PostTurnover$ is a dummy variable equal to 1 for the three quarters after a manager turnover, and $D^{Turnover}$ equals to 1 with a manager turnover during the sample period. Here $D_i$ is a dummy variable indicating whether a given quarter is the $i^{th}$ quarter after a manager turnover. Quarter fixed effects and fund fixed effects are included in each specification. Standard errors clustered at the fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | Gross Returns | Gross Returns | Gross Returns | Gross Returns |
|---|---|---|---|---|
| | Pre-Shock | Pre-Shock | Post-Shock | Post-Shock |
| $PostTurnover*$ | 0.0007 | | -0.0172*** | |
| $D^{Turnover}$ | [0.0048] | | [0.0067] | |
| $D^1 * D^{Turnover}$ | | -0.0001 | | -0.0240*** |
| | | [0.0068] | | [0.0070] |
| $D^2 * D^{Turnover}$ | | 0.0045 | | -0.0118 |
| | | [0.0075] | | [0.0114] |
| $D^3 * D^{Turnover}$ | | 0.0024 | | -0.0234** |
| | | [0.0098] | | [0.0115] |
| $D^0 * D^{Turnover}$ | | -0.0005 | | -0.0066 |
| | | [0.0072] | | [0.0049] |
| $D^{-2} * D^{Turnover}$ | | 0.0005 | | -0.0043 |
| | | [0.0055] | | [0.0050] |
| $D^{-3} * D^{Turnover}$ | | 0.0065 | | 0.0042 |
| | | [0.0056] | | [0.0054] |
| Quarter FE | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes |
| Clustered SE | Yes | Yes | Yes | Yes |
| # Observations | 2451 | 2451 | 1418 | 1418 |
| # Funds | 346 | 346 | 202 | 202 |

Table 3.7: **Robustness Test: Comparison of High- and Low-Exposure Mutual Funds.** All variables are as defined in Table 3.1 and are predetermined before the shock. I define $D^{HighExposure}$ as a dummy variable equal to 1 if the number of private funds sharing a zip code with the mutual fund falls in the top quintile. $D^{HighExposure}$ is assigned 0 for the mutual funds in the bottom two quintiles and and that match with the high-exposure mutual funds with respect to fund size, returns, and fund flow. The third column presents the differences between the two groups in the variables shown in the first column. Standard errors of the estimated differences are listed in the fourth column. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | $D^{HighExposure} = 1$ | $D^{HighExposure} = 0$ | Diff. | Std. Err. |
|---|---|---|---|---|
| Net value | 1246 | 1467 | -217 | [207.2] |
| Return before management fee | 0.023 | 0.021 | 0.002 | [0.002] |
| Return before expense | 0.028 | 0.026 | 0.002 | [0.002] |
| Turnover | 0.068 | 0.083 | -0.015 | [0.013] |
| Fund age (in sample) | 2.368 | 2.375 | -0.007 | [0.067] |
| Expense rate | 0.009 | 0.009 | -0.0004 | [0.0005] |
| Management fee ratio | 0.004 | 0.004 | -0.0002 | [0.00014] |

Table 3.8: **Robustness Test: the Effect on Mutual Funds Across Proximity to Hedge Funds.** I define $D^{HighExposure}$ as a dummy variable equal to 1 if the number of private funds having the same zip code as a mutual fund falls in the top quintile, while $D^{HighExposure}$ is assigned 0 for the mutual funds in the bottom two quintiles and that match with the high-exposure mutual funds with respect to fund size, return, and fund flow. $\#PF\_decile$ is the decile rank of private fund counts in the same zip code area as a mutual fund by the end of 2011. $PostShock$ retains the definition in Table 3.10. In column (1) The outcome variable is $Turnover$, as defined in Table 3.1. In column (2) the outcome variable is mutual fund's returns before expenses and in column (3) it is decile rank of returns before expenses.The outcome variable in column (4) is $Flow$, as defined in Table 3.1, and in dolumn (5) it is decile rank of of $Flow$. Fund fixed effects and quarter fixed effects are included in all specifications. Standard errors clustered at the fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | (1) Turnover | (2) Return | (3) Return Decile | (4) Flow | (5) Flow Decile |
|---|---|---|---|---|---|
| Panel A: cross-section variation on $D^{HighExposure}$ | | | | | |
| PostShock*$D^{HighExposure}$ | 0.068*** | -0.012** | -0.642*** | -0.062 | -0.812*** |
| | [0.018] | [0.006] | [0.212] | [0.125] | [0.253] |
| Quarterly FE | Yes | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes | Yes |
| Clustered SE | Yes | Yes | Yes | Yes | Yes |
| # Observations | 3494 | 3481 | 3481 | 3425 | 3425 |
| # Funds | 323 | 323 | 323 | 323 | 323 |
| Panel B: cross-section variation on $\#PF\_decile$ | | | | | |
| PostShock*$\#PF\_decile$ | 0.009*** | -0.0015* | -0.079*** | -0.014 | -0.127*** |
| | [0.003] | [0.001] | [0.030] | [0.019] | [0.036] |
| Quarterly FE | Yes | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes | Yes |
| Clustered SE | Yes | Yes | Yes | Yes | Yes |
| # Observations | 3494 | 3481 | 3481 | 3425 | 3425 |
| # Funds | 323 | 323 | 323 | 323 | 323 |

Table 3.9: **Robustness Test: Alternative Measures of Excess Returns.** $Treat$ is a dummy variable equal to 1 if the mutual fund is non-QDII and 0 if it is a QDII mutual fund. $PostShock$ is a dummy variable indicating whether a given quarter after the regulatory change. $D^{PostTurnover}$ is a dummy equal to 1 if there is at least one "valid" turnover during the four quarters leading up to the current period. For the observations after the regulatory shock, a valid turnover must occur after the shock, whereas before the shock, any manager departure is a valid turnover. The outcome variable in columns (1)–(6) is $ExcessReturn$, which is benchmark returns deducted from returns before mananagement fees. Fund fixed effects and quarter fixed effects are included in all specifications. Firm-quarter fixed effects are included in columns (2) and (5). Standard errors clustered at the fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | (1) Return2 | (2) Return2 | (3) Return2 | (4) Return2 | (5) Return2 | (6) Return2 |
|---|---|---|---|---|---|---|
| $D^{PostTurnover} * Treat * PostShock$ | -0.024*** | -0.019* | -0.023** | | | |
| | [0.009] | [0.011] | [0.010] | | | |
| $D^{PostTurnover} * PostShock$ | 0.014* | 0.014 | 0.012* | | | |
| | [0.008] | [0.010] | [0.007] | | | |
| $D^{PostTurnover}$*Treat | 0.004** | 0.001 | 0.000 | | | |
| | [0.002] | [ 0.002] | [0.002] | | | |
| Treat*PostShock | -0.031*** | -0.032*** | -0.022*** | -0.036*** | -0.035*** | -0.026*** |
| | [0.003] | [0.004] | [0.004] | [0.003] | [0.003] | [0.003] |
| Quarterly FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mng Firm Qrt FE | No | Yes | No | No | Yes | No |
| Clustered SE | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 11561 | 7534 | 2667 | 11561 | 7534 | 2667 |
| # Funds | 995 | 647 | 226 | 995 | 647 | 226 |

Table 3.10: **Robustness Test: Alternative Measures of Managerial Turnover.** 1 $Treat$ is a dummy variable equal to 1 if the mutual fund is non-QDII and 0 if it is a QDII mutual fund. $PostShock$ is a dummy variable indicating whether a given quarter is after the regulatory change. For the observations before the shock, $D^{PostTurnover2}$ equals the highest $Turnover2$ over the four quarters leading up to the current one, whereas for observation after the shock, $D^{PostTurnover2}$ equals the highest $Turnover2$ over the four quarters leading up to the current one and the turnover occurs after the shock. The outcome variable in columns (1)–(3) is $ExcessReturn$, which is benchmark returns deducted from returns before expenses.The outcome variable in columns (4)–(6) is Flow, as defined in Table 3.1. Fund fixed effects and quarter fixed effects are included in all specifications. Firm-quarter fixed effects are included in columns (2) and (5). Standard errors clustered at the fund level are reported in brackets. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

| | (1) Returns | (2) Returns | (3) Returns | (4) Flow | (5) Flow | (6) Flow |
|---|---|---|---|---|---|---|
| $D^{PostTurnover2}$* | -0.026*** | -0.027** | -0.024* | -0.087 | -0.190 | -0.312 |
| Treat*PostShock | [0.010] | [0.014] | [0.013] | [0.115] | [0.125] | [0.139] |
| $D^{PostTurnover2}$*PostShock | 0.011 | 0.013 | 0.011 | 0.052 | 0.118 | 0.070 |
| | [0.008] | [0.011] | [0.007] | [0.104] | [0.109] | [0.105] |
| $D^{PostTurnover2}$*Treat | 0.007*** | 0.005 | 0.002 | 0.038 | 0.030 | 0.032 |
| | [0.002] | [0.003] | [0.003] | [0.028] | [0.028] | [0.052] |
| Treat*PostShock | -0.028*** | -0.028*** | -0.019** | -0.008 | -0.015 | -0.000 |
| | [0.003] | [0.003] | [0.004] | [0.032] | [0.037] | [0.043] |
| Quarterly FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mng Firm Qrt FE | No | Yes | No | No | Yes | No |
| Clustered SE | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 11561 | 7546 | 2679 | 11431 | 7482 | 2632 |
| # Funds | 995 | 648 | 227 | 1003 | 654 | 226 |

# References


[1] J. A. Schumpeter, *Capitalism, socialism and democracy*. routledge, 1943.

[2] J. N. Baron, M. D. Burton, and M. T. Hannan, "The road taken: Origins and evolution of employment systems in emerging companies," *Industrial and Corporate Change*, vol. 5, no. 2, pp. 239–275, 1996.

[3] J. N. Baron, M. T. Hannan, and M. D. Burton, "Labor pains: Change in organizational models and employee turnover in young, high-tech firms," *American journal of sociology*, vol. 106, no. 4, pp. 960–1012, 2001.

[4] N. Wasserman, *The founder's dilemmas: Anticipating and avoiding the pitfalls that can sink a startup*. Princeton University Press, 2012.

[5] J. Choi, N. Goldschlag, J. C. Haltiwanger, and J. D. Kim, "The post entry growth and survival of business startups: The role of founding teams," 2019.

[6] T. E. Stuart and O. Sorenson, "Social networks and entrepreneurship," in *Handbook of entrepreneurship research*, Springer, 2005, pp. 233–252.

[7] M. Roach and H. Sauermann, "Founder or joiner? the role of preferences and context in shaping different entrepreneurial interests," *Management Science*, vol. 61, no. 9, pp. 2160–2184, 2015.

[8] A. Schoar, "The divide between subsistence and transformational entrepreneurship," *Innovation policy and the economy*, vol. 10, no. 1, pp. 57–81, 2010.

[9] S. Kerr, W. R. Kerr, and R. Nanda, "House money and entrepreneurship," National Bureau of Economic Research, Tech. Rep., 2015.

[10] E. Hurst and A. Lusardi, "Liquidity constraints, household wealth, and entrepreneurship," *Journal of political Economy*, vol. 112, no. 2, pp. 319–347, 2004.

[11] W. R. Kerr and R. Nanda, "Democratizing entry: Banking deregulations, financing constraints, and entrepreneurship," *Journal of Financial Economics*, vol. 94, no. 1, pp. 124–149, 2009.

[12] M. C. Schmalz, D. A. Sraer, and D. Thesmar, "Housing collateral and entrepreneurship," *The Journal of Finance*, vol. 72, no. 1, pp. 99–132, 2017.

[13] M. Adelino, A. Schoar, and F. Severino, "House prices, collateral, and self-employment," *Journal of Financial Economics*, vol. 117, no. 2, pp. 288–306, 2015.

[14] S. Corradin and A. Popov, "House prices, home equity borrowing, and entrepreneurship," *The Review of Financial Studies*, vol. 28, no. 8, pp. 2399–2428, 2015.

[15] L. Klapper, L. Laeven, and R. Rajan, "Entry regulation as a barrier to entrepreneurship," *Journal of financial economics*, vol. 82, no. 3, pp. 591–629, 2006.

[16] T. Rice and P. E. Strahan, "Does credit competition affect small-firm finance?" *The Journal of Finance*, vol. 65, no. 3, pp. 861–889, 2010.

[17] P. Ouimet and R. Zarutskie, "Who works for startups? the relation between firm age, employee age, and growth," *Journal of financial Economics*, vol. 112, no. 3, pp. 386–407, 2014.

[18] H. Sauermann, "Fire in the belly? employee motives and innovative performance in start-ups versus established firms," *Strategic Entrepreneurship Journal*, vol. 12, no. 4, pp. 423–454, 2018.

[19] A. Coad, K. Nielsen, and B. Timmermans, "My first employee: An empirical investigation," *Small Business Economics*, vol. 48, no. 1, pp. 25–45, 2017.

[20] S. Agarwal, C. Liu, and N. S. Souleles, "The reaction of consumer spending and debt to tax rebates—evidence from consumer credit data," *Journal of political Economy*, vol. 115, no. 6, pp. 986–1019, 2007.

[21] D. S. Johnson, J. A. Parker, and N. S. Souleles, "Household expenditure and the income tax rebates of 2001," *American Economic Review*, vol. 96, no. 5, pp. 1589–1610, 2006.

[22] D. Card, R. Chetty, and A. Weber, "Cash-on-hand and competing models of intertemporal behavior: New evidence from the labor market," *The Quarterly journal of economics*, vol. 122, no. 4, pp. 1511–1560, 2007.

[23] J. Eeckhout and A. Sepahsalari, "The effect of asset holdings on worker productivity," *Forthcoming, Review of Economic Studies*, 2018.

[24] MunichRe, *Topics Geo – Annual Review: Natural catastrophes 2017*. Munich Reinsurance Company, Munich, Germany, 2018.

[25] NOAA, *Billion-Dollar Weather and Climate Disasters: Overview*. National Oceanic and Atmospheric Administration, United States Department of Commerce, 2019.

[26] S. T. Bharath and D. Cho, "Ephemeral experiences, long-lived impact: Disasters and portfolio choice," *Working paper*, 2018.

[27] O. Dessaint and A. Matray, "Do managers overreact to salient risks? evidence from hurricane strikes," *Journal of Financial Economics*, vol. 126, no. 1, pp. 97–121, 2017.

[28] G. Bernile, V. Bhagwat, and P. R. Rau, "What doesn't kill you will only make you more risk-loving: Early-life disasters and ceo behavior," *The Journal of Finance*, vol. 72, no. 1, pp. 167–206, 2017.

[29] M. Kirchberger, "Natural disasters and labor markets," *Journal of Development Economics*, vol. 125, pp. 40–58, 2017.

[30] A. R. Belasen and S. W. Polachek, "How hurricanes affect wages and employment in local labor markets," *American Economic Review*, vol. 98, no. 2, pp. 49–53, 2008.

[31] R. Hornbeck and S. Naidu, "When the levee breaks: Black migration and economic development in the american south," *American Economic Review*, vol. 104, no. 3, pp. 963–90, 2014.

[32] R. Levine and Y. Rubinstein, "Smart and illicit: Who becomes an entrepreneur and do they earn more?" *The Quarterly Journal of Economics*, vol. 132, no. 2, pp. 963–1018, 2017.

[33] T. Babina, "Destructive creation at work: How financial distress spurs entrepreneurship," *Available at SSRN 2692841*, 2015.

[34] K. Herkenhoff, G. Phillips, and E. Cohen-Cole, "How credit constraints impact job finding rates, sorting & aggregate output," National Bureau of Economic Research, Tech. Rep., 2016.

[35] L. Page, D. A. Savage, and B. Torgler, "Variation in risk seeking behaviour following large losses: A natural experiment," *European Economic Review*, vol. 71, pp. 121–131, 2014.

[36] J. Hombert, A. Schoar, D. Sraer, and D. Thesmar, "Can unemployment insurance spur entrepreneurial activity?" National Bureau of Economic Research, Tech. Rep., 2014.

[37] J. Miao and N. Wang, "Investment, consumption, and hedging under incomplete markets," *Journal of Financial Economics*, vol. 86, no. 3, pp. 608–642, 2007.

[38] C. Wang, N. Wang, and J. Yang, "Optimal consumption and savings with stochastic income and recursive utility," *Journal of Economic Theory*, vol. 165, pp. 292–331, 2016.

[39] D. S. Evans and B. Jovanovic, "An estimated model of entrepreneurial choice under liquidity constraints," *Journal of political economy*, vol. 97, no. 4, pp. 808–827, 1989.

[40] R. E. Kihlstrom and J.-J. Laffont, "A general equilibrium entrepreneurial theory of firm formation based on risk aversion," *Journal of political economy*, vol. 87, no. 4, pp. 719–748, 1979.

[41] B. S. Frey, M. Benz, and A. Stutzer, "Introducing procedural utility: Not only what, but also how matters," *Journal of Institutional and Theoretical Economics (JITE)/Zeitschrift für die gesamte Staatswissenschaft*, pp. 377–401, 2004.

[42] E. Hurst and B. W. Pugsley, "What do small businesses do?" National Bureau of Economic Research, Tech. Rep., 2011.

[43] T. Åstebro and P. Thompson, "Entrepreneurs, jacks of all trades or hobos?" *Research policy*, vol. 40, no. 5, pp. 637–649, 2011.

[44] B. H. Hamilton, "Does entrepreneurship pay? an empirical analysis of the returns to self-employment," *Journal of Political economy*, vol. 108, no. 3, pp. 604–631, 2000.

[45] D. Dutta, S. Herath, and K. Musiake, "A mathematical model for flood loss estimation," *Journal of hydrology*, vol. 277, no. 1-2, pp. 24–49, 2003.

[46] S. DellaVigna and J. M. Pollet, "Investor inattention and friday earnings announcements," *The Journal of Finance*, vol. 64, no. 2, pp. 709–749, 2009.

[47] S. A. Corwin and J. F. Coughenour, "Limited attention and the allocation of effort in securities trading," *The Journal of Finance*, vol. 63, no. 6, pp. 3031–3067, 2008.

[48] L. Peng and W. Xiong, "Investor attention, overconfidence and category learning," *Journal of Financial Economics*, vol. 80, no. 3, pp. 563–602, 2006.

[49] D. Hirshleifer, S. S. Lim, and S. H. Teoh, "Limited investor attention and stock market misreactions to accounting information," *The Review of Asset Pricing Studies*, vol. 1, no. 1, pp. 35–73, 2011.

[50] T. Odean, "Do investors trade too much?" *The American economic review*, vol. 89, no. 5, pp. 1279–1298, 1999.

[51] B. M. Barber and T. Odean, "Trading is hazardous to your wealth: The common stock investment performance of individual investors," *The journal of Finance*, vol. 55, no. 2, pp. 773–806, 2000.

[52] R. M. Edelen, "Investor flows and the assessed performance of open-end mutual funds," *Journal of Financial Economics*, vol. 53, no. 3, pp. 439–466, 1999.

[53] J. Coval and E. Stafford, "Asset fire sales (and purchases) in equity markets," *Journal of Financial Economics*, vol. 86, no. 2, pp. 479–512, 2007.

[54] J. C. Stein, "Why are most funds open-end? competition and the limits of arbitrage," *The Quarterly journal of economics*, vol. 120, no. 1, pp. 247–272, 2005.

[55] B. M. Barber and T. Odean, "Boys will be boys: Gender, overconfidence, and common stock investment," *The quarterly journal of economics*, vol. 116, no. 1, pp. 261–292, 2001.

[56] M. A. Lundeberg, P. W. Fox, and J. Punćcohaŕ, "Highly confident but wrong: Gender differences and similarities in confidence judgments.," *Journal of educational psychology*, vol. 86, no. 1, p. 114, 1994.

[57] M. Glaser and M. Weber, "Which past returns affect trading volume?" *Journal of Financial Markets*, vol. 12, no. 1, pp. 1–31, 2009.

[58] J. Ameriks, A. Caplin, J. Leahy, and T. Tyler, "Measuring self-control problems," *The American Economic Review*, vol. 97, no. 3, pp. 966–972, 2007.

[59] F. D'Acunto, N. Prabhala, and A. Rossi, "The promises and pitfalls of robo-advising," 2018.

[60] S. Scheurle and K. Bräuer, "Fund savings plan choices with and without robo-advice," 2016.

[61] C. Cuevas, D. Bernhardt, M. Sanclemente, and M. Sanclemente, "The pied piper of pensioners," 2018.

[62] Z. Da, B. Larrain, C. Sialm, and J. Tessada, "Destabilizing financial advice: Evidence from pension fund reallocations," *The Review of Financial Studies*, vol. 31, no. 10, pp. 3720–3755, 2018.

[63] Q. Chen, I. Goldstein, and W. Jiang, "Payoff complementarities and financial fragility: Evidence from mutual fund outflows," *Journal of Financial Economics*, vol. 97, no. 2, pp. 239–262, 2010.

[64] I. Goldstein, H. Jiang, and D. T. Ng, "Investor flows and fragility in corporate bond funds," *Journal of Financial Economics*, vol. 126, no. 3, pp. 592–613, 2017.

[65] A. Frazzini and O. A. Lamont, "Dumb money: Mutual fund flows and the cross-section of stock returns," *Journal of financial economics*, vol. 88, no. 2, pp. 299–322, 2008.

[66] D. Lou, "A flow-based explanation for return predictability," *The Review of Financial Studies*, vol. 25, no. 12, pp. 3457–3489, 2012.

[67] B. M. Barber and T. Odean, "All that glitters: The effect of attention and news on the buying behavior of individual and institutional investors," *The Review of Financial Studies*, vol. 21, no. 2, pp. 785–818, 2007.

[68] N. Sicherman, G. Loewenstein, D. J. Seppi, and S. P. Utkus, "Financial attention," *The Review of Financial Studies*, vol. 29, no. 4, pp. 863–897, 2015.

[69] M. Woodford, "Information-constrained state-dependent pricing," *Journal of Monetary Economics*, vol. 56, S100–S124, 2009.

[70] Y. Chien, H. Cole, and H. Lustig, "Is the volatility of the market price of risk due to intermittent portfolio rebalancing?" *The American Economic Review*, vol. 102, no. 6, pp. 2859–2896, 2012.

[71] D. Andrei and M. Hasler, "Investor attention and stock market volatility," *The Review of Financial Studies*, vol. 28, no. 1, pp. 33–72, 2014.

[72] X. Gabaix and D. Laibson, "The 6d bias and the equity-premium puzzle," *NBER macroeconomics annual*, vol. 16, pp. 257–312, 2001.

[73] B. M. Barber and T. Odean, "The internet and the investor," *The Journal of Economic Perspectives*, vol. 15, no. 1, pp. 41–54, 2001.

[74] D. Simon and R. Heimer, "Facebook finance: How social interaction propagates active investing," 2012.

[75] J. Park, P. Konana, B. Gu, A. Kumar, and R. Raghunathan, "Confirmation bias, overconfidence, and investment performance: Evidence from stock message boards," 2010.

[76] P. Milgrom and N. Stokey, "Information, trade and common knowledge," *Journal of economic theory*, vol. 26, no. 1, pp. 17–27, 1982.

[77] C. A. Sims, "Implications of rational inattention," *Journal of monetary Economics*, vol. 50, no. 3, pp. 665–690, 2003.

[78] M. Kwon, J.-Y. Lee, W.-Y. Won, J.-W. Park, J.-A. Min, C. Hahn, X. Gu, J.-H. Choi, and D.-J. Kim, "Development and validation of a smartphone addiction scale (sas)," *PloS one*, vol. 8, no. 2, e56936, 2013.

[79] N. Karlsson, G. Loewenstein, and D. Seppi, "The ostrich effect: Selective attention to information," *Journal of Risk and uncertainty*, vol. 38, no. 2, pp. 95–115, 2009.

[80] A. J. Van Deursen, C. L. Bolle, S. M. Hegner, and P. A. Kommers, "Modeling habitual and addictive smartphone behavior: The role of smartphone usage types, emotional intelligence, social stress, self-regulation, age, and gender," *Computers in human behavior*, vol. 45, pp. 411–420, 2015.

[81] M. Muraven and R. F. Baumeister, "Self-regulation and depletion of limited resources: Does self-control resemble a muscle?" *Psychological bulletin*, vol. 126, no. 2, p. 247, 2000.

[82] M. Muraven, D. M. Tice, and R. F. Baumeister, "Self-control as a limited resource: Regulatory depletion patterns.," *Journal of personality and social psychology*, vol. 74, no. 3, p. 774, 1998.

[83] E. R. Sirri and P. Tufano, "Costly search and mutual fund flows," *The journal of finance*, vol. 53, no. 5, pp. 1589–1622, 1998.

[84] J. B. De Long, A. Shleifer, L. H. Summers, and R. J. Waldmann, "Noise trader risk in financial markets," *Journal of political Economy*, vol. 98, no. 4, pp. 703–738, 1990.

[85] P. C. Tetlock, "Giving content to investor sentiment: The role of media in the stock market," *The Journal of Finance*, vol. 62, no. 3, pp. 1139–1168, 2007.

[86] A. Ben-Rephael, S. Kandel, and A. Wohl, "Measuring investor sentiment with mutual fund flows," *Journal of Financial Economics*, vol. 104, no. 2, pp. 363–382, 2012.

[87] S. J. Grossman and J. E. Stiglitz, "On the impossibility of informationally efficient markets," *The American economic review*, vol. 70, no. 3, pp. 393–408, 1980.

[88] Z. Yi and N. Mao, "Mesuring investor sentiment in china's stock market: The construction of cicsi," *Journal of Financial Research*, no. 11, pp. 174–184, 2009.

[89] M. Baker and J. Wurgler, "Investor sentiment and the cross-section of stock returns," *The Journal of Finance*, vol. 61, no. 4, pp. 1645–1680, 2006.

[90] P. Deuskar, J. M. Pollet, Z. J. Wang, and L. Zheng, "The good or the bad? which mutual fund managers join hedge funds?" *Review of Financial Studies*, vol. 24, no. 9, pp. 3008–3024, 2011.

[91] L. Kostovetsky, "Brain drain: Are mutual funds losing their best minds?" *Unpublished working paper*, 2007.

[92] M. Kacperczyk, C. Sialm, and L. Zheng, "On the industry concentration of actively managed equity mutual funds," *The Journal of Finance*, vol. 60, no. 4, pp. 1983–2011, 2005.

[93] R. Kosowski, A. Timmermann, R. Wermers, and H. White, "Can mutual fund "stars" really pick stocks? new evidence from a bootstrap analysis," *Journal of Finance*, vol. 61, no. 6, pp. 2551–2595, 2006.

[94] K. Daniel, M. Grinblatt, S. Titman, and R. Wermers, "Measuring mutual fund performance with characteristic-based benchmarks," *Journal of Finance*, pp. 1035–1058, 1997.

[95] M. J. Gruber, "Another puzzle: The growth in actively managed mutual funds," *Journal of Finance*, vol. 51, no. 3, pp. 783–810, 1996.

[96] M. M. Carhart, "On persistence in mutual fund performance," *The Journal of Finance*, vol. 52, no. 1, pp. 57–82, 1997.

[97] L. Pástor and R. F. Stambaugh, "Mutual fund performance and seemingly unrelated assets," *Journal of Financial Economics*, vol. 63, no. 3, pp. 315–349, 2002.

[98] E. F. Fama and K. R. French, "Luck versus skill in the cross-section of mutual fund returns," *Journal of Finance*, vol. 65, no. 5, pp. 1915–1947, 2010.

# Appendix A: Additional Tests for Chapter 1

## A.1 Robustness Tests

### A.1.1 Regression Kink Analysis

Figure 1.1 shows that there is a kink regarding the intensity of innundation and flood exposure at the boundary of the flooded area. Therefore, a Regression Kink design is also applicable to this setting. To check the robustness of the RD analysis, I estimate the kink at the threshold and report the results in Appendix Table A.1. The key independent variable is $Distance_i * D_t^{Post} * D_i^{Inundated}$, where $Distance_i$ is defined as the distance between individual $i$'s residence location and the boundary of the inundated area before the shock, which is set to be positive inside the boundary, and negative outside.[1] The significantly negative coefficients on the triple-interaction term in all columns indicate the elasticity of pre-post change in $D^{Startup}$ against $Distance$ is significantly more negative inside than outside the boundary. Considering the inundation depth increases with $Distance$ inside the inundated area, this finding lends support to the hypothesis that experiencing a natural disaster dampens the incentive to pursue an entrepreneurial career.

### A.1.2 Do Observed and Unobserved Discrepancies across Elevation Confound the DID Analysis?

A potential concern about the DID design is that the high- and low-elevation residents fundamentally differ in some characteristics. If people varying on the characteristics happen to diverge in the employment decisions around the flood, the change in their entrepreneurial career choices might be unrelated to how much they are affected by the disaster.

To address this concern, I compare the low-elevation and high-elevation groups regarding a

[1] The definitions of other variables are consistent with Equation 1.1.

set of predetermined biographic variables, including age, gender, education, and quarterly income in Table A.2. Column (2) (Column (3)) reports the mean of the corresponding variable for the low- (high-) elevation residents, and Columns (3) and (4) report the differences between the first two columns and the t-statistics of the estimated gaps, respectively. $Age$ is defined as the age of an individual at the time of the flood. $Male$ denotes a dummy equal to 1 for male and 0 for female. $Education$ is the highest degree a person obtains on a scale of 1-4, where 1 represents a degree below high school, and 4 indicates a graduate degree. $Income$ is defined as the average quarterly income during the two years before the shock. According to Column (4), the two groups of individuals exhibit no significant difference except on the predetermined income level.

To test whether the result of the DID analysis is driven by the income discrepancy between the low- and high-elevation residents, I employ two methods. The first is incorporating $D_t^{post} \times Income_i$ as a control variable in Equation 1.4, where $Income_i$ is defined as the average quarterly income of individual $i$ in the two years before the shock. If the income gap between the high- and low-elevation residents does steer the two groups to diverge in their entrepreneurial activity after the flood, the coefficient on $D_t^{post} \times Income_i$ is expected to be significantly positive.

Panel A of Table A.3 shows $D^{Post} \times Income$ does not load contrary to the prediction. Also, note that the significance and the magnitude of the key coefficients on $D^{Post} * D^{LowElev}$ do not diminish after the inclusion of $D_t^{post} \times Income_i$ as a control variable. The finding suggests the income gap is unlikely the confounding driver of the DID result.

The second method builds on the following argument. If the decline in entrepreneurship among the low-elevation residents after a flood is attributed to their relatively low income ex ante, the high-income subgroup within the low-elevation residents should be presumably immune to the dampening effect of the floods relative to high-elevation residents. Accordingly, I split the low-elevation individuals into two subsets based on their predetermined income level. Using each subset as the treated group and the high-elevation residents as the control group in a separate partitioning analysis, I compare the change in entrepreneurship of the treated and control groups around the shock.

Panel B of Table A.3 reports the corresponding empirical results. Columns (1)-(3) (Columns (4)-(6)) report the estimation of Equation 1.4 based on the high-income (low-income) low-elevation residents and the high-elevation ones. For both sets of tests, the key coefficient on $D^{Post} * D^{LowElev}$ is significantly negative regardless of the specifications. Considering that the high-income, low-elevation residents on average earn slightly more than the high-elevation residents, the observation that they still refrain from working in a startup relative to the high-elevation residents after a flood suggests the dispersion between high- and low-elevation residents in entrepreneurship is probably not a result of the income discrepancy.

To further ensure the divergence between low- and high-elevation residents is not driven by a unobserved confounder coincident with the floods, I run a triple-differences regression of the form

$$\begin{aligned} D_{i,t}^{Startup} =& \alpha_i + \alpha_t + \beta_1 D_t^{Post} \times D_i^{LowElev} \times D_i^{Flood} + \beta_2 D_t^{Post} \times D_i^{LowElev} \\ &+ D_t^{Post} \times (\sum_{k=1}^{K} \gamma_k D_i^k) + D_t^{Post} \times (\sum_{j=1}^{J} \tau_j D_{i,t-1}^j) + \varepsilon_{i,t}, \end{aligned} \tag{A.1}$$

where $D_i^{Flood}$ is a dummy variable set to 1 if before the shock, individual $i$ lives in a county subject to a federally declared disaster, and 0 if he/she resides in other counties of the same state. $D_t^{post}$ is a dummy variable equal to 1 if quarter $t$ is after the flood of interest in the state where individual $i$ lives, whether he/she lives in the flooded counties or not, and 0 otherwise.[2] The test builds on the following logic: if a confounder drives the results by exerting differential impacts on high- and low-elevation residents within a county, the divergence across elevation should emerge in the nearby unflooded counties as much as in the flooded counties. Accordingly, I would expect $\beta_1$ to be insignificant (significantly negative) if a confounder exists (does not exist).

Columns (4) to (6) in Table A.4 report the results. The coefficient on the triple-interaction term remains significantly negative for all specifications, suggesting the differential impact across elevation in the flooded counties is not driven by a confounder. To verify the divergence across

[2]The definitions of other variables remain consistent with Equation 1.4.

elevation does not exist outside the flooded areas, I perform a placebo test on the unaffected counties in the five states of interest. In particular, I estimate Equation 1.4 on the unaffected counties *as if* a flood hit those counties at the same time that an actual flood occurs elsewhere in the state. Columns (1) to (3) report the result. The insignificant coefficients on $D^{Post} \times D^{LowElev}$ shows low- and high-elevation residents in the unaffected counties do not experience divergence around the "flood," which is consistent with the absence of a confounder.

Table A.1: **Robustness Test: Regression Kink Analysis.** This table shows the estimation of the size of the kink present at the boundary of the inundated area at the individual-quarter level. Columns (1)-(2) are based on the individuals living a narrow band along the boundary of the inundated area with a half-width of 0.1 miles, and Columns (3)-(4) (Columns (5)-(6)) based on a band with 0.15 (0.2) miles half-width. The outcome variable is $D^{startup}$, a dummy variable set to 1 if an individual works for a startup as a primary job during a given quarter, and 0 if he/she works for a more matured firm. *Distance* is defined as the distance between an individual's predetermined residence location to the boundary of the inundated area, set to be positive if inside the boundary and negative if outside. The definitions of other explanatory variables are consistent with Table 1.3. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{startup}$ | | $D^{startup}$ | | $D^{startup}$ | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| | 0.1 miles | | 0.15 miles | | 0.2 miles | |
| $Distance \times D^{Post} \times D^{Inundated}$ | -15.06*** | -15.18*** | -11.93*** | -11.97*** | -7.448*** | -7.499*** |
| | (-3.00) | (-3.03) | (-4.24) | (-4.26) | (-3.83) | (-3.86) |
| $Distance \times D^{Post}$ | -1.301 | -1.223 | 5.636*** | 5.678*** | 3.468*** | 3.506*** |
| | (-0.52) | (-0.49) | (4.16) | (4.19) | (3.90) | (3.94) |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | No | Yes | No | Yes | No |
| Quarter FE | No | Yes | No | Yes | No | Yes |
| County × Post FE | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 80,500 | 80,500 | 121,000 | 121,000 | 162,000 | 162,000 |
| R-sq | 0.51 | 0.51 | 0.519 | 0.519 | 0.533 | 0.533 |

Table A.2: **Summary Statistics of the Low- and High-Elevation Residents.** This table compares the low-elevation and high-elevation groups regarding a set of predetermined demographic variables. Column (2) (Column (3)) report the mean of the corresponding variable for the low- (high-) elevation residents, and Columns (3) and (4) report the differences between the first two Columns and the t-statistics of the estimated gaps, respectively. $Age$ is defined as the age of an individual at the time of the flood. $Male$ denotes a dummy equal to 1 for male, and 0 for female. $Education$ is the highest degree a person obtains with a scale of 1-4, where 1 represents a degree below high school, and 4 indicates a graduate degree. $Income$ is defined as the average quarterly income during the two years before the shock. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| Predetermined Attributes | Low-elevation [1] | High-elevation [2] | Difference [3] | T-statistics [4] |
|---|---|---|---|---|
| Male | 0.543 | 0.527 | 0.016 | (1.33) |
| Age | 37.70 | 37.90 | -0.197 | (-0.54) |
| Education | 2.727 | 2.699 | 0.028 | (0.32) |
| Income | 6,981 | 8,710 | -1,729*** | (-4.15) |

Table A.3: **Does Income Discrepancy Confound the DID Analysis?** Panel A reports the estimates of Equation 1.4 at the individual-quarter level, with $D_t^{post} \times Income_i$ as an additional control variable. $Income_i$ is defined as the average quarterly income of individual $i$ in the two years before the shock. Panel B reports the partitioning analysis based on the predetermined income level of the low-elevation residents. Columns (1)-(3) (Columns (4)-(6)) of Panel B report the estimation of Equation 1.4 using the high-income (low-income) low-elevation residents as the treated group and the high-elevation ones as the control group. The outcome variable in both panels is $D^{startup}$, a dummy variable set to 1 if an individual works for a startup as a primary job during a given quarter, and 0 if he/she works for a more matured firm. The definitions of other explanatory variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

Panel A: Control Variable Method

| | $D^{startup}$ | | |
|---|---|---|---|
| | [1] | [2] | [3] |
| $D^{Post} \times D^{LowElev}$ | -0.493*** | -0.494*** | -0.522*** |
| | (-3.43) | (-3.15) | (-3.34) |
| $D^{Post}\times$ Income | -0.000 | 0.000 | 0.000 |
| | (-0.84) | (0.53) | (0.28) |
| Quarter FE | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes |
| Post × County FE | Yes | Yes | No |
| Post × Firm FE | Yes | No | No |
| N | 2,144,000 | 2,179,000 | 2,179,000 |
| R-sq | 0.770 | 0.564 | 0.564 |

Panel B: Partitioning the Treated Group by Income

| | $D^{startup}$ | | | $D^{startup}$ | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| | Top Half by Income | | | Bottom Half by Income | | |
| $D^{Post} \times D^{LowElev}$ | -0.371*** | -0.398 | -0.426* | -0.911*** | -0.713** | -0.711** |
| | (-3.00) | (-1.63) | (-1.75) | (-3.30) | (-2.44) | (-2.44) |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Post × County FE | Yes | Yes | No | Yes | Yes | No |
| Post × Firm FE | Yes | No | No | Yes | No | No |
| N | 1,608,000 | 1,634,000 | 1,634,000 | 1,608,000 | 1,634,000 | 1,634,000 |
| R-sq | 0.783 | 0.565 | 0.564 | 0.778 | 0.565 | 0.564 |

Table A.4: **Is the Divergence Across Elevation Driven by Confounder?** Columns (1)-(3) report the estimates of Equation 1.4 at the individual-quarter level based on the unaffected counties in the five states of interest, and Columns (4)-(6) report the estimates of Equation A.1 based on all counties in the five states at the individual-quarter level. The outcome variable is $D^{startup}$, a dummy variable set to 1 if an individual works for a startup as a primary job during a given quarter, and 0 if he/she works for a more matured firm. $D^{Flood}$ is a dummy variable set to 1 if before the shock an individual lives in a county where the U.S. federal government declared a natural disaster, and zero if he/she resides in other counties of the same state. $D^{Post}$ is a dummy variable equal to 1 after the flood event that hits a part of the state, and 0 otherwise. The definitions of other explanatory variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{startup}$ | | | $D^{startup}$ | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| | Unaffected Counties | | | All Counties | | |
| $D^{Post} \times D^{LowElev} \times D^{Flood}$ | | | | -0.640** | -0.584* | -0.621* |
| | | | | (-2.13) | (-1.73) | (-1.85) |
| $D^{Post} \times D^{LowElev}$ | 0.020 | 0.054 | 0.108 | 0.135 | 0.096 | 0.098 |
| | (0.09) | (0.18) | (0.36) | (0.51) | (0.32) | (0.33) |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Post × County FE | Yes | Yes | No | Yes | Yes | No |
| Post × Firm FE | Yes | No | No | Yes | No | No |
| N | 532,000 | 551,000 | 551,000 | 2,675,000 | 2,730,000 | 2,730,000 |
| R-sq | 0.795 | 0.571 | 0.569 | 0.76 | 0.565 | 0.565 |

## A.2 Additional Partitioning Analysis

### A.2.1 How Does the Effect Vary with Sectors?

One potential concern is that the documented effect on individuals' entrepreneurial career choices might be driven by a particular industry. To understand whether this is the case, I conduct a set of partitioning analyses by NAICS 2-digit industry code. The five industries accounting for the largest percentages of jobs in my sample are "Accommodation and Food Services", "Professional, Scientific, and Technical Services", "Construction", "Administrative and Support Services", and "Other Services". I compare the low- and high-elevation residents within a county in terms of their decisions to work in a startup in each of the above industries around the flood using a DID analysis similar to Specification 1.4. Table A.5 reports the result. The outcome variables for Column (1)-(5) indicate whether a person is working in a startup of a given industry in a quarter. For example, the outcome variable in Column (2) is a dummy variable set to 1 if an individual works in a startup in the hospitality industry in a given quarter, and 0 if he/she works for a more matured firm in the hospitality industry or any firm in other industries. My analysis shows the point estimates of the effect on entrepreneurial career choices are negative for all the five industries, although the magnitude varies. I also find a particular pronounced effect for *Professional, Scientific, and Technical Services*, a 8.3% decline relative to the baseline rate of working in an entrepreneurial firm in this field. Since many high-tech startups are classified into the professional services industry, the above findings suggest the wealth damage from natural disasters like flood can discourage people from pursuing entrepreneurial careers in a wide range of areas including technology-relevant fields.

### A.2.2 Is the Effect Driven by Departure from Startups or Transition into Startups?

Two forces can potentially drive the dampening effect of experiencing a flood on entrepreneurial career choices. On one hand, after his/her home being flooded, a person who currently works for an established firm may become less willing to join a startup even if such job opportunity presents. One the other hand, a worker in a startup may decide to transition into a more matured firm if

he/she is exposed to the flood.

To understand which force is more dominant, I conduct partitioning analysis on the two employment decisions respectively and report the result in Table A.6. The outcome variable in Column (1) is $D^{startup}_{transition}$, a dummy variable set to 1 if an individual works for a startup in a given quarter and for an established firm in the previous quarter, and 0 if he/she works for an established firm in the given quarter or works for a startup in the previous quarter. The outcome variable in Column (2) is $D^{startup}_{retainment}$, a dummy variable set to 1 if an individual works for a startup in a given quarter and in the previous quarter, and 0 if he/she works for an established firm in the previous quarter or in the given quarter. A back-of-envelop calculation based on the coefficient estimate in Column (1) suggests about 79.5% of the lowered propensity to work in a startup is attributed to the decline in the transition from established firms into startups.

The calculation process is as follows. Column (2) of Table 1.5 suggests among the treated people, the propensity to work in a startup conditional on being employed is 0.498% lowered than the pre-shock level in the two years after the flood. Suppose the rate of working in a startup declines at the constant rate during the two years, the likelihood of working in a startup in a given quarter will decrease by about $0.498\%/4.5 = 0.111\%$ from the previous quarter. As shown in Column (1) of Table A.6, each quarter the likelihood of leaving a matured firm for a startup declines by 0.0882% conditional on being employed in comparison with the pre-shock level, which accounts for $0.0882\%/0.111\% = 79.5\%$ of the baseline effect.

Table A.5: **DID Design: Does the Effect Vary Across Industry?** This table reports the estimates of Equation 1.4 at the individual-quarter level using alternative outcome variables. The outcome variable in Column (1) is $D^{startup}_{scientific}$, a dummy variable set to 1 if an individual works for a startup in the "Professional, Scientific, and Technical Services" industry in a given quarter, and 0 if he/she works for a more matured firm or any job in other industries. In Columns (2)-(5), $D^{startup}_{hospitality}$ ($D^{startup}_{construction}$, $D^{startup}_{other}$, and $D^{startup}_{other}$) is a dummy variable indicating whether a person is working for the "Accommodation and Food Services" industry ("Construction", "Other Services", and "Administrative and Support Services" industries) defined in a similar way. The definitions of other variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{startup}_{scientific}$ [1] | $D^{startup}_{hospitality}$ [2] | $D^{startup}_{construction}$ [3] | $D^{startup}_{other}$ [4] | $D^{startup}_{administrative}$ [5] |
|---|---|---|---|---|---|
| $D^{Post} \times D^{LowElev}$ | -0.0858* | -0.0961* | -0.0308 | -0.0043 | -0.0107 |
| | (-1.67) | (-1.71) | (-0.62) | (0.17) | (-0.21) |
| Quarter FE | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes |
| Post × County FE | Yes | Yes | Yes | Yes | Yes |
| N | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 | 2,179,000 |
| R-sq | 0.539 | 0.601 | 0.543 | 0.646 | 0.527 |

Table A.6: **DID Design: Departure from Startups or Transition into Startups?** This table reports the estimates of Equation 1.4 at the individual-quarter level using alternative outcome variables. The outcome variable in Column (1) is $D^{startup}_{transition}$, a dummy variable set to 1 if an individual works for a startup in a given quarter and for an established firm in the previous quarter, and 0 if he/she works for an established firm in the given quarter or works for a startup in the previous quarter. The outcome variable in Column (2) is $D^{startup}_{retainment}$, a dummy variable set to 1 if an individual works for a startup in a given quarter and in the previous quarter, and 0 if he/she works for an established firm in the previous quarter or in the given quarter. The definitions of other variables are consistent with Table 1.5. T-statistics based on the clustered standard errors by individual are reported in parentheses. *, **, and *** denote the estimates are statistically significant at the 10%, 5%, and 1% levels, respectively.

| | $D^{startup}_{transition}$ [1] | $D^{startup}_{retain}$ [2] |
|---|---|---|
| $D^{Post} \times D^{LowElev}$ | -0.0882** | -0.410*** |
| | (-2.45) | (-2.79) |
| Quarter FE | Yes | Yes |
| Individual FE | Yes | Yes |
| Post × County FE | Yes | Yes |
| N | 2,179,000 | 2,179,000 |
| R-sq | 0.119 | 0.565 |