THÈSE

En vue de l'obtention du

DOCTORAT DE L'UNIVERSITÉ DE TOULOUSE

Délivré par l'Université Toulouse 1 Capitole

Présentée et soutenue par

YIFEI ZHANG

Le 9 juin 2022

Three Essays on Corporate Innovation and Shareholder Activism

Ecole doctorale : **TSE - Toulouse Sciences Economiques**

Spécialité : **Sciences Economiques - Toulouse**

Unité de recherche :
TSE-R - Toulouse School of Economics - Recherche

Thèse dirigée par
Ulrich HEGE

Jury

M. Florian Ederer, Rapporteur
Mme Merih Sevilir, Rapporteure
Mme Edith Ginglinger, Examinatrice
M. Milo Bianchi, Examinateur
M. Ulrich Hege, Directeur de thèse

Three Essays on Corporate Innovation and Shareholder Activism

By

Yifei Zhang

A THESIS SUBMITTED IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR

The Doctorate Degree in Economics

[June, 2022]

Supervisor

Ulrich Hege

Committee Members

Milo Bianchi (Toulouse School of Economics)

Florian Ederer (Yale University, School of Management)

Edith Ginglinger (Université Paris Dauphine-PSL)

Merih Sevilir (Indiana University, Kelley School of Business)

To my parents who offered love and support throughout.

感谢我亲爱的爸爸妈妈。

Acknowledgements

"The pursuit of PhD is an enduring daring adventure, but a little bit torturing to a stupid boy like me." — From a Forum

Throughout my PhD studies, I was asked several times why I decided to pursue a PhD. In reality, I still don't have a definite answer. I chose empirical corporate finance as my research area simply because it is interesting, intuitive, and involves less complex math. In fact, in my undergraduate real analysis class, I only received a B. So, it's a story of a stupid boy trying to prove himself.

I recall starting my own research with my advisor Ulrich Hege, and at that time, I knew very little about finance and finance research. I knew nothing about the cost of capital or the Fama-French 3-factor model. Uli recently gave me a bantering talk: "Yifei, after rereading your second-year paper, I realized again all your progresses in these years." In fact, my job market paper is Chapter 2 of this thesis, and my "notorious" second-year paper is Chapter 4 (with some improvements). These are parts of footprints in my PhD life.

During my six-year Ph.D. study and research, there were so many people helping me to grow and teaching me how to become a serious researcher.

First, I would like to express my sincere gratitude to my supervisor, Ulrich Hege, for his constant support, research suggestions, encouragement, and assistance during my PhD career. Without Uli, I would be unable to complete my doctoral studies at Toulouse School of Economics.

Next, I want to convey my heartfelt gratitude to Milo Bianchi, Florian Ederer, and Sébastien Pouget for their invaluable assistance for my job market paper, the thesis Chapter 2, and enormous support for my job market application. Without them, I could not have received an offer from Peking University. Milo especially constantly offered many suggestions to me during the past six years.

Moreover, I would like to thank other members of finance faculty at TSE, especially Alexander Guembel, Matthieu Bouvard, Catherine Casamatta, Patrick Coen, Sophie Moinas, and Silvia Rossetto. Their help is again enormous. Nour Meddahi, the Ph.D. director at TSE, constantly encourages me for my research and job market applications. Finally, I want to thank TSE for offering me a 6-year scholarship for my studies. Special thanks to Ludmila Namolovan for her excellent help in my job application. Special thanks to Joanne Juan Chen, my future co-author, for our job market preparations.

I am grateful to Song Ma and Andrew Metrick for my Yale visit. Moreover, a number of scholars and researchers at other institutions provided help and advise at many crucial moments during my PhD studies, including Rustam Abuzov, Nicolas Boyson, Alon Brav, Alex Edmans, Caroline Genc, Stefano Lovo, Armin Schwienbacher, Merih Sevilir, Guillaume Vuillemey, Xiang Zheng. Thank you!

To my fellow PhD friends at TSE: Filip Mrowiec, Ying Liang, Lan Lan, Li Yu, Jun Yan, Yang Yang, Natalia Kovaleva, Yu Wen, Ling Zhou, Celine Yue Fei, Yuting Yang, Jiakun Zheng, Yaxin Li, Xiaojuan Liu, Li Bao, Yixin Huang, Alae Baha, Paloma Carrillo, Fernando Stipanicic, thank you! This thesis is also in memory of my great friend, Yunpeng Duan.

Indeed, I cannot forget Dakang Huang (Mao), my roommate who spent six years with me. Dakang is not merely my friend, colleague, and roommate. He is my "brother", like the one in Hong Kong movies. I enjoyed praising Dakang in front of others (including Nour and Alae). So, Dakang, I want to say thank you! You are a genius!

Most importantly, I want to thank my parents for their unwavering support and understanding of my 6-year overseas studies in France. During these six years, they have made numerous sacrifices. And I plan to accompany them more once I begin my new work at Peking University. Therefore, this dissertation is dedicated to my parents.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

Over the last two decades, empirical corporate finance research has grown broader and broader, incorporating many new research topics and concepts. Many of these topics address whether and how firms respond to external forces or pressures such as disruptive technologies, peaks of venture capital investments, shareholder activism, and investors' preference changes on ESG. Therefore, my thesis is made up of three empirical corporate finance papers on corporate innovation and shareholder activism, which answer the aforementioned question. The first chapter analyzes how firms respond to disruptive technologies and emerging business opportunities by engaging in corporate venture capital investments, while the second and third chapters investigate the reaction to hedge fund activism campaigns and their potential threats.

The first paper in my thesis is about innovation. Corporate innovation research has proliferated in the last decade, thanks to some leading researchers, such as Xuan Tian at Tsinghua University. Researchers not only gauge innovation activities by patents but also start to focus on the patenting examination process and many open innovation strategies. As a matter of fact, corporate venture capital investments belong to this open innovation.

In the next chapter, I ask whether and how corporate venture capital (CVC) spurs changes in firm scope. Using two sets of firm scope metrics, a text-based emerging business measure and Compustat segment measures, I document that CVC investments are strongly associated with subsequent firm scope changes of the CVC corporate parent, including seeding emerging businesses, establishing new divisions, terminating obsolete divisions, and changing the primary industry. Further evidence is consistent with an experimentation view about CVC.

The experimentation view postulates that CVC allows a corporate to experiment with various business opportunities, in which case the manager is uncertain about their final results. Each CVC deal thus could be regarded as an experiment that creates a real option for a potential new line of products or activities. Through interacting with the startup managers and participating in the startup's operation in a CVC deal, a CVC parent firm could receive valuable information (called a signal in this chapter) about the future potential of the new business. A CVC parent finally pins down the best business option through signals from multiple experiments. As CVC is commonly known as an open innovation strategy in the innovation toolbox of modern corporations, this chapter helps to understand the interaction between open innovation and firms' growth strategies.

In this chapter, I provide some innovative work on identification strategies of CVC research. First, I introduce a new instrument for CVC investments using the fund inflow shocks of independent venture capital firms (IVC) in each CVC program's past syndicate network. Second, I introduce the US non-stop airline routes as a quasi-natural experiment.

The third and fourth chapters are about hedge fund activism. Hedge fund activism research has become popular thanks to sharing hand-collected data by Alon Brav and Wei Jiang. Recently, researchers still actively work on this area by investigating the spillover impact of HFA campaigns and the impact on ESG performance.

Chapter 3 is a joint research with Ulrich Hege. We explore the impact of hedge fund activism on corporate asset markets. We find that activist target firms are more likely to receive merger bids, and make more divestitures and fewer acquisitions, in line with earlier studies. We consider a second channel of activism pressure, the disciplining effect on firms exposed to activism threats. We propose measures of activism threats at the firm level and at the industry level, and find that firms exposed to such threats change their behavior in similar ways, but with subtle differences: they divest more, but are only marginally more likely to be sold. Only large firms under threat reduce their acquisition activity, whereas small firms expand it.

Comparing these two parallel channels of hedge fund pressure, we find that they contribute about equally to the change in deal activity in highly affected industries exposed, with activism threats being more important for acquisitions, and targets more important for corporate sales. We consider the impact on real asset liquidity: when firms in affected industries want to simultaneously sell more and buy less assets, then real asset liquidity shrinks by up to 35%, creating a role for outside liquidity providers. We find that acquirers from outside the affected industry - private equity funds and listed firms in other industries - provide liquidity, and more so in industries with high asset redeployability.

We find evidence that the squeeze on real asset liquidity also affects transaction prices: seller announcement returns are smaller in corporate sales when industries are affected by activist pressure (merger bids and divestiture bids), and buyer announcement returns are (weakly) larger in this case. The effect is stronger in industries with low redeployability. However, we find that divestitures done by activist targets resist the price pressure remarkably well.

Finally, we consider whether activist pressure leads to more efficient transactions. Isolating the incremental effect of transactions done under activism influence, we find positive long-run performance effects when corporate transactions are undertaken by activism targets; we do not find a similar effect for transactions undertaken under activism threat. Thus, the direct involvement of hedge fund activists seems necessary to create additional efficiency gains.

Our paper shows that activism creates important market externalities for firms not directly targeted, by changing the environment and behavior in acquisition markets. It is not clear that these changes are efficient, but at least small firms disciplined by activism threats seem to make better acquisitions. Our findings lead to new questions that go beyond the scope of this paper, for example whether activists reduce or magnify the cyclicality of real asset markets.

Chapter 4 is based on my second-year thesis. I investigate which kind of targeted firms benefit the most from hedge fund activism campaigns. I first document that ex-ante better governance firms experience larger value and performance improvements after activism campaigns. Moreover, good governance firms operating in relatively competitive industries benefit the most from HFA campaigns among all targeted firms. Both results are counter-intuitive since ex-ante good governance firms operating in relatively competitive industries should suffer the least from agency costs and have already operated on the industry efficiency frontier. As a result, further value improvements should be minimal. I provide a new explanation for the puzzling results through the success probabilities of activist campaigns and value improvement conditional on campaign success.

Chapter 2

Corporate Venture Capital and Firm Scope

2.1 Introduction

Understanding firm scope and the boundary of firms is a central topic in economics and finance.[1] However, there is little empirical work on determinants of firm scope.[2] Perhaps even more surprisingly, despite the prominence of the "Schumpeterian" view that innovation is the key driving force behind growth and evolution of firms and economies, there is almost no work contributing to the relationship between corporate innovation strategies and dynamics of firm scope.[3] Therefore, this paper contributes to the empirical literature on firm scope by studying an open corporate innovation strategy – corporate

[1]Important milestones in theory of the firm include transaction cost theory (Coase [1937] and Williamson [1985]) and the property rights approach (Grossman and Hart [1986] and Hart and Moore [1990]). Recent theoretical work about firm scope includes Hart and Holmstrom [2010].

[2]As argued recently in Hoberg and Phillips [2018], the traditional conglomerate literature takes firm scope as given and seldom explores determinants of firm scope.

[3]Although Bena and Li [2014], Seru [2014], and more recently, Frésard et al. [2020] study corporate innovation and boundaries of the firm, they do not specifically investigate firm scope, that is the set of businesses or products a firm operates and offers.

venture capital investments – popular among large industry leaders.

A corporate venture capital (CVC) program is a venture capital arm affiliated with an established firm. CVC has grown in recent decades to become an important tool in open innovation strategies of many leading companies, including tech giants, such as Apple, Google, and Microsoft. Thus, investigating the impact of CVC on scope changes of those leading firms is of great importance for both academia and practitioners. Moreover, given its special characteristics, CVC offers a unique opportunity to discover a new firm growth (and scope change) strategy, the experimentation strategy, which lies at the heart of this paper.[4]

More specifically, this paper asks whether and, more crucially, how CVC spurs firm scope changes. The hypothesis relies on anecdotal and survey evidence documenting that establishing a CVC program could help its parent firm (such as Google) to identify new business opportunities.[5] Given the newly identified business opportunities, a CVC parent firm will naturally integrate those new businesses into its current business domain, thus reshaping its firm scope. Further evidence is consistent with an experimentation view of CVC investments, with more promising ventures having a stronger impact on the scope change of parent firms. The organization of findings in the paper is illustrated in Figure 2.1.

I use two ways to gauge firm scope changes. First, I leverage textual analysis in defining emerging businesses and count how many emerging businesses are newly added to each publicly-listed firm's annual 10-K business description. Emerging businesses are proxied by "emerging phrases", the top 5% most popular business short phrases taken from the union of VC-backed startups' business descriptions in a given year. (See Fig-

[4]Compared with other instruments that firms have at their disposal to foster innovation (e.g., in-house efforts to carry out R&D and create new intellectual property, acquisitions of research results or innovative startups, the recruitment of employees with new expertise), CVC offers the advantage that firms initial investment decisions, as well as metrics of investment outcomes, can be observed hence offering an exciting view on the use of experimentation in firm strategy.

[5]The evidence and surveys are documented in Section 2.2. However, the precise mechanism by which it happens is not documented in the survey or the anecdotal evidence.

ure 2.3 for a quick view.) Second, I use Compustat Segment data to construct variables for scope changes, including establishing new divisions (segments), terminating obsolete divisions, and changing the corporate primary industry.[6]

In both cases, I find that CVC investments are strongly associated with a subsequent change of firm scope. Specifically, a CVC parent, on average, adds 1.5 (100%) more emerging phrases into the firm's 10-K annual business description than those industry-year peers without CVC investments within two years after CVC deals. Moreover, firms are around 60% more likely to establish a new division (operating in a new industry) and around 35% more likely to remove an old division within the next two years following the CVC investment decision.[7]

Having documented these basic facts, I turn to scrutinizing the channel through which CVC helps identify new business opportunities and ultimately spurs firm scope change. The empirical evidence is consistent with an experimentation view of CVC investments. The experimentation view postulates that CVC allows a corporation to experiment with various business opportunities, in which case the manager is uncertain about their final results.

Each CVC deal could be regarded as an experiment that creates a real option for a potential new line of products or activities [Keil et al., 2008]. Through interacting with the startup managers and participating in the startup's operation in a deal, a CVC parent firm could receive valuable information (which I call a "signal" in this paper) about the future potential of the relevant business. Crucially, these signals help pinpoint promising and new business opportunities and avoid those business "traps". The experimentation strategy is a logical response to identify good business opportunities under huge uncer-

[6]These two methods are actually complementary. While Compustat Segment measures capture larger changes, they are coarse in terms of measuring scope dynamics. The text-based emerging business measure is more granular and captures popular businesses that are not only new relatively to the CVC parent firm but also new relatively to the US economy as a whole.

[7]The corporate primary industry change takes effect over a longer horizon: within three to five years after investments.

tainty in the VC industry [Kerr et al., 2014, Ewens et al., 2018].

There are two sets of evidence supporting the experimentation hypothesis. First, under high uncertainty in VC investments, diversifying CVC deals across industries and business areas is a necessary step to increase the odds of discovering promising business opportunities. Consistent with this hypothesis, I find that industry diversification is popular among CVC programs, and the more diversified a program's investment strategy is, the more likely its parent firm conducts scope changes. Second, I test the signaling and winner-picking. I estimate two discrete choice models [McFadden, 1973] about the industry choice of establishing a new division and seeding an emerging business, respectively. I find that, conditional on CVC investments, receiving a good signal from an invested startup is strongly associated with the choice of establishing a new division or adding emerging phrases in the startup's industry, where the signal is only observable after CVC deals but not before.[8] Moreover, CVC parents only react to signals, the private information obtained from their own CVC deals, and do not react to public information, such as industry IPO waves.

Adding to established evidence that firms experiment for future growth directions through CVC, I then sharpen the causality between CVC and firm scope and exclude an alternative story for my results: it is the business opportunity that drives CVC investments and the further firm scope changes.

I introduce a new instrument for CVC investments using fund inflow shocks of independent venture capital firms (IVC) in each CVC program's past syndicate network.[9] The idea of the instrument is that, if an independent VC firm j (IVC j) receives a positive fund inflow shock today, and meanwhile, the CVC Firm i is in IVC j's past syndicate

[8] It is challenging to measure those signals since they are private information transmitting from startups to CVC firms. Therefore, I proxy signals in the empirical exercises using the startups' IPO, acquisition, bankruptcy, and patent growth information.

[9] Independent venture capital firms (IVC) are simply the traditional VC firms with a limited partnership as the organizational form. I call them IVC to distinguish them from CVC. Furthermore, IVC terminology is widely used in the CVC literature, for example, Chemmanur et al. [2014] and Ma [2020].

network, then the IVC j is very likely to initiate new deals and invite CVC Firm i, its old partner, to join in its new investments.[10] Alternatively, IVC j could simply recommend some deals to the connected CVC.[11]

Notably, the fund inflow shocks are idiosyncratic across IVCs, orthogonal to any VC industry investment opportunities and technology shocks. I construct my instrument following the recent Granular IV (GIV) approach developed in Gabaix and Koijen [2020]. More precisely, the GIV is the sum of the idiosyncratic fund inflow shocks of those IVCs in the past 5-year syndicate network of each CVC program.

The instrument works on a small sample of U.S.-listed firms that have already started CVC investments in the past, thus, enjoying IVC networks today. In both the first and second stage regressions involving these CVC firms, I control for their past IVC network size and IVC characteristics (average age and past IPO performance) in the network, as well as the past three-year CVC investments. I document that my instrument strongly predicts the continuation of investments by a CVC program. In the second stage, I find that the number of CVC deals positively predicts firm scope changes measured by both the emerging phrases and Compustat segment dummies.

In the last section of my paper, I investigate two extensions. First, I use U.S. direct and indirect airline routes (following Bernstein et al. [2016]) between the geographical location of CVC parent firm and the startup in a CVC deal to generate plausibly exogenous variation in the treatment intensity of CVC deals, that is the extent to which a CVC manager interacts with startup managers. Controlling for various fixed effects, I document that CVC deals with higher treatment intensity are more likely to lead to subsequent firm scope changes by CVC parents. Second, I investigate value creation in-

[10] Here is an example of the invitation: between 1994 and 2000, Cisco Systems (a large industrial firm) was invited into 13 syndications led by Sequoia Capital (a pure VC firm), as documented in Ferrary [2010].

[11] The idea is based on previous findings in VC and CVC literature: (1) syndicate networks are crucial in the VC world, and many IVCs invite their old partners in the previous syndicate network to join in their new deals [Hochberg et al., 2007, 2010, Keil et al., 2010]; (2) IVC is the largest deal source of most CVC programs. IVC usually recommends deals to CVC or offers “deal flow” to CVC [MacMillan et al., 2008].

volving post-CVC firm scope changes. I find that most value creation by CVC actually derives from post-CVC firm scope changes, i.e., division creations and removals. This result helps rule out the empire-building hypothesis regarding CVC investments as pet projects.

The paper is related to three broad strands of the literature. First, this paper is related to canonical literature regarding firm scope, dating back to Teece [1980] and Panzar and Willig [1981] in economics and Lang and Stulz [1994], Berger and Ofek [1995], and Lamont [1997] in finance. Recently, Hoberg and Phillips [2021] document that 21st century US firms usually expand their businesses across related industries and thus are immune to the well-known diversification discount. In this paper, I find a new mechanism of scope change by US-listed firms through CVC experimentation.

Second, the paper contributes to the experimentation literature in entrepreneurial finance, such as Manso [2016] and Ewens et al. [2018]. Specifically, Ewens et al. [2018] find that recent VC firms adopt a new experimentation strategy in their investments, so-called the "spray and pray" strategy, especially after the cost of starting software and internet-related ventures drops significantly. There is one key difference between their experimentation and my experimentation – the goal. While VC firms engage in experimentation to search for "unicorns", CVC firms aim to figure out optimal growth directions for company's future.

Third, the paper contributes to the VC literature, and more precisely, the CVC literature pioneered by Gompers and Lerner [2000] and Hellmann [2002]. Previous literature documents that established firms in more competitive industries [Fulghieri and Sevilir, 2009, Kim et al., 2016], in industries with higher technology uncertainty [Basu et al., 2011] and low intellectual property protection [Dushnitsky and Lenox, 2005], with lower institutional ownership [Tian and Ye, 2018], and firms experiencing deterioration of internal innovation [Ma, 2020] are more likely to conduct CVC investments. I complement the aforementioned studies by relating CVC to firm scope changes of its parent corporation. Another closely related CVC paper is Shan [2019]. He studies how the ex-ante product and technology distances between startups and established firms influence decisions between acquisitions and CVC investments by established firms. In contrast, my paper addresses the broad questions whether CVC investments lead to changes in firm scope, and whether there is evidence in support of the experimentation view of CVC strategies.[12]

The rest of the paper proceeds as follows. Section 2.2 introduces the background of CVC and develops the hypotheses; Section 2.3 describes the data and summary statistics; Section 2.4 provides the basic facts between CVC and firm scope changes; Section 2.5 studies CVC as an experimentation process; Section 2.6 provides the identification between CVC and firm scope changes. The last section concludes the paper.

2.2 Background and Hypothesis Development

This section starts with the institutional background of corporate venture capital (CVC). A CVC deal is formally defined as a minority equity investment by an established corporation in a privately held entrepreneurial company [Dushnitsky, 2012]. Alternatively, one can interpret a CVC program as the venture capital arm affiliated with an established corporation (such as Google Venture affiliated with Google).

CVC departs from traditional VC firms mainly in three key aspects. First, whereas traditional venture capital firms solicit funding from prospective limited partners, fundings for investments of a CVC program mostly come from its unique corporate parent. Second, around two-thirds of CVC programs do not have a dedicated fund structure (including a fixed fund lifetime); instead, they are more akin to "discretionary" or "ever-

[12] Although he also uses textual measure based on the 10-K and startup's businesses, his measure is very different compared with mine. He uses the textual measure to gauge the ex-ante difference of businesses and technologies between startups and established firms, while I construct the emerging phrases to proxy emerging businesses in the economy.

green" funds: they invest when investment opportunities arrive [MacMillan et al., 2008].

The third, and the most important feature is that, although seeking financial returns remains an essential objective, in most cases, a CVC program also seeks strategic goals for its corporate parent, such as identifying new technology, seeking new growth opportunities, and importing innovation into existing business units [Siegel et al., 1988, MacMillan et al., 2008]. Regarding the strategic goals, CVC literature has reached a consensus about its importance. Chesbrough [2002] argues that if CVC investments were uncoupled from corporate strategies and operating capabilities and were only motivated by prospect of financial gains, then shareholders of CVC parent would do better by investing in IVC funds instead.

Strategic management scholars have conducted various interview-based surveys toward CVC managers worldwide to understand their strategic goals.[13] There are two strategic objects frequently appearing in these surveys. The first is to offer a window for new technology (open innovation); the second one, which is more likely to be neglected, is that CVC investments could help to identify new business opportunities for its corporate parent.

For example, in a recent survey of 48 large CVC programs conducted by the National Venture Capital Association (NVCA), more than half of CVC managers report that identifying new markets and new business directions are critical strategic aims of the programs [MacMillan et al., 2008]. Other survey evidence supporting the CVC objective in finding new business opportunities lies in Winters and Murfin [1988], Sykes [1990], McNally [1997], and Ernst and Young [2009]. This paper's hypothesis is thus developed on this second important strategic objective.

Therefore, I argue that CVC programs and CVC investments can help firms to identify new business opportunities. (See Figure 2.1 for an illustration). After identifying an opportunity (say an emerging business), a CVC parent firm will naturally integrate

[13] For a summary, see Dushnitsky [2012].

the emerging business into its current business, thus changing the firm scope. I use two distinct but complementary approaches to measure those changes in firm scope: on the one hand, I use textual analysis to identify emerging businesses in the US economy and further gauge the business integration of those emerging businesses by CVC parents through SEC annual 10-K filings; on the other hand, I deploy the traditional Compustat Segment dataset in measuring firm scope change (see Figure 2.1).

A natural follow-up question is about how CVC helps to identify new business opportunities (see the question mark in Figure 2.1). In this paper, I argue that it is a learning-through-experimentation story. More generally, this story is in line with the "long-shot bets" feature of VC investments documented in the literature with very few "unicorn" startups reaching big successes [Bergemann and Hege, 2005].

The experimentation hypothesis postulates that CVC investments allow a corporate to experiment various business opportunities before making large-scale investment decisions, reflecting that the manager is uncertain about their final results. Each CVC deal thus could be regarded as an experiment that creates a real option for a potential new line of products or activities [Keil et al., 2008]. Through interacting with the startup managers and participating in the startup's operation in the deal, a CVC parent firm can receive valuable information (called it a signal in this paper) about the future potential of this new business. Crucially, the signal can contain both soft and hard information, which is not available without investments and interactions with CVC-backed startups.[14] A CVC parent finally pins down the best business option according to the various positive and negative signals received from multiple experiments.

In the strategic management literature, this view is supported by Keil et al. [2008] who conducted several interviews of CVC program's senior managers and argue that CVC is a process of "disembodied experimentation" in learning the knowledge from

[14]Keil et al. [2008] argues that the knowledge regarding emerging business from CVC-backed startups are usually non-codified or colloquial information. Accessing the knowledge is possible only if the firm accesses to the community, the VC industry.

CVC-backed startups. In one of Keil et al. [2008]'s interviews, a CVC manager recalls that

> If the [venture] turns out to be something important, you have to put in your own machines (page 1485). Sometimes we just speak up and say: 'That will never work. I have seen it! Guys, that's complete nonsense, I have seen the total opposite [failure] here in a start-up.' (page 1490)

This view is also a good application of Ewens et al. [2018]'s experimentation theory. Ewens et al. [2018] document that many VCs start to conduct a "spray and pray" strategy in response to the reduced cost of initiating businesses in the software and internet-related industry. More specifically, VCs spray their deals to more ventures in the early investment stage and also abandon more when they receive bad signals from startups. Interestingly, the software and internet-related industry is the sector with the most intensive CVC deal activity.

The view is also supported by Lerner [2012] who argues that CVC has the function of leveraging limited resources to pursue or test a variety of technology options. Its cost-saving function is crucial when an established firm needs to test a large number of technology options. CVC also helps to quickly pull the plug of unpromising initiatives in the experiments, while the inside project will never stop optimally as the internal R&D manager has a strong incentive to hide unfavorable signals [Seru, 2014].

Two empirical predictions could be derived from the experimentation view. First, given the purpose of experimentation to resolve future uncertainty and figure out the best growth opportunity, a CVC program should necessarily diversify its deals across technology fields and industries. Therefore, we should expect that CVC programs with higher degree of diversification strategies are more likely to pin down a good business opportunity and ultimately lead to business changes (firm scope changes). Second, firms should conduct "winner picking" in choosing the industry in which they start a new business. Specifically, they should only establish a new division in an industry where they receive positive signals from CVC experiments. Similarly, when a negative signal arrives, they should quickly pull the plug and avoid a "business trap".

2.3 Data and Sample Selection

2.3.1 CVC Sample

The raw data of my CVC sample is extracted from the Thomson Reuters SDC VenturExpert Database. Following Chemmanur et al. [2014] and Ma [2020], I start with a list of 1,248 US corporate-affiliated venture capital firms as reported by VentureXpert.[15] I then manually link these CVC program names with historical names of CRSP and Compustat firms (provided by WRDS) by checking various sources from Google, Factiva, LexisNexis, and PitchBook. This step helps me to identify the corporate parent(s) of each CVC program. As VentureXpert sometimes mislabels some CVC programs as IVCs or other types, I conduct an extensive search among all VC types following Hellmann et al. [2008] and supplement the above beginning CVC list with extra 35 CVC firms. Taken together, I obtain 623 unique CVC firms (programs) affiliated with either CRSP firms or Compustat firms from 1980 to 2017.

In the next step, I impose extra filters on these 623 CVC firms/programs by requiring that (1) the corporate parent of CVC is incorporated in the US and is not operated in financial industries (SIC code starting with 6); (2) only a single corporate parent is matched to the CVC; (3) the CVC program is not initiated by financial division(s) of company, such as GE Capital Equity Group or Exxon Pension Fund, as these CVCs most

[15] In detail, these VCs are Non-Financial Corporate Affiliate or Subsidiary Partnership, Venture/PE Subsidiary of Non-Financial Corporation, Venture/PE Subsidiary of Other Companies NEC, Venture/PE Subsidiary of Service Providers, Direct Investor/Non-Financial Corporation, Direct Investor/Service Provider, SBIC Affiliate with Non-Financial Corporation, and Non-Financial Corporate Affiliate or Subsidiary. In addition, I require the VC firms located in the US.

likely seek financial goals but no strategic goals in their investments.

The final CVC sample contains 497 CVC programs launched by 448 unique public corporations, investing in CVC at least once in the sample period, with around 11,300 deals. Finally, VentureXpert also provides a 4-digit primary SIC code for each startup. The SIC code allows me to match each CVC deal to the Compustat Historical Segment Database (by the SIC-3) and further sort deals into unrelated or related deals.[16] The unrelated CVC deals, those not related to the corporate parent's existing divisions or businesses, account for about 52% of total CVC deals sample.

Figure 2.2 plots the annual aggregate CVC investments by US public (non-financial) corporations in Compustat database. Investments are measured by (1) the number of deals (left axis) and (2) the fraction of deals among all VC deals (right axis).

2.3.2 Sample for Firm Scope Change

Textual Data on Emerging Business

To obtain a textual measure capturing time-varying emerging businesses in the US economy, I combine two text sources from VentureXpert and from the SEC digital filing system (EDGAR).

First, I download a detailed business description for each US-based VC-backed startup from VentureXpert.[17] I group a set of startups' detailed business descriptions into a yearly single corpus, where the set contains all active VC-backed startups receiving VC funding in a given year. I drop common words and stop words and form short phrases

[16] An unrelated CVC deal is defined as a deal with the entrepreneurial company's SIC-3 code not matching with any SIC-3 codes of CVC corporate parent. A conglomerate firm has multiple SIC-3 codes, whereas a stand-alone firm has a single SIC-3 code.

[17] One caveat of this approach is that those startups' business descriptions are not historical but are updated to the date of data downloading.

(each containing two single words) for any two adjacent words in each sentence.[18] Next, I define each year's "emerging phrases" set as those short phrases that are most widely used by the VC-backed startup community during that year. More precisely, I select the top 5% most frequently-used short phrases from the yearly startup corpus. Approximately 30 short phrases that represent too general businesses (for example, "business service" or "product service") are excluded manually.

Ideally, each emerging phrase represents an emerging business that is popular among the startup community. I call them "emerging phrases" under the implicit assumption that any popular businesses in the VC industry should be novel and emerging relatively to any businesses of US listed firms. Figure 2.3 shows two examples of the emerging phrases set in 2000 and 2017 using word clouds. As shown in Panels A and B, emerging phrases significantly evolve over time. The emerging phrase often relates to "internet" and "e-commerce" during the 2000s (the internet bubble period), while, in the most recent year of my sample, more "tech buzzwords" are included, such as "artificial intelligence", "virtual reality", "online platform", and "digital health". More words clouds of emerging phrases are plotted in the Online Appendix.

I then search for these emerging phrases in listed companies' business descriptions. I obtain the US public listed firms' business descriptions from the annual 10-K filings following Hoberg and Phillips [2016]. First, I download all the 10-K filings from the SEC EDGAR system with Python automation scripts. Following Hoberg and Phillips [2016], I extract the Item 1 (Business Description) as my text source of firms' business in my regression sample, including those 450 US firms with CVC investments and other firms without CVC deals.

In the main analysis, I search the emerging phrases in each 10-K filing in order to identify any emerging businesses that are newly integrated by CVC parents as well as by other public listed firms with no CVC investments. The detailed procedures are in

[18] The online appendix lists those common words and stop words. Stop words are mainly from the NLTK.

Section 2.4.1.

Compustat Segment Data

To obtain traditional measures of firm scope changes, I begin with all firms and their segments listed in Compustat Historical Segment Database for 1980-2017.[19] For each segment observation, I require that the segment primary SIC code (item $SICS1$) is not missing, as well as the segment sales being non-negative. Next, within each firm, I aggregate sales and total assets of raw segments by SIC-3 industries. I refer to the resulting firm-industry-year observations as the divisions. Thus, a division is equivalent to operating in the associated SIC-3 industry.

Next, I construct three dummies, one dummy for creating a new division, one for removing an obsolete division, and one for the change of corporate primary industry, using the divisions reporting information of each firm. Establishing a new division is identified if the firm reports a new division with its SIC-3 code appearing in the first time in the company history. In other words, a division creation means that the firm steps into a new industry for the first time. Similarly, removing an old division means that a firm stops reporting a division with a certain SIC-3 code and this division is never reported again.[20] Finally, the primary corporate industry is defined as the industry of the division with the largest sales in the year.[21]

Of course, I am fully aware of the potential drawback of using Compustat Segment Data to capture business changes. As recently argued by Hoberg and Phillips [2021], firms tend to under-report their true industry scope in the Compustat Segment Database.

[19] Following the recent conglomerate study by Matvos et al. [2018], I include only business segments during the data retrieving and further keep only observations of unrestated segments by choosing the SRCDATE that exactly matches to DATADATE.

[20] In the creation and removal of the division, I do not consider the temporary change (turnover) to reduce the noise of the measure.

[21] I do not choose the Compustat company-level historical SIC code in gauging the industry change since, in the full Compustat sample (not CRSP/Compustat), the item $SICH$ is missing for about one-third of the firm-year sample.

Moreover, the recent SFAS 131 regulation change in 1997 requires that managers report segments based on how managers themselves internally evaluate operating performance (management approach). Prior to this rule change, segment reporting was instead based on an industry approach. Therefore, the newly established division before and after 1997 might not be comparable, and we should expect more new reported divisions after the 1997 fiscal year.

To tackle these different challenges, I split the sample into the periods before and after 1997 and conduct robustness checks for the two subsamples. Furthermore, I replicate and obtain Hoberg and Phillips [2021]'s textual segments (divisions) based on the overlap between 10-K Item 1 business description and SIC (NAIC) industry description. The textual segments are immune from the criticism as mentioned above. In the Online Appendix, I show that my results do not change if I use the textual-based segments to construct dummies of the firm scope change.

2.3.3 Other Data Sources and Summary Statistics

As discussed in the Introduction, this paper uses two different identification strategies to estimate CVC's impact on firm scope change: the fund inflow shocks of independent venture capital firms and the US non-stop airline routes. First, I obtain information regarding independent venture capital firms again from SDC VentureXpert. I measure and proxy the fund inflow shocks using data from SDC VentureXpert and PitchBook. Second, the US non-stop airline routes are downloaded from the T-100 Domestic Segment Database in the United States Department of Transportation. Furthermore, I get the distribution of US metropolitan statistical areas (MSA) from the US Census Bureau.

Table 2.1 Panel A presents the summary statistics of the firm-year sample, the sample used in most regression analysis. Following CVC literature (Ma [2020]), industries (3-digit SIC) with no CVC activities during the whole sample period are excluded entirely.

All variables (except dummies) are winsorized at the 1% and 99% levels. Key variable constructions are illustrated in the Appendix 2.9.1. As shown in Table 2.1, firms with CVC investments are larger in firm size, are more profitable measured by ROA, and more likely to be a conglomerate.

2.4 CVC and Change of Firm Scope

To start, I provide some strong suggestive evidences about CVC and firm scope changes. The main challenge of these tests is a lack of proper metric on the change of firm scope. Even if one might expect that CVC firms would integrate emerging businesses following CVC investments, how could we empirically measure it? Section 2.4.1 provides the first answer using the text-based "emerging phrases", while Section 2.4.2 provides a second answer based on the traditional Compustat Segment measures. Lastly, I leave the identification for causality in the next section.

2.4.1 Evidence on Emerging Business

Given the hypothesis development in Section 2.2, it is ultimately an empirical question whether CVC leads to changes in firm scope. One way to capture firm scope change is to gauge the emerging business integration by CVC parents as well as by their industry peers using textual analysis. I estimate the following regression,

$$EmergingPhrases_{i,t+1} = \beta D(CVC)_{i,t} + \gamma \mathbf{X}_{i,t} + \upsilon_t \times \iota_j + (\tau_i) + \varepsilon_{i,t} \tag{2.1}$$

where $EmergingPhrases_{i,t+1}$ denotes the number of "emerging phrases" – those top 5% business short phrases most popular among the startup community (see Figure 2.3) – that are newly added into the Firm i's 10-K Item 1 (business description) in Year t+1. (By saying "newly added", I mean that the phrases are found in Year t+1's 10-K but not in

Year t's 10-K.) Intuitively, this measure captures newly added businesses that are new relatively to the CVC parent's old businesses and also new to the whole US economy.[22] D(CVC) is a dummy equal to 1 if the firm conducts at least one CVC deal in Year t. Firm-level controls (**X**) include Firm Size, Tobin's Q, ROA, R&D, Leverage, Capx., HHI, D(Conglomerate), Firm Age, as well as two mechanical textual measures: the number of any new short phrases appearing in the 10-K Item 1 and the total length of 10-K Item 1.

Before delving into regression results, I illustrate the regression design in Figure 2.4. Take Google as an example. Suppose the Google CVC program (Google Venture) invests in startups in 2016, and during that year, the set of emerging phrases includes "virtual reality", "digital health", and "smart home". Then I search in Google's 2017 10-K these three emerging phrases. The dependent variable thus counts the number of 2016 emerging phrases newly added in 2017's 10-K. The intuition is that, when Google invests in CVC in 2016, it helps Google to identify new business opportunities such as digital health, and one year after investment (2017), Google should be more likely to add it into its own portfolio of businesses.

Table 2.2 corroborates that CVC investments are strongly correlated with adding new emerging phrases. As shown in Column (1) (Column (4)), on average, a CVC parent will add 0.78 (0.68) more emerging phrases compared with its industry-year peers in the first year (second year) after investment. This amount of increase translates into 100% of the sample average (0.75). In Columns (2) and (3), I split D(CVC) into two dummies, D(CVC Unrelated) and D(CVC Related), according to whether the startup in the deal is related to the parent firm's current business. The regressions suggest that both related and unrelated deals could lead to the emerging business integration, with the effect being stronger for related ones in Year t+1. Finally, the result is very robust with firm fixed effects.

[22] Those popular businesses in the VC-backed startups' community should be new and emerging. Otherwise, there is no reason that VC will invest in those companies. The emerging characteristic could be a new technology, a new business model, or a new industry or product.

Figure 2.5 turns to scrutinizing the usage of new "emerging phrases" in the years around each CVC deal. The point estimates (from OLS) and confidence intervals are taken from the following regression specification,

$$EmergingPhrases_{i,t} = \sum_{k=-3}^{+5} \gamma_k D(CVC\ Unr;k)_{i,t} + \sum_{k=-3}^{+5} \alpha_k D(CVC\ Rel;k)_{i,t} + \beta \mathbf{X} + \tau_i + v_t + \varepsilon_{i,t} \tag{2.2}$$

$EmergingPhrases_{i,t}$ simply counts the number of newly added emerging phrases in Year t's 10-K. $\{D(CVC\ Unr;k)\}_{k=-3}^{+5}$ denotes a set of nine dummies in the [-3, +5] year window around each unrelated CVC deal. As an example, $D(CVC\ Unr;+3)$ is equal to 1 if the firm-year observation is the third year after an unrelated CVC deal. A similar set-up applies to $\{D(CVC\ Rel;k)\}_{k=-3}^{+5}$ for CVC related deals. τ_i and v_t are firm and year fixed effects, respectively.

Figure 2.5 shows that the "treatment" effect of CVC deals mainly lies within the two years after investments, whereas there is no significant effect before and three years after the investment. This is intuitive since old CVC deals (say the deals in 2010) could not help identify any current emerging business opportunities in 2014, and neither do future deals in 2018. In other words, CVC deals perform the best in helping identify contemporaneous business opportunities.

Next, do CVC parents add new emerging phrases that directly correspond to the specific CVC deals? For example, CVC deals related to artificial intelligence should predict the adding of "artificial intelligence" into the 10-K. Table 2.3 answers this question. First, I sort each emerging phrase into one of the eight VentureXpert VEIC industries according to the industry of those startups that use the emerging phrases to describe their businesses.[23] As shown in the main diagonal, CVC deals in Industry j usually predict the Industry j specific emerging phrases newly added into the firm's 10-K annual report.

[23]This sorting is non-exclusive. Take an example: Artificial Intelligence is sorted into both Computer Software and Internet Specific industries.

Finally, a natural concern is how long a CVC parent retains emerging phrases in its subsequent annual 10-Ks (after adding them). Panel C and D of Figure 2.3 answer the question, where Panel C plots the words cloud of the top 50 frequent emerging words newly added into CVC parents' business descriptions within the three years after investments.[24] Consistent with the intuition, more general phrases (such as information technology) are more likely to be added than those tech buzzwords across the CVC firms sample.[25] Furthermore, Panel D plots the distribution of years of survival for each emerging phrase after being added into the business of a CVC parent. On average, each phrase survives in the next 2.5 years, with more than 75% at least surviving in the next annual 10-K report.

2.4.2 Evidence on Dynamics of Firm Segments

An alternative method to measure firm scope change is to explore the Compustat Segment data. The following logit model thus examines whether CVC leads to future firm scope changes using division (segment) measures. The empirical model takes the following form, on the firm-year panel with all US public firms in non-financial industries,

$$D(Scope\ Change)_{i,t+1:t+k} = \beta D(CVC)_{i,t} + \gamma \mathbf{X}_{i,t} + \upsilon_t \times \iota_j + (\tau_i) + \varepsilon_{i,t} \tag{2.3}$$

where $D(Scope\ Change)$ corresponds to three different dummies for firm scope changes as illustrated in Section 2.3.2. Regarding establishing new divisions and removing obsolete divisions, I examine them within the next two years after each CVC deal, whereas I identify the change of primary corporate industry (SIC-3) in the next three to five years. Later, I will provide evidence about why I choose those specific intervals. The regression sample is further adjusted to alleviate the potential survivorship bias.[26]

[24] There are 2,081 emerging phrases added by CVC parents, with 512 unique phrases.

[25] Many words in Panel C also relate to software and the internet, consistent with the fact that around 50% of CVC-backed startups operate in the SIC-3 737 industry, the software service industry.

[26] Specifically, in Panel A of Table 2.4, I require that each firm observation survives, at least, in the next two years. For panel B, I require each firm observation to survive, at least, in the next five years.

Table 2.4, Panel A (Columns 1 – 3) investigates post-CVC division creations, where the dependent variable is a dummy equal to 1 if the firm creates at least one new division (in a new industry) within the next two years.[27] In Columns (2) and (3), I split the D(CVC) into two dummies, D(CVC Unrelated) and D(CVC Related), according to whether the startup in the deal is matchable to the CVC parent's business using SIC-3 codes.[28]

In Panel A, the coefficients of both D(CVC) and D(CVC Unrelated) are positive and statistically significant, but not the coefficient of D(CVC Related). It implies that firms are more likely to create a new business (in a new industry) within the next two years following unrelated CVC investments, consistent with the prediction in Section 2.2. Unrelated CVC deals help its corporate parent to identify new business opportunities outside its current business domain and further prompt the firm to integrate the new business.

The marginal effect is very significant: the probability increased by conducting CVC unrelated deals is about 4.91%, equivalent to 57% of the unconditional probability of creating new divisions in the sample.

The finding that related deals have no impact on division creation is simple: the dummy-version firm scope change is a pretty coarse measure, without capturing granular changes of businesses within a SIC-3 code. Therefore, even though related deals are strongly associated with business changes in firm's current domain when measured by rather granular "emerging phrases", the same effect does not show when applying segment dummies in Table 2.4.

Turn to Columns 4-6 in Panel A, I obtain a similar pattern on post-CVC divisions removal: firms are 2.73% – 5.02% more likely to remove existing divisions within the next two years following CVC deals. Again, the impact is only confined to unrelated deals.

[27] See Section 2.3.2 for details.

[28] Furthermore, in Columns (1) and (2), I add the high dimensional industry by year fixed effect to absorb any industry shocks driving the firm scope change, as studied in Harford [2005a] and Maksimovic and Phillips [2008], while the firm fixed effect is controlled in Column (3).

In Panel B, the dependent variable is instead a dummy of changing a firm's primary corporate industry in the next three to five years (in the next four to six years in Columns (4) to (6)). As shown by the coefficients, CVC investments (only unrelated deals) significantly lead to the future change of the primary corporate industry. If I instead identify industry change within the next two years, there is no effect. It suggests that any change of the main industry takes longer time than division creation or removal. This timing makes sense since it takes time for the newly established business to grow and become the core business. Indeed, as shown in Panel C of Table 2.1, there are 104 industry changes taking place within 3-5 years following unrelated CVC deals, and 43 of them are attributable to the continuing growth of the newly established division which finally turns to the new primary business.

To investigate the choice of those specific intervals of the scope change dummies, Figure 2.6 studies the scope change activities in the [-3 Year, +5 Year] window around CVC investments. The regression specification is the same as Figure 2.5. For simplicity, only the coefficients of unrelated CVC deals are plotted since I do document that only unrelated deals impose significant impacts in Table 2.4.[29]

The result of Figure 2.6 closely mirrors Table 2.4: CVC investments lead to division creations and terminations in the next three years and further push the changes of the industry in the fourth to the fifth year. Nevertheless, there is no pre-CVC increase or decrease in the three years before. The joint tests of the difference between the coefficients of three years before and three years after in Panel A and B are significant: $p = 0.0279$ and $p = 0.0166$, respectively. Interestingly, division removal seems to be usually concomitant with division creations, which appears to be an indicate that CVC investments accelerate the process of creative destruction, with firms adjusting their portfolio of activities to changing technologies and markets.

[29] However, the complete estimate results are shown in the online appendix.

2.4.3 More Discussions

All told, the launch of a CVC program is associated with the firm scope change of its parent firm using both measures. However, potential endogeneity issues still could contaminate the baseline results. There are three different sources of endogeneity. First, firms with some obsolete businesses or technologies are more likely to leverage on CVC to obtain new ideas, which finally leads to firm scope changes [Ma, 2020]. Second, in contrast to the story that CVC facilitates to identify new business opportunities, the emergence of new technology or new business opportunities might incentivize the manager to invest in CVC (reverse causality in the first arrow of Figure 2.1). Third, after deciding to enter the CVC foray, choosing between CVC-related and unrelated deals is endogenous.

Unfortunately, the CVC literature is silent on tackling this kind of complicated endogeneity. Therefore, I introduce a new IV strategy using independent VC firm's fund inflow as the source of exogenous variation in Section 2.6. But before elaborating this IV, I will first study how CVC spurs to identify new business opportunities. In fact, it also helps to address some aspects of endogeneity concerns.

2.5 Experimentation and Firm Scope

This section explores how CVC could help identify new business opportunities and ultimately spur firm scope changes. To summarize the story in one sentence, CVC is a learning-through-experimentation process. In this process, a CVC parent firm usually sprays VC deals across various technology or business options (this is in the same spirit of the "spraying" strategy in Ewens et al. [2018]), then waits for signals revealing potential of options, and finally responds to the signals with its decisions where to launch new business activities.

2.5.1 Diversifying CVC Investment Strategy

As developed in the hypothesis section (Section 2.2), two empirical predictions can be derived from the experimentation hypothesis. First, given the purpose of experimentation to resolve future uncertainty and pin down the best growth opportunity, a CVC program should necessarily diversify its deals across technology fields and industries, which optimally increases the probability of discovering promising business opportunities. If the experimentation is indeed the underlying mechanism (shown in Figure 2.1), we should find in the data that this diversifying CVC strategy, a necessary step of experimentation and tackling with uncertainty, is strongly linked to future firm scope changes. Furthermore, an underlying prediction of the experimentation story is that, before the CVC investments (and of course before CVC program launch), a CVC parent usually does not clearly know which direction is the best for its future business expansion.[30]

Table 2.5 aims to test this diversification hypothesis. First, I define a new dummy variable of CVC investments, D(CVC Past 3yr), by grouping all CVC deals in the past 3-year period of the firm. Precisely, the dummy is equal to 1 if the firm conducted at least one CVC deal within the past three years. Choosing three years follows Ma [2020]'s finding that a typical CVC program lasts three to four years. Then I interact this new dummy with two new variables, measuring to which extent a CVC parent firm diversifies its CVC deals across ten detailed VentureXpert industries (VEIC Industries).[31] Inverse HHI(VEIC) is calculated as one minus the HHI of the past three-year CVC deals across the VEIC industries, while Num(VEIC) counts the number of VEIC industries covered by the firm's past 3-year investments. Furthermore, I also control the number of deals within the past three years.

Table 2.5 shows that the interaction term between D(CVC Past 3yr) and CVC diversi-

[30]The alternative story is that, before the CVC investments, the firm clearly knows the future direction of its business growth, such as AI, and therefore the firm chooses to only invest in AI.

[31]VentureXpert assigns startups to its own industry classification called VEIC. All results are robust if I instead choose a more granular VEIC industry classification.

fication measure (Inverse HHI (VEIC) or Num(VEIC)) is always positive and significant, implying that firms with a higher degree of diversification are more likely to conduct division creation and industry change (Column (1) – (4)) and add new emerging phrases (Column (5) – (6)). In contrast, conducting more CVC deals does not guarantee success in identifying new businesses since the interaction term between CVC dummy and Num(CVC Deals) is not always positive.

In conclusion, Table 2.5 supports the experimentation story via diversification strategy. Furthermore, it implies that, before a CVC program launch or during the CVC investments, the parent firm is unsure about the growth direction. Perhaps the strongest result lies in Columns (3) and (4) of Table 2.5. If the firm already knows to which industry it will shift the business, there is no need to diversify deals across industries ex-ante but instead to focus deals in the predetermined industry. This evidence helps to partially rule out the reverse causality concern that it is not CVC that identifies business opportunities but that firms first observe business opportunities and then decide to invest in CVC.

2.5.2 CVC Signals and Firm Scope Change

Having documented the diversifying investment strategy, now I turn to the role of CVC signals in industry choices for new division creations. Intuitively, a CVC parent firm will not establish a new division in every industry where it sprays CVC deals; instead, only an industry with positive post-investment signals (received from startups) is considered. This feedback loop of information gleaned from CVC investments is a key feature of experimentation. In other words, if firm scope changes are driven by experimentation, I should find in the data that firms react to the positive and negative information updates (signals) they receive from their CVC investments in their decisions to launch new activities and divisions.

Introduction to a New Discrete Choice Model

Table 2.6 and 2.7 develop and estimate a McFadden discrete choice model regarding industry choices for division creations, therefore providing some insights into this CVC signal hypothesis. First, I introduce the empirical model setup and then turn to the testing of CVC-signal responses.

The observation unit is at the firm-year-industry level, where each observation represents an alternative (Industry *j*) in which Firm *i* in Year *t* could choose to create a new division. The set of alternatives (choice set) consists of 404 non-financial SIC-3 industries documented at least once in the Compustat Historical Segment database from 1980 to 2017.

I only include firms having invested in at least one CVC deal during the sample period. For each decision-maker (a firm-year pair), I drop those alternatives (industries) that already exist as a division of the firm in Year *t*-1 or those that have already been created before Year *t*. In the model, the dependent variable of interest is a dummy equal to 1 if the alternative industry is chosen by the Firm *i* in Year *t* to establish a new division.

Some Basic Facts

Table 2.6 starts to explore some basic features of the division creation process, while Table 2.7 focuses on signal responses. In Column (1) of 2.6, I start to introduce a crucial control variable, D(CVC 3yr), a dummy equal to 1 if, within the past three years, the Firm *i* has invested CVC deals in that industry (with the invested startup in Industry *j*). The positive and strongly significant coefficient shows that a CVC parent often creates a new division in the industry where it has sprayed CVC deals in the past.

In Column (2), I interact D(CVC 3yr) with two industry proximity measures. D(Ind. Proxy SIC1) and D(Ind. Proxy SIC2) both capture the industry proximity between the alternative and the industries of existing divisions of Firm i in Year t-1.[32] The coefficients of these two proximity measures are both positive and significant, which implies that, in general, firms are more likely to establish a new division close to their existing business domains. This is possibly due to higher asset redeployability or closer product language usage [Hoberg and Phillips, 2018]. In contrast, the interaction term is negative and highly significant, showing that CVC usually spurs its parent to create a new division far away from its current business domains.

Evidence on CVC Signals

Next, I turn to Table 2.7 to test the CVC signal hypothesis, where I interact D(CVC 3yr) with each signal variable iteratively. However, it is challenging to proxy the signal variable since researchers are not able to observe the information transmission from startups to a CVC parent firm. For example, to which extent the potential new business will fit with the parent's old business is a typical dimension of soft information only observed by insiders.

As a result, in Panel A of Table 2.7, I use startup's measurable performances as the proxy of signals. Importantly, each signal variable is observable to the CVC parent after the investment but not before. For example, in Column (1), the signal is measured by the number of startups in the parent's CVC portfolio which finally exit through IPO. In other words, the signal variable is based on information from the past three-year CVC investments in Industry j. To illustrate, if Google Venture has invested five startups in the past three years of Year t in Industry j, and finally, three of them go public (IPO), then the signal variable is equal to 3. The number of deals is 5, which will also be controlled in regressions. One important assumption I have to make here is that, since the IPO date of these three startups will be naturally after Year t (the decision-making year of

[32] D(Ind. Proxy SIC2) is a dummy equal to 1 if the alternative has the same 2-digit SIC with one of the existing divisions of Firm i. Similarly, D(Ind. Proxy SIC1) is a dummy equal to 1 if the alternative has the same 1-digit SIC with one of the existing divisions of Firm i but does not have the same 2-SIC with them.

division creation), Google could not directly observe that the signal is equal to 3 at the time of division creation decision. But Google is able to draw valuable information on the promise of its investments in these five startups (from taking board seats, participating in operational management, talking to syndicated VCs), which is supported by Bergemann and Hege [2005] and Dushnitsky [2012].[33] The private signal obtained by Google is proxied in my empirical strategy by the eventually observed IPO outcome. In Column (1) of Panel A, consistent with my hypothesis, firms are more likely to create a new division when they receive a positive signal from their past three-year investments in the relevant industry. The coefficient in Panel A Column (1) translates into a 120% increase of unconditional probability of establishing new divisions by one standard deviation increase of the IPO signal.

Moreover, I control the number of startups invested in the past three years in Industry j and denote it as $Num(Startups\ Invested)$. It is essential since, naturally, the more you invest, the more IPO startups you will have. In Panel B, I define all signal variables as the form of fraction, such as the fraction of IPO startups, but do not control $Num(Startups\ Invested)$. One might also argue that the IPO signal variable might proxy industry-year general IPO trends or clusters, and thus I control the industry-year IPO trend as a separate control, as well as its interaction with the CVC dummy. The negative and significant sign of the interaction term shows that CVC parents usually do not over-react to the IPO industry cluster like non-CVC peer firms (captured by the non-interaction, the IPO Cluster), but only respond to their deal-specific signals.

Similarly, I construct signal variables from the startup's acquisition, bankruptcy, and patent information using a similar method as in the construction of IPO signal. In summary, CVC parent firms react to positive signals from IPO and patent growth, the negative signal of bankruptcy, but not the acquisition of the startups by third-party.[34] Column

[33]This also implies that the signal is private information for Google and not observable to Microsoft in Year t since the three IPO startups have not yet exited through IPO in Year t.

[34]This is consistent with Kerr et al. [2014] stating that most startups acquisitions (missing transaction value in the dataset) are fire sales, i.e, the startup failed. I find positive and significant coefficients in

(4) of Panel A and Column (3) of Panel B also document that the direct acquisition of portfolio companies in the CVC investments could positively predict the division creation. It is consistent with the intuition that the CVC firm understands the capability needs for adding new businesses and directly builds the capability through acquiring startups [Keil et al., 2008].

Lastly, one might worry about reverse causality that a firm with superior technology in an industry in which the firm is planning to create a new division might be more likely to nurture successful startups. However, this possibility is immediately in contradiction with the conclusion in Table 2.6: that is CVC firms usually create a new division distant from their existing business in terms of industry distance. This implies that it is highly implausible that the CVC firm owns superior technology in a distant industry where the firm has no existing business.

2.5.3 CVC Signals and Emerging Phrases

Turning to emerging phrases. By similar reasoning, CVC parent firms will not integrate every emerging business they have invested in through CVC programs; instead, they pick winners with positive signals.

Table 2.8 thus estimates a similar discrete choice model for the industry choice of emerging phrases added into annual 10-K reports, where each emerging phrase is sorted into eight VentureXpert Industries (VEIC). However, strictly speaking, it is not a discrete choice model since a firm could add emerging phrases in multiple VEIC industries simultaneously, which is very rare in the case of division creation.

In the choice model, the unit of observations is at the firm-year-VEIC level. I sort each emerging phrase defined in Section 2.3.2 into 8 VEIC industries following the procedure

Column (3) of Panel A when I only focus on the sub-sample of acquisition with above-median IRR (IRR = Acquisition Transaction Value / The Sum of Total Round Amount).

of Table 2.3. The dependent variable of interest is the number of VEIC-*j* specific emerging phrases newly added into the firm's 10-K in Year *t*. Panel A studies the basic discrete choice model, while Panel B focuses on CVC signals and responses. In Panel A, the key control variable, D(CVC VEIC *j*), is defined as a dummy equal to 1 if the firm has invested startups in the VEIC-*j* industry within the past three years. The regressions show that firms usually add industry-specific emerging phrases following their industry-specific investments. More specifically, CVC parents do not add phrases in each industry. They absorb emerging words only from Biotechnology, Communication, Computer Software, Internet Specific industries, and others.

Panel B repeats the same exercise as in Table 2.7 by interacting the CVC dummy with the signal variables mentioned above. Interestingly, all results are exactly the same as those in Table 2.7, suggesting that, in the decision of establishing new divisions and adding emerging businesses, firms react to the same set of signals.

Finally, some remarks on the tests are in order. First, this section could not rule out other underlying mechanisms explaining how CVC could spur findings of new business opportunities and, subsequently, change of firm scope. One alternative explanation is the network effect. By accessing VC communities and building the network with IVCs, CVC firms could accumulate substantial "connections" helping them find new ideas, technology, and businesses in the market. This idea is partially tested in the next section when I formally propose a new IV strategy. Second, one might ask why CVC firms would conduct experimentation strategically instead of passively learning from successful startups. Alternatively, they could conduct firm scope change following the IPO winners even without CVC investments. The simple answer is that CVC firms would take advantage of "first mover" not until startups capture a significant market share and severely disrupt the CVC firm.

2.6 Exogenous Variation on CVC Experimentation

It is very challenging to find any exogenous variations on a CVC program initiation. This is implied by a fact that there is no attempt to provide any identification strategies for the CVC program launch in current finance and strategic literature.

To have the first attempt, I introduce an exogenous variation on the continuation of CVC investments conditional on that the CVC program has already been started. In Section 2.6.1, I introduce this instrument and report the estimate, Section 2.6.2 offers more discussion about its validity.

2.6.1 Identification Strategy with IVC Fund Inflow Shock

I exploit fund (capital) inflow shocks of independent venture capital firms (IVC) in each CVC program's past syndicate network. Notably, those fund inflow shocks are idiosyncratic, being orthogonal to aggregate shocks in the VC industry. An example can be a pension fund that injects a large amount of capital into a non-star VC during a non-bubble period.

The instrument works on a small sample of US public firms already starting the foray of CVC investments in the past. It relies on the VC literature about syndicating investments. First, the syndicating investment and its network formation are common in the venture capital world, and many VC firms commonly invite their past syndicating partners to join in their new investments [Hochberg et al., 2007]. Second, the IVC is the most crucial channel of deal sourcing for CVC firms, as documented in Sykes [1990] and MacMillan et al. [2008], among others.

Based on the two premises, an idea of the instrument is that, if an independent VC firm j (IVC j) receives a positive fund inflow shock today, and meanwhile, a CVC Firm

i is in its past syndicate network, then the IVC j is very likely to initiate new deals and invite CVC Firm i, its old partner, to join in its new investments. Alternatively, IVCs can recommend new deals to CVCs when IVCs start new funds and seek deals. As a result, the new investment of CVC Firm i is driven by IVC's idiosyncratic fund inflow shocks instead of the CVC firm's product life cycle and other unobserved corporate strategies.

To facilitate the understanding, consider an example illustrated in Figure 2.7, showing how its IVC partners drive the CVC investment decisions of Apple Inc. In the past five years before 1990, Apple Inc has built three connections with three distinct IVCs through syndicate investments. Among the three IVCs, two received positive inflow shocks in 1990. One of these two IVCs, Mayfield Fund LLC, then spent its new money on investing in a seed-stage startup called BioCAD Corp in 1990, followed by Apple Inc's joining due to Mayfield's invitation.[35]

To construct this instrument, I proceed with two steps. First, for each CVC Firm i in Year t, I obtain its past five-year syndicate network by searching all IVCs that have co-invested with Firm i within the past five years. The co-investment (syndication) is defined as a scenario in which CVC Firm i and IVC Firm j invest in the same round of the same startup k [Hochberg et al., 2007]. In the second step, for each IVC in the network, I check whether it receives a positive fund inflow shock in Year t.

Here, the main challenge is obtaining the IVC's fund inflow shock exogenous to any VC investment opportunities and any technology shocks. I construct it following the recent Granular IV approach developed by Gabaix and Koijen [2020]. First, I proxy an IVC's raw capital inflow by its raising of new follow-on funds since (i) fundraising is usually accompanied by the largest capital inflow, and (ii) when an IVC starts a new follow-on (sequential) fund, it is more likely to invite CVC firms to join its new deals.

[35]Another example of the invitation is that, between 1994 and 2000, Cisco Systems (a large industrial firm) was invited into 13 syndications led by Sequoia Capital (an independent VC firm), as documented in Ferrary [2010].

Next step, I estimate Gompers and Lerner [1998]'s fundraising model with plenty of VC funding factors and VC organization controls, along with high dimensional fixed effects. I obtain the idiosyncratic fund inflow shock from the error term of the fundraising model. Appendix 2.9.2 shows the detailed procedures, estimated results, and error terms' properties. In the last step, I sum up the error term (the idiosyncratic shock) across IVCs in each CVC program's network and define it as my Granular IV.

The intuition behind my Granular IV is similar as in Gabaix and Koijen [2020]. Gabaix and Koijen [2020] argues that the Granular IV heavily relies on the "unexpected" change in the loading on a common shock. If OPEC decided to cut down oil productions, but Saudi Arabia cuts down more than anticipated, that is an idiosyncratic shock. The same argument applies to the idiosyncratic capital inflow shock of IVCs.

Table 2.9 reports the first stage regression where I use the Granular IV (sum of the idiosyncratic fund inflow shocks) to instrument the continuation of CVC investments by each CVC program. I restrict my analysis to a small sub-sample of CVC firms having already initiated a CVC program in the past five years before and thus enjoy some VC networks today. In the regressions, I control the size and quality of the past IVC network, given that the network (past investments) is endogenous. Finally, Table 2.9 shows that the sum of IVC's idiosyncratic fund inflow shocks highly predicts the new CVC investments for both general deals and initial deals (no follow-on deals).[36]

Regarding the exclusive condition, a potential concern is that some specific industry (technology)-year shocks might drive both the fund inflow shocks (new VC fundraising) and firm scope changes. For example, the introduction of cloud computing services by Amazon, studied in Ewens et al. [2018], might push many past-connected VC firms to launch new funds and to invest in e-commerce startups. Many established firms in the retail sales industry might follow the technology shock and start creating a new division

[36]Initial deals are those deals in which the CVC firm invests in a specific startup for the first time, i.e., not the follow-on investments. The number of initial deals better measures the impact of GIV on the deal sourcing availability of CVC firms.

regarding e-commerce.

To mitigate this concern, I always include the industry (SIC-3) by year fixed effects in both the first and second stage regressions. Furthermore, when estimating Gompers and Lerner [1998]'s fundraising model (where I get the error term and thus the shock), I add both the VC industry specialization by year and VC location by year fixed effects.[37]

Equipped with the instrument, I conduct 2SLS regression by instrumenting the number of CVC initial investments (with natural logarithm) with the Granular IV and report the results in Table 2.10. Columns (1) to (3) analyze the text-based scope measures, whereas the segment measures are used as the dependent variable in Columns (4) to (6).

In Column (2), I introduce a new textual measure, the Business Change, which is another granular measure of the CVC parent's business change. Following Hoberg et al. [2014], it equals one minus the cosine-similarity between the firm's year t and year t+1's business descriptions.[38] I document a strong and positive effect of CVC on firm scope change (3.77% change of Cosine similarity) and adding 0.85 more emerging phrases.

As shown in Columns (4) to (6), CVC investments impose a positive and significant impact on division creation and industry change but not division removal. For example, one standard deviation increase of Num(CVC Initial Deals) leads to about 6% of probability increase of establishing a new division in the next two years.

[37] To further provide the deal-level evidence of my instrument (the evidence that IVCs do invite CVC), I estimate a discrete choice model (McFadden [1973]) (in the online appendix) regarding the choice of portfolio companies by CVC programs. The empirical model shows that CVC does follow the choice of picking startups by its past-connected IVC partners, especially when the latter receives positive fund inflow shocks.

[38] In unreported results, I find that the segment dummies are all strongly and positively correlated with the new textual measure.

2.6.2 Further Discussion

Two upshots deserve further clarification. First, as my instrument relies on the argument that IVC invites CVC or at least recommends deals to CVC after receiving fund inflow shock, one might worry that the counter-hypothesis that CVC invites IVC might also happen, which could dampen my instrument.

Nevertheless, this hypothesis is not supported in both the data and survey evidence. In most syndicating cases between CVC and IVC, the IVC usually leads the deal, while the CVC does not lead. And only the leading investors invite others to join the deal. Moreover, MacMillan et al. [2008] summarizes the CVC-IVC syndicate as follows: "CVCs and independent venture capital often co-invest in companies through syndicated investments. The independent venture capital investor usually takes the role of lead investor. CVCs benefit from access to the investment 'deal flow' of independent venture capital, while independent venture capital benefits from strategic insight and technology expertise provided by CVCs."

Second, one might argue that the invitation from IVCs to CVCs is endogenous (depending on CVC's technology expertise), as well as the decision regarding whether CVC accepts the invitation. However, notice that the instrument purely relies on the fund inflow shock and does not hang on the two aforementioned decisions. Thereby the instrument is valid as long as the idiosyncratic fund inflow shock is truly exogenous.

2.7 Additional Analysis

2.7.1 Alternative Identification: Evidence from US Airline Route

The new section starts with an alternative identification which serves for two purposes. First, previous analyzes are agnostic about the treatment intensity of CVC deals. Second, one major caveat of my Granular IV is that it fails in distinguishing between related and unrelated CVC deals. Therefore, this section provides an alternative identification exercise by exploring the US airline route in Bernstein et al. [2016], thus focusing exclusively on the unrelated CVC deals and providing new insights on those two aforementioned questions.

To start this analysis, I first gather all *unrelated* CVC deals with both the startup and the CVC firm located in the US.[39] The deals range from 1990 to 2017, the interval in which I can obtain both the US airline route data and Compustat Segment data. Next, I classify all unrelated deals into two groups according to whether there is a direct (non-stop) flight route, during the year right after the deal year, between the metropolitan statistic area (MSA) of the CVC firm and of the startup.[40]

Following Bernstein et al. [2016], the idea is that a higher frequency of non-stop flights between CVC and the startup's location provides more chances for the CVC manager to visit the startup and subsequently acquire knowledge regarding the startup and its emerging business opportunities behind it.[41] Therefore, the CVC parent should be more likely to conduct firm scope changes after a more frequent interaction with the invested startup.

[39] Furthermore, I require that the startup's SIC-3 code is not missing, as well as no-missing MSA information for both the startup and CVC firm in the deal.

[40] I obtain very similar results if I instead measure the direct flight during the deal year.

[41] It is in line with the fact that much of the emerging business knowledge consists of tacit and narrative knowledge, in which case visiting the startup is the only way to access it [Keil et al., 2008].

The identification assumption is that the number of non-stop flights is quasi-exogenous to any firm or industry characteristics driving the scope change decisions after controlling both the startup and CVC location by year fixed effects. This assumption is quite reasonable as the first-order effects driving the number of non-stop flights between two cities are usually the number of travelers between them and airports' hub-and-spoke connection.[42]

Figure 2.8 plots the two groups of CVC deals, separately, in Panel A and B, on the US states map. The blue point denotes the CVC firm location, while the red point represents the startup's location. Moreover, around 50 blue-red pairs appear in both Panel A and B due to either introducing a new non-stop airline or stopping an old airline route in my sample period.[43]

Table 2.11, Panel A provides the deal sample's summary statistics, breaking them down by deals with and without any direct flights.[44] As shown in the panel, startups in those CVC deals with direct flights are more likely to be located in the hot areas for VC activities – California, Massachusetts, and New York.

In Panel B and C of Table 2.11, I run OLS regressions on the CVC deals sample and study the relation between direct flight and scope changes. In Panel B, the dependent variable is a dummy equal to 1 if, within the next three years after the deal, the CVC parent establishes a new division in the same industry of the startup of the deal. As shown in the panel, the number of non-stop flights (measured in the year right after the deal year) is positively correlated with creating a new division by the CVC parent (and the division is exactly located in the startup industry). The results are robust across different controls and fixed effect specifications. Importantly, I add both the CVC firm and startup's location by year fixed effect to control local shocks in Column (4), along with the

[42]For example, see the article: https://www.afar.com/magazine/how-airlines-get-new-routes.

[43]Since this subsample is too small, I could not conduct any diff-in-diff analysis as in Bernstein et al. [2016].

[44]Those deals with the startups and the CVC firm located in the same MSA are not included in this panel.

CVC firm fixed effect in Column (5). In Panel C, I obtain very similar results on changing the primary industry: more frequent non-stop flights lead to a higher probabilities of turning its primary industry close to the startup's business.

Lastly, one might worry that the deal-selection (between deals with and without direct flight) might bias the result. If it does, I argue that theoretically it could only generate opposite results and dampens my findings. According to the deal selection view, the deals without direct flight should offers the CVC parent manager more insights about future business opportunities since the (timing) cost of monitoring and interaction is higher than those deals with direct flights. As a result, I expect that the results of Table 2.11 should be even stronger without the deal selection effect.

2.7.2 Post-CVC Value Creation

Given that CVC creates value for shareholders of its corporate parent [Dushnitsky and Lenox, 2006, Ma, 2020], my final analysis is about whether post-CVC scope changes also creates value for its parent in Table 2.12. The left-hand side variable is the difference of Tobin's Q of Firm i between Year t+h and Year t, where h usually takes a value of three or four. In Column (1) and (2), I conduct the horse-racing test between CVC related and unrelated dummy. As shown in the two columns, only the coefficients of D(CVC Unrelated) dummy are positive and significant, showing that only unrelated CVC deals create significant values.

From Column (3) to Column (8), I iteratively interact the D(CVC Unrelated) dummy with three scope changes dummies. The scope changes dummies are again forward-looking: in the next two years for division creation and removal and within three to five years for changing of primary corporate industry. As a result, in Columns (7) and (8), the Δ of Tobin's Q takes five or six years to ensure that the industry changes have already been completed before measuring the value improvement. I find that only D(CVC

Unrelated) $\times$ D(scope changes) is significant, while D(CVC Unrelated) $\times$ ($1-$D(scope changes)) is not. This shows that CVC investments' value creation mostly derives from post-CVC scope changes, as CVC without any scope changes does not bring significant value improvement.

It is a bit arbitrary regarding whether I should include the firm fixed effects in Table 2.12. On the one hand, it might be redundant since the left-hand side variable already takes the difference, eliminating any time-invariant firm characteristics. On the other hand, if I add them, the joint F test for those firm fixed effects is significant. However, my results are quite robust with and without firm fixed effects. In the online appendix, I show the alternative regressions without any firm fixed effects for Column (3) to (8) of Table 2.12.

2.8 Conclusion

This paper investigates the effects of corporate venture capital (CVC) on the scope changes and product innovation of the CVC parent corporation. To deal with the potential endogeneity, I develop two different identification strategies.

First, I introduce a new instrument for CVC investments using the fund inflow shocks of independent venture capital firms (IVC) in each CVC program's past syndicate network. The idea of the instrument is that, if an independent VC firm j (IVC j) receives a positive fund inflow shock today, and meanwhile, the CVC Firm i is in its past syndicate network, then the IVC j is very likely to launch a new sequential fund, initiate new deals, and invite CVC Firm i, its old partner, to join in its new investments. Second, I introduce the US non-stop airline routes as a quasi-natural experiment. I consistently corroborate the causal relationships.

In the various extension analysis, I document that a CVC parent is more likely to establish a new division in the industry where it has sprayed CVC deals before. Furthermore, the post-CVC value enhancement of the CVC parent derives mostly from post-CVC scope changes. Overall, the evidence is consistent with the idea that CVC helps to identify new business opportunities.

2.9 Appendix

2.9.1 Variable Definition

Variable Name	Definition and Construction of Variable	Data Source
D(CVC)	Dummy variable equal to 1 if the firm conducts at least one CVC investment in year t	VentureXpert
D(CVC Unrelated)	Dummy variable equal to 1 if the firm conducts at least one unrelated CVC deal in year t. An unrelated CVC deal is defined as a deal with the entrepreneurial company's SIC-3 code not matching with any SIC-3 codes of CVC corporate parent. A conglomerate firm has multiple SIC-3 codes, whereas a stand-alone firm has a single SIC-3 code.	VentureXpert & Compustat Historical Segment
D(CVC Related)	Dummy variable equal to 1 if the firm conducts at least one related CVC deal in year t. A related CVC deal is defined as a deal with the entrepreneurial company's SIC-3 code matching with one of SIC-3 codes of CVC corporate parent.	VentureXpert & Compustat Historical Segment
Num(CVC Deal)	Number of CVC deals conducted by Firm i in year t	VentureXpert
Num(CVC Initial Deal)	Number of CVC initial deals conducted by Firm i in year t. CVC initial deal is defined as the deal in which case the CVC firm invests in an entrepreneurial start-up for the first time, that is, not the follow-on investments.	VentureXpert

Continued on next page

Appendix A continued from previous page

Variable name	Definition and construction of variable	Data Source
Granular IV (Fund Inflow Shock)	Defined as the sum of the (positive) idiosyncratic fund inflow shocks of those IVCs in the past 5-year syndicating network. The idiosyncratic fund inflow shocks are obtained as the error term of the Gompers and Lerner (1998)'s fundraising model, as illustrated in Online Appendix, Section B.	VentureXpert
Num(IVC in the Network)	The natural logarithm of one plus the number of IVCs in the past 5-year syndication network of CVC Firm i.	VentureXpert
D(New Div.)[t+1,t+2]	A dummy equal to 1 if the firm creates at least one new division within the next two years (Year t+1 and Year t+2). Divisions are aggregated and defined in SIC-3 industries. Establishing a new division is identified if the firm reports a new division with its SIC-3 code appearing in the first time in the company history.	Compustat Historical Segment
D(Div. Rem.)[t+1,t+2]	A dummy equal to 1 if the firm removes at least one old division within the next two years. Removing an old division means that a firm stops reporting a division in the future forever.	Compustat Historical Segment
D(Chg. Ind.)[t+3,t+5]	A dummy equal to 1 if, in the next 3 to 5 years, the firm's primary industry has changed.	Compustat Historical Segment
D(Conglomerate)	Dummy equal to 1 if the firm is a conglomerate in year t. A conglomerate is defined as a firm reporting multiple segments in at least 2 different SIC-3 industries in Compustat Historical Segment database.	Compustat Historical Segment
Firm Size	Firm size, measured as natural logarithm of the firm's market capitalization (Compustat item $CSHO_t$ $\times$ item $PRCC\_F_t$)	Compustat

Continued on next page

Appendix A continued from previous page

Variable name	Definition and construction of variable	Data Source
Tobin's Q	Market-to-book ratio in assets. Market value of assets equals the book value of assets (item AT_t) + the market value of common equity at fiscal year-end (item $CSHO_t$ × item $PRCC\_F_t$) − the book value of common equity (item CEQ_t) − balance sheet deferred taxes (item $TXDB_t$)	Compustat
R&D Exp.	R&D expenditure, measured as item XRD_t scaled by lagged book assets (item AT_{t-1}). If the item XRD_t is missing, I replace it with the industry-year median XRD_t.	Compustat
ROA	Return on assets, defined as EBITDA scaled by lagged book assets	Compustat
Leverage	Book leverage, defined as debt including long-term debt (item $DLTT_t$) plus debt in current liabilities (item DLC_t) divided by the sum of debt and book value of common equity (item CEQ_t)	Compustat
Capx.	Capital expenditure, measured as item $CAPX_t$ scaled by lagged book assets	Compustat
HHI	The Hirschman-Herfindahl index of sales (item $SALE_t$) in the industry where the firm is located	Compustat
Cash	Defined as cash and cash equivalents (item CHE_t) scaled by lagged book assets	Compustat

2.9.2 The Construction of Granular IV

As discussed in the paper, I use the idiosyncratic fund inflow shock of IVC firms in the past 5-year network of each CVC firm as the instrument of the CVC investment. To obtain the idiosyncratic fund inflow shock, I follow Gabaix and Koijen [2020]'s Granular IV (GIV) approach.

To illustrate the link between my instrument and the GIV approach, I apply Gabaix and Koijen [2020]'s non-loop model (see Section 2.1.4 Model with an enriched factor structure). The model is as follows. Suppose CVC Firm i's investment decision is influenced by the raw fund inflow of IVCs in its network (this is due to the fact that IVC is usually the largest deal source of CVC firms and IVC frequently invites CVC to join in their new deals [MacMillan et al., 2008]). Then, CVC i's investment amount in year t follows,

$$Num\_CVC_{i,t} = \alpha \bar{S}_{i,t} + \beta_1 X_{i,t} + \varepsilon_{i,t} \tag{2.4}$$

where $Num\_CVC_{i,t}$ gauges the number of CVC deals initiated by CVC Firm i in Year t; while $\bar{S}_{i,t}$ is the sum of raw fund inflow of k IVC firms in the past 5-year network of the CVC firm, where k is equal to 3 in the Figure 2.7's example, the Apple Inc's example. So,

$$\bar{S}_{i,t} = \sum_{j \in Network_i} S_{j,t} \tag{2.5}$$

And $S_{j,t}$ is the raw fund inflow of IVC firm j in Year t. Next, the raw fund inflow is a function of IVC's firm characteristics $\bar{X}_{j,t}$ ($\bar{X}$ includes large sets of fixed effects) and time factors λ_t.

$$S_{j,t} = \gamma_{j,t} \lambda_t + \beta_2 \bar{X}_{j,t} + \mu_{j,t} \tag{2.6}$$

$\mu_{j,t}$ is assumed to be the idiosyncratic fund inflow shock. The crucial assumption to validate the GIV is then $E(\mu_{j,t}\varepsilon_{i,t}) = 0$ for any i and j. Then the formula of the GIV is,

$$GIV_{i,t} = \sum_{j \in Network_i} \mu_{j,t} \tag{2.7}$$

Following Gabaix and Koijen (2020)'s main setting, I consider the parametric factor exposures,

$$\gamma_{j,t} = \gamma_0 + \gamma_1 \tilde{X}_{j,t} \tag{2.8}$$

To obtain the $\mu_{j,t}$, I implement an empirical fundraising model from Gompers and Lerner (1998). Moreover, I proxy the fund inflow ($S_{j,t}$) of IVC firms with the dummy of raising a new follow-on fund. This proxy has two practical reasons: (1) new fundraising is always accompanied by the largest fund inflow; (2) when the IVC launches a new fund, it is most likely that the IVC conducts new deals and invites CVCs. The distribution of those new follow-on fundraising is plotted in Figure B.2.

In Gompers and Lerner (1998)'s two-stage Heckman selection model, the time factors λ_t include the Number of startups brought public last year by all VCs, T-bill return (10-year), Real GDP growth, and CRSP value-weighted return. $\bar{X}_{j,t}$ contains Years since raising last fund, the square of Years since raising last fund, Age of the venture organization (years), Number of startups brought public this year, Number of startups brought public last year, and finally the Number of funds launched before.

For simplicity, I assume that $\tilde{X}_{j,t} = \bar{X}_{j,t}$. In other words, the interaction terms between each IVC firm characteristics and time factors are included in equation (3). I estimate the equation (3) with OLS, adding VC industry specialization (VEIC) by year fixed effect and the location (State) by year fixed effect. Following Hochberg et al. [2007], I take a VC firm's industry specialization to be the broad Venture Economics industry group (VEIC) that accounts for most of its invested capital. The error term from the above regression is thus the $\mu_{j,t}$. Finally, I only take the positive idiosyncratic fund inflow shock (in my case, the negative shocks and positive shocks cannot cancel out since what matters finally is how many IVC receives the positive inflow shocks),

$$\widehat{GIV}_{i,t} = \sum_{j \in Network_i} \max\{\hat{\mu}_{j,t}, 0\} \tag{2.9}$$

This follows the threshold GIV as discussed in Section 2.5 of Gabaix and Koijen (2020). Table B.1 reports the OLS estimate of equation (3), where I use the error term of the Column (3) to construct my GIV.

Table B.1: Gompers and Lerner (1998)'s fundraising model

OLS	(1)	(2)	(3)
		D(Launch New Fund)	
Individual IVC characteristics			
Years since raising last fund	-0.00828***	0.00402***	0.00463***
	(-10.87)	(2.67)	(3.00)
(Years since raising last fund)^2	0.0000827**	-0.0000563*	-0.0000808**
	(2.56)	(-1.79)	(-2.46)
Age of the venture organization	0.00264***	-0.00286**	-0.00294**
	(10.47)	(-2.50)	(-2.51)
Number of startups brought public this year	0.0304***	-0.0135**	-0.0117**
	(19.88)	(-2.47)	(-2.09)
Number of startups brought public last year	0.0240***	-0.00163	-0.00164
	(15.35)	(-1.00)	(-0.98)
Number of past funds launched		0.00679**	0.00685**
		(2.09)	(2.07)
VC funding factors			
Number of startups brought public last year by all VCs	0.000279***		
	(12.06)		
T-bill return	0.0259		
	(1.40)		
Real GDP Growth	0.00480***		
	(5.05)		
CRSP value weighted return	0.112		
	(0.95)		
Interaction terms λ_2			
Years since raising last fund*		0.0000424***	0.0000445***
Number of startups brought public last year by all VCs		(4.99)	(5.13)
Years since raising last fund*		0.0710***	0.0664***
T-bill return		(8.90)	(8.08)
Years since raising last fund*		0.00181***	0.00175***
Real GDP Growth		(5.03)	(4.75)
Years since raising last fund*		0.200***	0.190***
CRSP value weighted return		(4.38)	(4.06)
Age of the venture organization*		-0.0000605***	-0.0000617***
Number of startups brought public last year by all VCs		(-8.17)	(-8.19)
Age of the venture organization*		-0.0685***	-0.0643***
T-bill return		(-9.30)	(-8.51)
Age of the venture organization*		-0.00203***	-0.00196***
Real GDP Growth		(-6.37)	(-6.02)
Age of the venture organization*		-0.214***	-0.209***

CRSP value weighted return		(-5.18)	(-4.92)
Number of startups brought public this year* Number of startups brought public last year by all VCs		-0.0000253 (-1.12)	-0.0000285 (-1.22)
Number of startups brought public this year* T-bill return		-0.0290* (-1.73)	-0.0345** (-2.00)
Number of startups brought public this year* Real GDP Growth		0.00789*** (6.33)	0.00776*** (6.03)
Number of startups brought public this year* CRSP value weighted return		0.235 (1.55)	0.204 (1.30)
Number of past funds launched* Number of startups brought public last year by all VCs		0.000355*** (16.69)	0.000358*** (16.55)
Number of past funds launched* T-bill return		0.228*** (11.57)	0.215*** (10.65)
Number of past funds launched* Real GDP Growth		0.00642*** (7.29)	0.00620*** (6.91)
Number of past funds launched* CRSP value weighted return		0.934*** (8.25)	0.918*** (7.93)
VEIC $\times$ Year F.E.		Yes	Yes
Location $\times$ Year F.E.			Yes
Num. Obs.	33,163	33,163	33,163
Adj. R^2	0.076	0.205	0.203

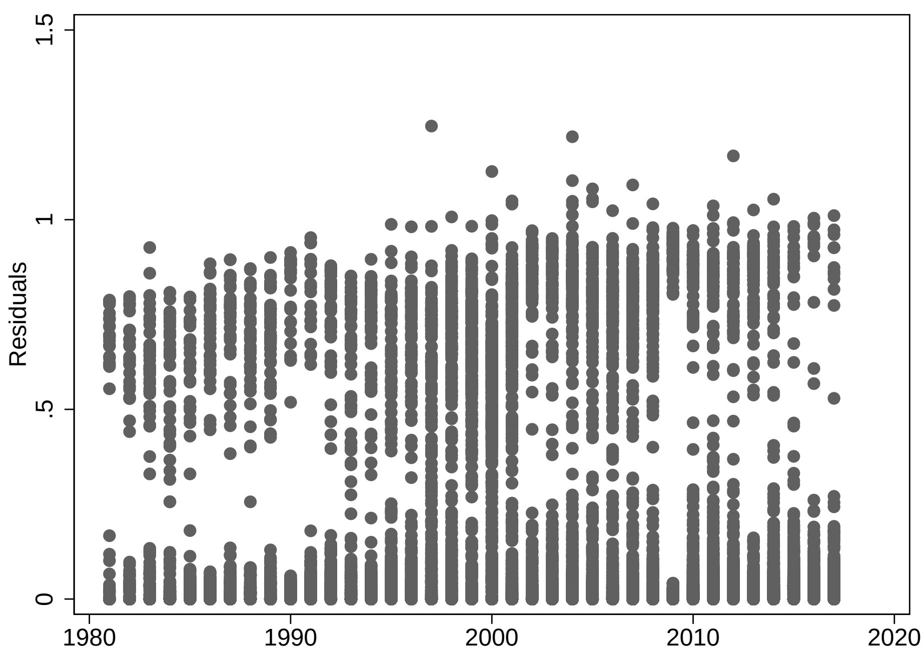
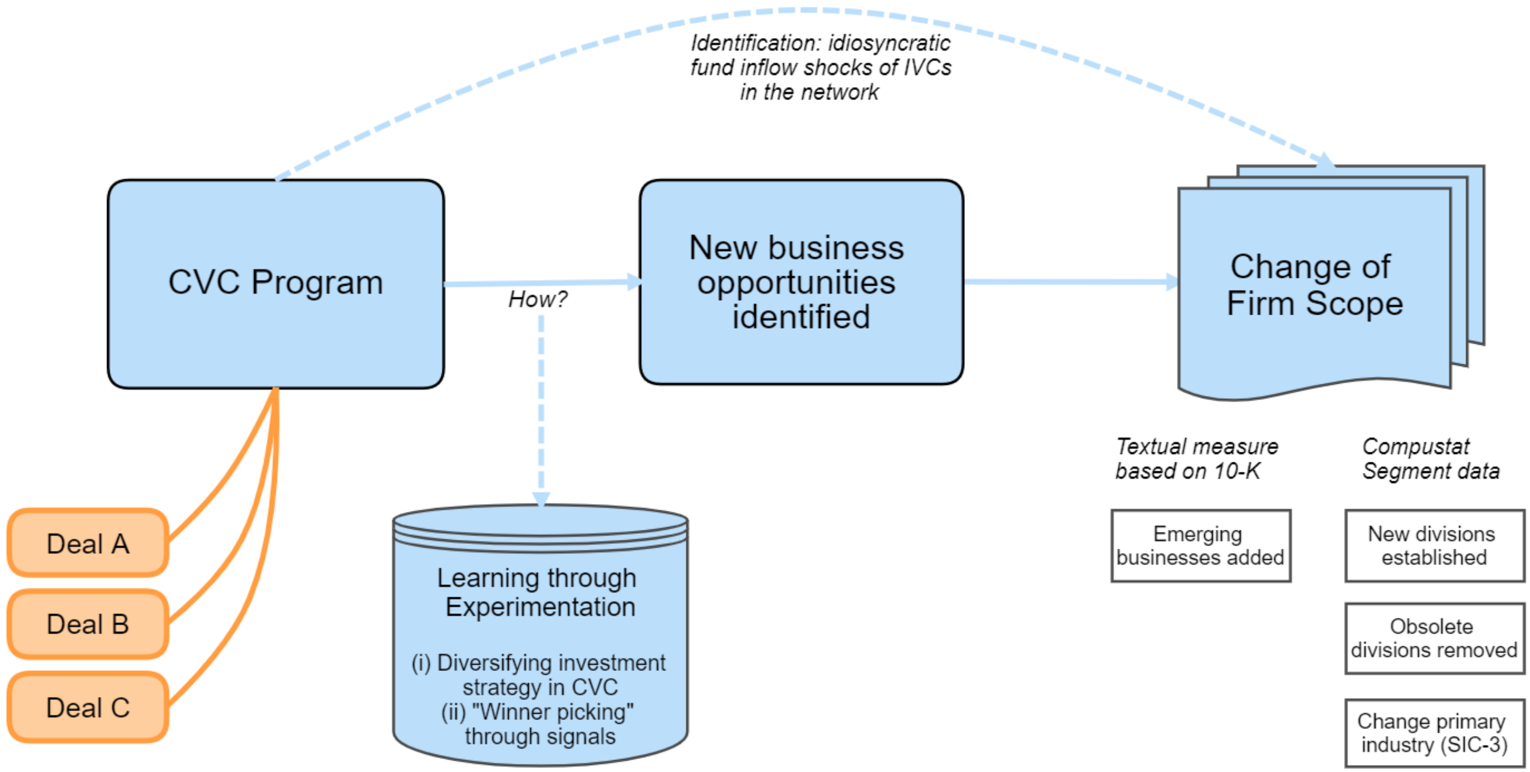


Figure B.1: The distribution of the residues by years

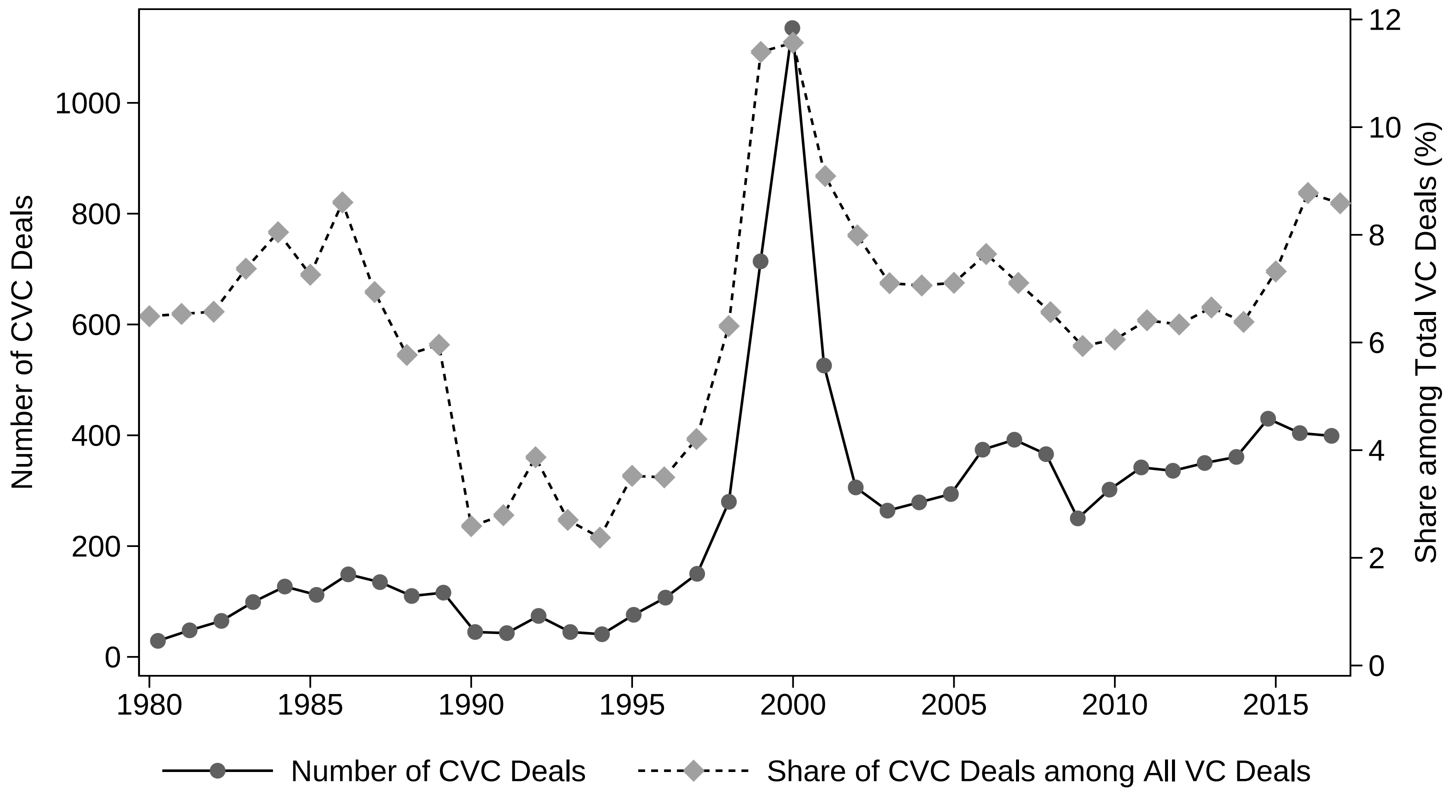
2.10 Figures and Tables

Figure 2.1: The Overview of the Main Idea and Findings



The figure provides an overview of the main idea and findings in the paper. A CVC program could help identify new business opportunities for its parent firm and further spur the firm to change the firm scope. Two types of measures are used in capturing the firm scope changes: a textual measure based on the annual 10-K filings and Compustat Segment measures. Furthermore, I document that the mechanism through which CVC could identify new business opportunities is the story of experimentation.

Figure 2.2: Corporate Venture Capital Deals by Calendar Year



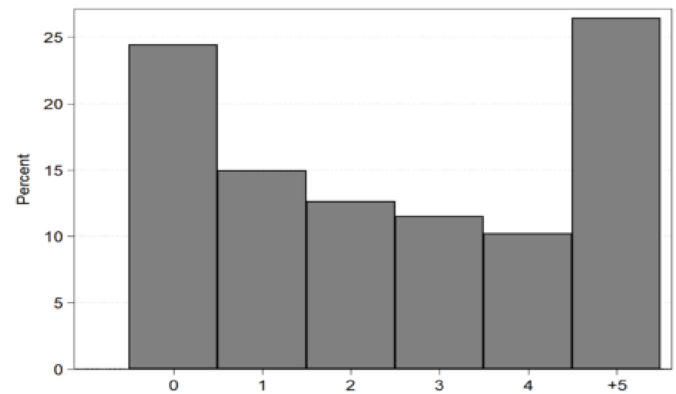
The figure plots annual CVC investments initiated by US public (non-financial) corporations in Compustat database. The left axis is the number of CVC deals in each year, and the right axis is the share (in percentage) of the CVC deals among all VC deals. The data are mainly obtained from SDC VentureXpert. The data range is from 1980 to 2017.

Figure 2.3: "Emerging Phrases" and Emerging Business Integration

(a) CVC-backed startups' "emerging phrases" in 2000

(b) CVC-backed startups' "emerging phrases" in 2017

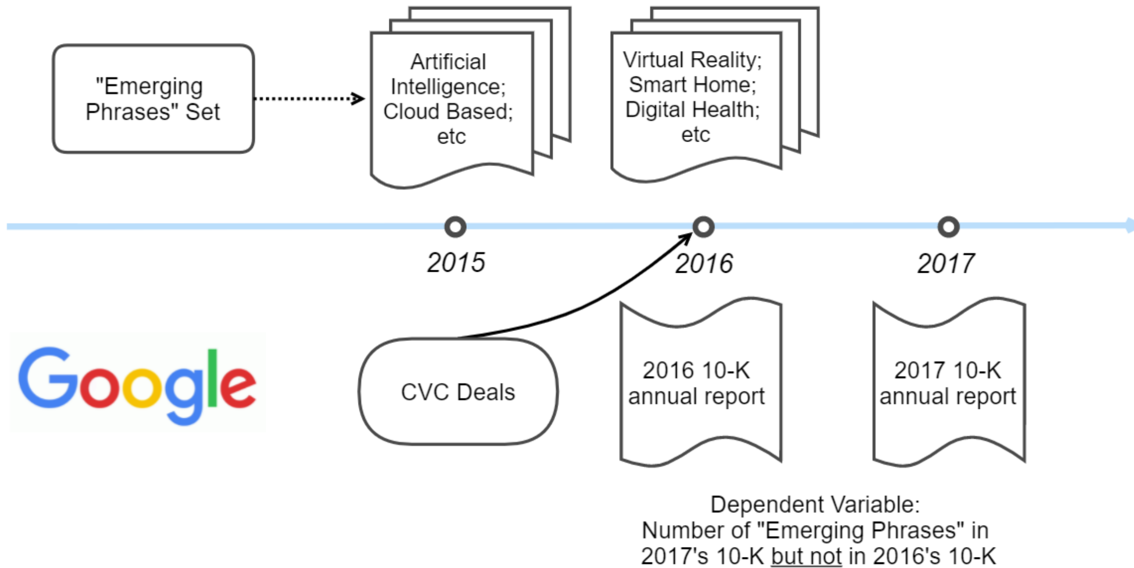
(c) Top 50 "emerging phrases" mostly integrated by CVC parents



(d) Years of surviving of "emerging phrases" in the subsequent 10-Ks

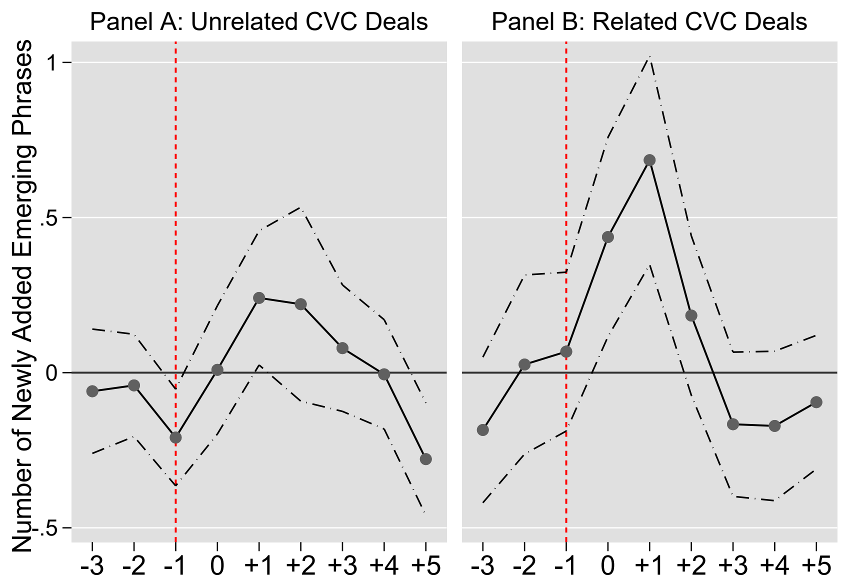
Panel A and B present the words clouds of "emerging phrases" in 2000 and 2017. Emerging phrases are the top 5% most popular short phrases (excluding stopwords and common words) in the detailed business descriptions of all VC-backed startups receiving VC fundings in a given year. Panel C plots the top 50 most frequent emerging phrases newly added by CVC parents into 10-K Item 1 (business description) within two years after CVC deals. Panel D plots the distribution of years of surviving of all 2,081 emerging phrases added by CVC parents after investments.

Figure 2.4: Regression Design in Table 2.2: An Example



The figure explains the regression design in Table 2.2. Take Google as an example. Suppose the Google CVC program (Google Venture) invests in startups in 2016, and during that year, the set of emerging phrases includes virtual reality, digital health, and smart home. Then I search in Google's 2017 10-K (Item 1 Business Description) these three emerging phrases. The dependent variable thus counts the number of 2016 emerging phrases newly added in 2017's 10-K business description. The intuition is that when Google invests in CVC in 2016, it helps Google identify new business opportunities such as digital health, and one year after investment (2017), Google should be more likely to integrate it into its own business.

Figure 2.5: Firm Scope Change around the CVC Deals (Measured by "Emerging Phrases")

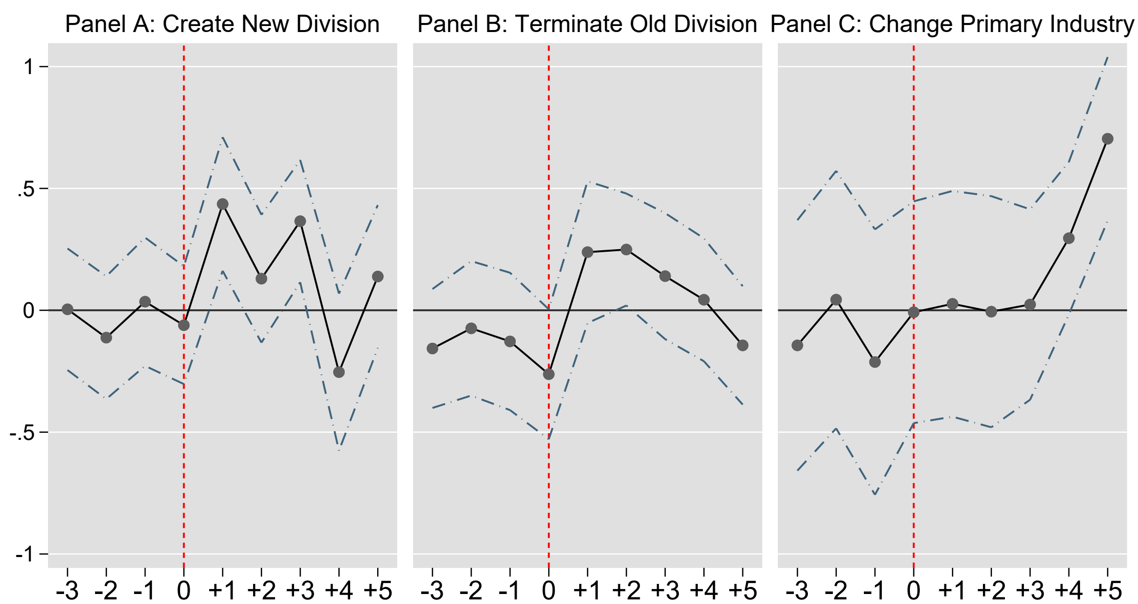


The figure examines the emerging phrases usage in the years around the CVC deals. The estimates (OLS) and confidence intervals are taken from the following regression specification,

$$EmergingPhrases_{i,t} = \sum_{k=-3}^{+5} \gamma_k D(CVC\ Unr;k)_{i,t} + \sum_{k=-3}^{+5} \alpha_k D(CVC\ Rel;k)_{i,t} + \beta\mathbf{X} + \tau_i + \upsilon_t + \varepsilon_{i,t}$$

where the left-hand side variable counts the number of new emerging phrases newly added into the firm's 10-K Item 1, i.e. appears in Year *t* but not in Year *t*-1. Emerging phrases (plotted in the online appendix) are the top 5% most popular short phrases (excluding common words and stopwords) taken from all VC-backed startup's business in a given year. $\{D(CVC\ Unr;k)\}_{k=-3}^{+5}$ is a bunch of dummies equal to 1 if the year is *k* years before or after each CVC unrelated deal. A similar setup applies to $\{D(CVC\ Rel;k)\}_{k=-3}^{+5}$ for CVC related deals. The firm and year fixed effects are included in all regressions. Standard errors are clustered at the firm level. The confidence intervals are calculated at the 90% confidence level. Coefficients of $\{D(CVC\ Unr;k)\}_{k=-3}^{+5}$ and of $\{D(CVC\ Rel;k)\}_{k=-3}^{+5}$ are plotted in Panel A and B separately. **X** includes Firm Size, Tobin's Q, ROA, R&D, Leverage, Capx., Cash, Sales Growth, HHI, Firm Age, and D(Conglomerate)(lagged).

Figure 2.6: Firm Scope Change around the CVC Deals (Measured by Segment Variables)

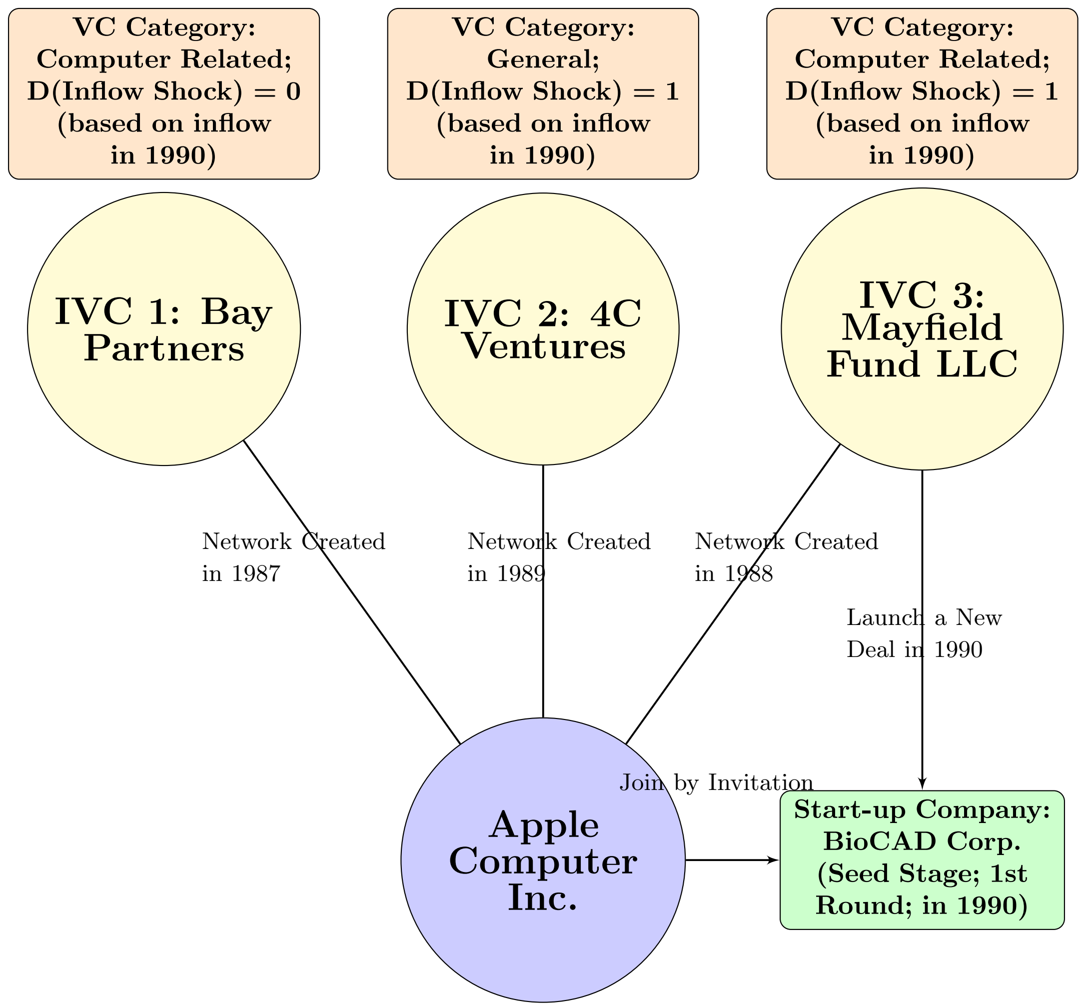


The figure examines the firm scope change in the years around the CVC deals. The estimates (from logit) and confidence intervals are taken from the following regression specification,

$$D[Scope\ Change]_{i,t} = \sum_{k=-3}^{+5} \gamma_k D(CVC\ Unr;k)_{i,t} + \sum_{k=-3}^{+5} \alpha_k D(CVC\ Rel;k)_{i,t} + \beta \mathbf{X} + \tau_i + \upsilon_t + \varepsilon_{i,t}$$

where $D[Scope\ Change]$ denotes three firm scope change dummies regarding creating a new division, removing an old division, and changing the corporate primary industry, respectively, measured in Year t. $\{D(CVC\ Unr;k)\}_{k=-3}^{+5}$ is a bunch of dummies equal to 1 if the year is k years before or after each CVC unrelated deal. A similar setup applies to $\{D(CVC\ Rel;k)\}_{k=-3}^{+5}$ for CVC related deals. Firm and year fixed effects are included in all regressions. Standard errors are clustered at the firm level. The confidence intervals are calculated at 90% confidence level. For simplicity, only coefficients of $\{D(CVC\ Unr;k)\}_{k=-3}^{+5}$ are plotted in the figure. $\mathbf{X}$ includes Firm Size, Tobin's Q, ROA, R&D, Leverage, Capx., Cash, Sales Growth, HHI, Firm Age, and D(Conglomerate)(lagged).

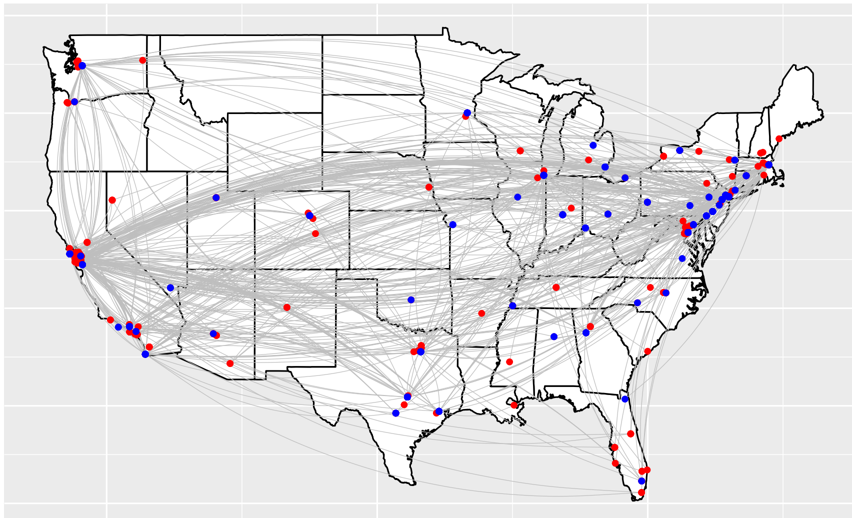
Figure 2.7: An Example of Instrument Variable of CVC Investments



The figure shows a simple example of the instrument variable of CVC investments. The idea of the instrument is that, if an independent VC firm j (IVC j) receives a positive fund inflow shock today, and meanwhile, the CVC Firm i is in its past syndicate network, then, the IVC j is very likely to initiate new deals and invite CVC Firm i, its old partner, to join in its new investments. Alternatively, IVCs can recommend new deals to CVCs when IVCs start new funds and seek deals. Consider the case illustrated in the above figure. This figure illustrates how its IVC partners drive the CVC investment decision of Apple Computer Inc. In the past five years of 1990, Apple Inc has built three connections with three distinct IVCs through syndicate investments. Among the three IVCs, two received positive inflow shocks in 1990. One of these two IVCs, Mayfield Fund LLC, then spent its new money on investing a seed-stage startup called BioCAD Corp in 1990, followed by the joining of Apple Inc due to the invitation of Mayfield Fund. The idiosyncratic fund inflow shock is constructed following the granular IV approach [Gabaix and Koijen, 2020]. The construction of the past 5-year syndication network is illustrated in the text.

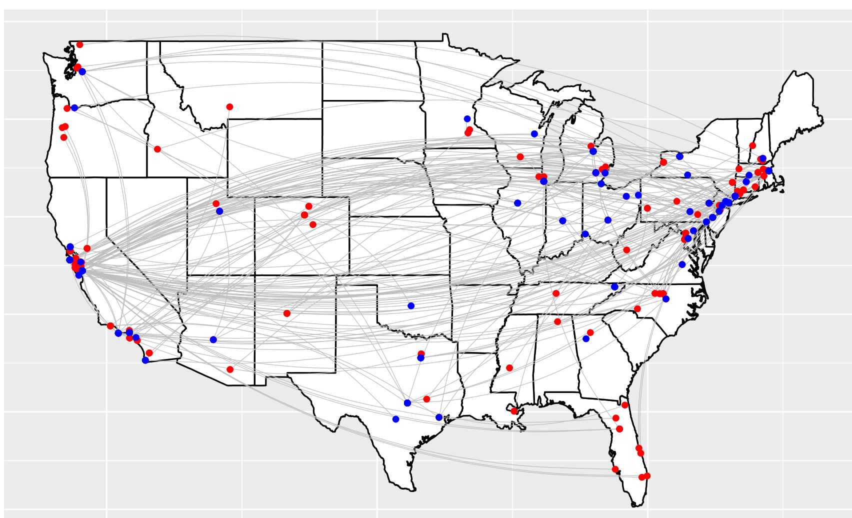
Figure 2.8: CVC Deals with and without Direct Flights

Panel A: CVC Deals with Direct Flights



Blue Dot: CVC Firm's location; Red Dot: startup's location

Panel B: CVC Deals without Direct Flights



Blue Dot: CVC Firm's location; Red Dot: startup's location

This figure presents the geographic location of CVC deals, where the blue point denotes the CVC firm's location, and the red point denotes the location of the startup. Panel A includes those deals with direct flights, and Panel B draws those deals without direct flights. A CVC deal with direct flight is defined as a deal with a non-stop airline route between the MSA of CVC firm's headquarter and MSA of the startup during the deal year.

Table 2.1: Summary Statistics

This table presents the summary statistics of the firm-year sample used in most regressions. The firm-year sample consists of all observations recorded in both the Compustat and Compustat Historical Segment database from 1980 to 2017. I exclude foreign firms (firms incorporated outside of the US) and firms in financial industries (SIC industry codes starting with 6) from the sample. Furthermore, industries (3-digit SIC) with no CVC activity during the whole sample period are excluded entirely. All variables (except dummies) are winsorized at 1% and 99% levels. In Panel A, the sample is split by D(CVC), the dummy of CVC deals. D(CVC) is equal to 1 if the Firm i conducts at least one CVC deal in Year t. D(New Div.) is a dummy equal to 1 if the firm creates at least one new division in a new industry within the next two years of Year t. D(Div. Rem.) is a dummy equal to 1 if the firm removes at least one old division within the next two years of Year t. D(Chg. Ind.) (3-5) is a dummy equal to 1 if the firm changes the primary corporate industry in the next three to five years. D(Chg. Ind.) (4-6) is defined similarly but based on the next four to six years. Definitions of all other variables are in Appendix 2.9.1.

Panel A: Firm-Year sample

Variables	D(CVC) = 1			D(CVC) = 0			Test of Mean
	Mean	S.D.	N.	Mean	S.D.	N.	p value
D(New Div.)	0.139	0.346	2,129	0.086	0.280	152,169	0.000
D(Div. Rem.)	0.171	0.377	2,129	0.099	0.298	152,169	0.000
D(Chg. Ind.) (3-5)	0.088	0.336	2,129	0.053	0.280	152,169	0.000
D(Chg. Ind.) (4-6)	0.083	0.339	2,129	0.045	0.276	152,169	0.000
Firm Size	7.979	1.790	2,096	4.430	2.313	129,622	0.000
Tobin's Q	2.592	3.314	1,894	3.472	17.622	124,357	0.030
R&D Exp.	0.088	0.233	2,125	0.203	1.598	150,678	0.000
ROA	0.130	0.286	2,080	-0.082	1.154	136,788	0.000
Book Leverage	0.322	0.297	2,106	0.338	0.549	148,477	0.169
Capx.	0.071	0.089	2,082	0.081	0.116	136,812	0.000
HHI	0.083	0.077	2,129	0.084	0.083	152,169	0.669
Cash	0.195	0.191	2,127	0.189	0.227	151,034	0.236
D(Conglomerate)	0.469	0.499	2,129	0.232	0.422	152,169	0.000

Panel B: CVC related and unrelated deals

CVC Deal Type	Number	Percentage
Related Deals	4,159	38.17%
Unrelated Deals	5,744	52.72%
The startup's SIC-3 code is missing	992	9.11%

Panel C: Change of the firm scope after CVC

	Num. Events
Within the next 2 years following CVC unrelated deals:	
Establish new divisions in new industries	243
Remove obsolete divisions	255
Within the next 3-5 years following CVC unrelated deals:	
Change the corporate primary industry	104
The new division becomes the business of the primary industry	43

Table 2.2: CVC Investments and Firm Scope Change: Emerging Phrases

This table presents the regressions about CVC and the firm scope change measured by emerging phrases. The regression sample consists of all Compustat firms incorporated in the US, with 10-K fillings of Year *t* and *t*-1 searchable in SEC, and are not in financial industries. Industries (defined as 3-digit SIC) with no CVC activity during the whole sample period are excluded entirely. The dependent variable is defined as the number of "Emerging Phrases" newly added in the next year (or in the second year)'s 10-K business description. The Emerging Phrases are those top 5% most frequently-used word pairs (excluding stopwords and common words) in the detailed business descriptions of all VC-backed startups receiving VC funding in a given year. Column (1) - (3) count those "Emerging Phrases" appearing in Year *t*+1's 10-K Item 1 but not in the Year *t*. T-statistics are shown in parentheses, and standard errors are clustered by firm. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
OLS	Num. of "Emerging Phrases" Newly Added in 10-K Item 1					
	in Year *t*+1's business *but not* in Year *t*'s business			in Year *t*+2's business *but not* in Year *t*+1's business		
D(CVC)	0.776***			0.676***		
	(7.36)			(6.53)		
D(CVC Unrelated)		0.442***	0.300***		0.562***	0.331**
		(3.82)	(2.82)		(4.41)	(2.40)
D(CVC Related)		0.782***	0.873***		0.433***	0.301**
		(5.23)	(5.45)		(3.48)	(2.31)
Num. New Word Pairs	0.863***	0.862***	0.937***	0.865***	0.865***	0.935***
Added in t+*i* (÷1000)	(33.49)	(33.58)	(30.50)	(32.56)	(32.58)	(31.14)
Firm-level Controls	Firm Size, Tobin's Q, ROA, R&D, Leverage, Cash, Sales Growth, Capx., HHI, D(Conglomerate), Firm Age, 10-K (Item 1) Text Length					
Year × Industry F.E.	✓	✓		✓	✓	
Year F.E.			✓			✓
Firm F.E.			✓			✓
Num. Obs.	50,931	50,931	49,916	46,749	46,749	45,856
Adj. R^2	0.394	0.394	0.425	0.379	0.379	0.419

Table 2.3: Industry-specific CVC Investments and Industry-specific Emerging Phrases

This table presents the regressions about industry-specific CVC investments and industry-specific emerging phrases newly used by US public firms. The left-hand side variable is the number of industry-specific emerging phrases that are newly added into the firm's annual 10-K Item 1. The dependent variable takes the natural logarithm transformation (ln(1+variable)). Each emerging phrase is sorted into eight VEIC industries. The control variables are dummies of industry-specific CVC investments in the past three years. CVC investments are again sorted into eight VEIC industries. T-statistics are shown in parentheses, and standard errors are clustered by firm and year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Newly Added Emerging Phrases (VEIC Industry Specific)							
VEIC Industry	Biotech- -nology	Communic- -ation	Computer Hardware	Computer Software	Internet Specific	Medical Health	Non-High- -Tech	Others
D(CVC in Biotechnology)	0.057**	0.006	0.021	-0.000	-0.027	0.014	0.012	0.009
	(2.315)	(0.388)	(1.517)	(-0.013)	(-0.803)	(0.762)	(0.818)	(0.442)
D(CVC in Communication)	-0.005	0.075**	-0.002	0.019	0.052	-0.005	-0.001	0.026*
	(-1.592)	(2.761)	(-0.176)	(0.512)	(1.283)	(-1.007)	(-0.088)	(1.864)
D(CVC in Computer Hardware)	0.010	0.004	-0.002	-0.045	0.028	-0.004	-0.004	-0.021
	(1.233)	(0.118)	(-0.154)	(-1.294)	(0.825)	(-0.349)	(-0.258)	(-1.260)
D(CVC in Computer Software)	-0.006	0.016	0.003	0.070**	0.022	0.008	0.002	-0.000
	(-1.069)	(0.936)	(0.385)	(2.573)	(0.753)	(1.333)	(0.160)	(-0.005)
D(CVC in Internet Specific)	0.008	0.008	0.009	0.084***	0.113**	-0.002	0.004	0.012
	(1.546)	(0.471)	(1.224)	(3.137)	(2.380)	(-0.606)	(0.436)	(1.243)
D(CVC in Medical Health)	0.016	-0.023	-0.019	0.049	0.055*	0.010	0.008	0.011
	(0.931)	(-1.016)	(-1.577)	(1.541)	(1.801)	(0.592)	(0.508)	(0.835)
D(CVC in Non-high-tech)	-0.009	-0.005	0.007	0.034	0.007	-0.012	-0.024**	0.005
	(-1.487)	(-0.260)	(0.628)	(1.179)	(0.270)	(-1.540)	(-2.196)	(0.447)
D(CVC in Others)	-0.011**	0.002	0.006	0.001	-0.010	-0.001	0.005	0.047***
	(-2.519)	(0.071)	(0.471)	(0.026)	(-0.323)	(-0.114)	(0.659)	(3.913)
Firm F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Industry × Year F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Num. Obs.	50,931	50,931	50,931	50,931	50,931	50,931	50,931	50,931
Adj. R^2	0.156	0.227	0.049	0.281	0.320	0.055	0.055	0.087

Table 2.4: CVC Investments and Firm Scope Change: Segment Measures

This table provides the estimate of logistic regressions about CVC investments and the subsequent firm scope change by CVC corporate parents. The regression sample consists of all Compustat firms which are incorporated in the US and are not in financial industries. Industries (defined as 3-digit SIC) with no CVC activity during the whole sample period are excluded entirely. Panel A (Columns 1 – 3) investigates the scenario of creating new divisions. The dependent variable is a dummy equal to 1 if the firm creates at least one new division within the next two years (Year t+1 and Year t+2). Establishing a new division is identified if the firm reports a new division with its SIC-3 code appearing in the first time in the company history. Panel A (Columns 4 – 6) studies the situation of removing old divisions. The dependent variable is a dummy equal to 1 if the firm removes at least one old division within the next two years. Panel B investigates the change of the primary corporate business. The dependent variable is a dummy equal to 1 if the firm's primary industry has changed in the next 3 to 5 years. About control variables, D(CVC) is a dummy equal to 1 if the firm invests in CVC deals in Year t. The D(CVC) variable is further divided into two variables in Columns (2) and (3) of each panel. D(CVC Related) is a dummy equal to 1 if the firm conducts at least one related CVC deal in Year t. The related CVC deal is the CVC deal related to the existing business of the corporate parent. D(CVC Unrelated) is a dummy equal to 1 if the firm conducts at least one unrelated CVC deal in Year t. The regression sample is further adjusted to alleviate the survivorship bias within the next two years for Panel A and B and within the next 3–5 years for Panel C. Industry fixed effects are defined in SIC-2 Industries. T-statistics are shown in parentheses, and standard errors are clustered by firm. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Creating new divisions and removing old divisions						
Conditional Logit	(1)	(2)	(3)	(4)	(5)	(6)
Period	D(Create New Division) [t+1, t+2]			D(Remove Old Division) [t+1, t+2]		
D(CVC)	0.350***			0.323**		
	(2.68)			(2.57)		
D(CVC Unrelated)		0.531***	0.434***		0.464***	0.195
		(3.74)	(2.91)		(3.10)	(1.21)
D(CVC Related)		-0.294	-0.00921		-0.195	-0.231
		(-1.42)	(-0.04)		(-1.00)	(-1.06)
Division Creation/Removal in the Past 2 Years	0.189***	0.189***		0.283***	0.284***	
	(3.82)	(3.81)		(6.57)	(6.59)	
Firm Controls:	Firm Size, Tobin's Q, ROA, R&D, Leverage, Capx., Cash, HHI, D(Conglomerate), Firm Age					
Year × Industry F.E.	✓	✓		✓	✓	
Year F.E.			✓			✓
Firm F.E.			✓			✓
Num. Obs.	86,030	86,030	42,584	87,066	87,066	39,191
Pseudo R^2	0.026	0.027	0.069	0.166	0.166	0.099
Prob. Increased						
by D(CVC) = 1	+3.45%	–	–	+3.23%	–	–
by D(CVC Unrelated) = 1	–	+5.86%	4.91%	–	5.02%	2.73%

Panel B: Change corporate primary industry						
Conditional Logit	(1)	(2)	(3)	(4)	(5)	(6)
Period	D(Change Industry) [t+3, t+5]			D(Change Industry) [t+4, t+6]		
D(CVC)	0.479**			0.501**		
	(2.45)			(2.40)		
D(CVC Unrelated)		0.524***	0.446**		0.608***	0.535**
		(2.60)	(2.10)		(2.85)	(2.57)
D(CVC Related)		-0.0128	-0.0161		-0.226	-0.329
		(-0.04)	(-0.04)		(-0.72)	(-0.89)
Change Primary Industry in the Past 2 Years	0.762***	0.762***		0.740***	0.740***	
	(12.43)	(12.42)		(11.20)	(11.19)	
Firm Controls:	Firm Size, Tobin's Q, ROA, R&D, Leverage, Capx., Cash, HHI, D(Conglomerate), Firm Age					
Year $\times$ Industry F.E.	✓	✓		✓	✓	
Year F.E.			✓			✓
Firm F.E.			✓			✓
Num. Obs.	82,339	82,339	22,751	80,056	80,056	21,202
Pseudo R^2	0.071	0.070	0.062	0.062	0.062	0.076
Prob. Increased						
by D(CVC) = 1	+3.14%	–	–	+3.12%	–	–
by D(CVC Unrelated) = 1	–	+3.56%	3.08%	–	4.04%	3.58%

Table 2.5: CVC Investments and Firm Scope Change: CVC Diversification Strategies

This table presents the diversification strategy in CVC investments and the firm scope change. The regression sample and definitions of dependent variables follow Table 2.4. D[New Div.] is a dummy equal to 1 if the firm establishes a new division within the next two years; D[Chg.Ind.] is equal to 1 if the firm changes the corporate primary business (industry) in the next 3-5 years. Num. Emerging Phrases is the number of "Emerging Phrases" newly added by the firm into its annual 10-K business description in Year t+1. D[CVC Past 3yr] is a dummy equal to 1 if the firm conducts at least one CVC deal in the past three years. Inverse HHI(VEIC) is the inverse of the HHI measure regarding the past three-year CVC deals across 10 VEIC industries. Num(VEIC) is the number of VEIC industries in which the firm has CVC investments during the past three years. Industry×Year fixed effects are defined in SIC-2 industries. T-statistics are shown in parentheses, and standard errors are clustered by firm. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Period	D[New Division] [t+1, t+2]		D[Change Industry] [t+3, t+5]		Num. Emerging Phrases in t+1 But not in t	
D[CVC Past 3yr]	0.158	-0.0101	0.333***	0.210*	0.394***	0.269**
	(1.63)	(-0.09)	(3.65)	(1.84)	(4.20)	(2.04)
D[CVC Past 3yr] × Inverse HHI(VEIC) (Past 3yr)	0.507**		0.725***		0.518*	
	(2.35)		(3.54)		(1.88)	
D[CVC Past 3yr] × Num(VEIC) (Past 3yr)		0.157***		0.168***		0.139**
		(3.62)		(3.68)		(2.09)
D[CVC Past 3yr] × Num Deals (Past 3yr)	0.000414	-0.00485	-0.00517***	-0.0113***	0.0102***	0.00656***
	(0.20)	(-1.50)	(-2.63)	(-2.98)	(4.13)	(2.66)
Firm Controls:	Firm Size, Tobin's Q, ROA, R&D, Leverage, Capx., CASH, sale_grt, HHI, D(Conglomerate), Age					
Industry*Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Num. Obs.	86,310	86,310	84,460	84,460	45,437	45,437
Pseudo R^2 / Adj. R^2	0.028	0.028	0.076	0.076	0.386	0.386

Table 2.6: Discrete Choice Model of Division Creation

This table presents the estimate of a discrete choice model (McFadden [1973]). The observations are at the firm-year-industry level. Each observation represents the alternative (Industry *j*) where Firm *i* in Year *t* could choose to create a new division. The set of alternatives (choice set) consists of 404 non-financial SIC-3 industries that haven been documented at least once in the Compustat Historical Segment database from 1980 to 2017. The set of alternatives varies across each firm-year pair (case). In the choice model, I only include firms investing at least one CVC deal from 1980 to 2017. For each firm-year pair (case), I drop those industries that already exist as divisions of the firm in Year *t*-1 and those that have already been created before Year *t*. The dependent variable is a dummy equal to 1 if the Firm *i* in Year *t* creates a new division in Industry *j*. D(CVC 3yr) is a dummy equal to 1 if, within the last three years, the Firm *i* has invested in CVC deals in Industry *j*. D(Ind. Proxy SIC1) and D(Ind. Proxy SIC2) capture industry proximity between the alternative and the industries of existing divisions of Firm *i* in Year *t*-1. D(Ind. Proxy SIC2) is a dummy equal to 1 if the alternative has the same 2-digit SIC with one of the existing divisions of Firm *i*. D(Ind. Proxy SIC1) is a dummy equal to 1 if the alternative has the same 1-digit SIC with one of the existing divisions of Firm *i*, but does not have the same 2-SIC with them. D(Ind. Services) are those industries starting with 7 in SIC-3. The conditional logit regression is grouped in the firm-year level. Standard errors are clustered by firm-year. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Conditional Logit	(1) D(Create New Division)	(2)	(3)
D(CVC 3yr)	3.249***	4.378***	4.004***
	(13.31)	(20.33)	(14.70)
D(Ind. Proxy SIC2)	2.830***	2.997***	2.988***
	(25.33)	(27.30)	(27.28)
D(Ind. Proxy SIC1)	0.751***	0.834***	0.823***
	(6.83)	(7.37)	(7.29)
D(CVC 3yr) $\times$ D(Ind. Proxy SIC2)		-2.854***	-2.724***
		(-6.46)	(-5.96)
D(CVC 3yr) $\times$ D(Ind. Proxy SIC1)		-1.594***	-1.431***
		(-3.12)	(-2.65)
D(CVC 3yr) $\times$ D(Ind. Business Services)			0.662**
			(1.99)
CLOGIT Grouped by Firm-Year	✓	✓	✓
Industry F.E.	✓	✓	✓
Num. Obs.	234,539	234,539	234,539
Pseudo R^2	0.131	0.138	0.138

Table 2.7: CVC Signal Response and Division Creation

This table studies the CVC signal and division creation following the signal. The setup and estimate of the discrete choice model follow Table 2.6. The dependent variable is a dummy equal to 1 if the Industry j is chosen by the Firm i in Year t to establish a new division. Each signal variable is constructed and based on the past three-year CVC investments in the given industry and is interacted with the D(CVC 3yr) dummy, which is equal to 1 if the firm has made CVC investments in that industry within the past three years. Panel A uses the main setup to construct signal variables, and Panel B uses the fraction as the definition. Regarding CVC signal variables, Num(Startups IPO) is the number of Industry-j startups (invested within three years before) that finally exit through IPO (IPO date after Year t is allowed). Num(Startups Acquired) is the number of Industry-j startups acquired by the third-party (not acquired by the CVC parent firm itself). The conditional logit regression is grouped at the firm-year level. Standard errors are clustered by firm and year. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Main Setup

Conditional Logit	(1)	(2)	(3)	(4) D(Create New Division)	(5)	(6)	(7)
D(CVC 3yr)	3.486***	3.729***	3.682***	3.665***	3.606***	3.660***	3.492***
	(11.55)	(13.34)	(12.65)	(12.79)	(11.61)	(12.82)	(11.04)
D(CVC 3yr) × D(Ind. Proxy SIC2)	-2.363***	-2.437***	-2.454***	-2.441***	-2.480***	-2.448***	-2.520***
	(-5.06)	(-5.12)	(-4.96)	(-5.13)	(-5.21)	(-5.11)	(-5.41)
D(CVC 3yr) × D(Ind. Proxy SIC1)	-1.561**	-1.665**	-1.459**	-1.556**	-1.547**	-1.809***	-1.836***
	(-2.48)	(-2.42)	(-2.24)	(-2.40)	(-2.27)	(-2.68)	(-2.68)
D(CVC 3yr) × Num(Startups IPO)	0.774***						
	(2.90)						
D(CVC 3yr) × Num(Startups Acquired by Third Party)		0.296					
		(0.59)					
D(CVC 3yr) × Num(Startups Acquired with Above-median IRR)			0.421**				
			(2.17)				
D(CVC 3yr) × Num(Startups Acquired by CVC Parent Itself)				0.322***			
				(3.30)			
D(CVC 3yr) × Num(Startups Bank- -ruptcy)					-0.567*		
					(-1.77)		
D(CVC 3yr) × Patents Growth Signal						1.585**	
						(2.52)	
D(CVC 3yr) × Patents Positive Signal							0.796**
							(2.11)
D(CVC 3yr) × Patents Negative Signal							-0.253
							(-0.46)
D(CVC 3yr) × Num(Startups Invested)	-0.0292	0.0120	0.00222	0.0353	0.144**	0.0525	0.0531
	(-0.56)	(0.14)	(0.04)	(0.90)	(2.52)	(1.27)	(1.28)
Industry Cluster Controls							
IPO Cluster	0.0226***	0.0226***	0.0226***	0.0227***	0.0226***	0.0226***	0.0226***
	(9.56)	(9.55)	(9.55)	(9.56)	(9.55)	(9.56)	(9.55)
Acquisition Cluster	0.0123***	0.0123***	0.0123***	0.0123***	0.0123***	0.0123***	0.0122***
	(5.79)	(5.78)	(5.78)	(5.80)	(5.79)	(5.79)	(5.77)
Patent Cluster	0.000405*	0.000407**	0.000408**	0.000407**	0.000406**	0.000408**	0.000410**
	(1.96)	(1.97)	(1.97)	(1.97)	(1.96)	(1.97)	(1.98)
D(CVC 3yr) × IPO Cluster	-0.0173***	-0.0128**	-0.0132**	-0.0126**	-0.0125**	-0.0141***	-0.0128**
	(-3.26)	(-2.47)	(-2.45)	(-2.43)	(-2.44)	(-2.60)	(-2.40)
D(CVC 3yr) × Acquisition Cluster	-0.00493	-0.00747**	-0.00651*	-0.00684**	-0.00790**	-0.00621*	-0.00759**
	(-1.37)	(-2.27)	(-1.95)	(-2.06)	(-2.39)	(-1.86)	(-2.29)
D(CVC 3yr) × Patent Cluster	-0.0000594	-0.0000714	-0.0000886	-0.000113	-0.0000563	-0.000180	-0.000167
	(-0.20)	(-0.24)	(-0.30)	(-0.37)	(-0.19)	(-0.57)	(-0.54)
CLOGIT grouped by Firm-Year	✓	✓	✓	✓	✓	✓	✓
Industry F.E.	✓	✓	✓	✓	✓	✓	✓
Num. Obs.	234,539	234,539	234,539	234,539	234,539	234,539	234,539
Pseudo R^2	0.181	0.180	0.181	0.181	0.181	0.181	0.181

Panel B: Alternative Construction of Signals	(1)	(2)	(3)	(4)	(5)
Conditional Logit			D(Create New Division)		
D(CVC 3yr)	3.416***	3.759***	3.695***	3.751***	3.691***
	(10.45)	(11.57)	(12.92)	(13.42)	(13.01)
D(CVC 3yr) × D(Ind. Proxy SIC2)	-2.352***	-2.394***	-2.390***	-2.411***	-2.372***
	(-4.93)	(-4.95)	(-4.98)	(-4.98)	(-4.90)
D(CVC 3yr) × D(Ind. Proxy SIC1)	-1.560**	-1.485**	-1.460**	-1.434**	-1.555**
	(-2.50)	(-2.43)	(-2.41)	(-2.38)	(-2.49)
D(CVC 3yr) × Frac(Startups IPO)	0.328**				
	(1.98)				
D(CVC 3yr) × Frac(Startups Acquired) by Third Party)		-0.0177			
		(-0.10)			
D(CVC 3yr) × Frac(Startups Acquired) by CVC Parent Itself)			0.125*		
			(1.69)		
D(CVC 3yr) × Frac(Startups Bankruptcy)				-0.875	
				(-0.48)	
D(CVC 3yr) × Frac(Positive Patent Signal)					0.232*
					(1.78)
Industry Cluster Controls					
IPO Cluster	0.0227***	0.0226***	0.0226***	0.0226***	0.0226***
	(9.56)	(9.55)	(9.55)	(9.55)	(9.55)
Acquisition Cluster	0.0122***	0.0123***	0.0123***	0.0122***	0.0122***
	(5.76)	(5.77)	(5.77)	(5.76)	(5.76)
Patent Cluster	0.000409**	0.000410**	0.000410**	0.000410**	0.000411**
	(1.98)	(1.98)	(1.98)	(1.98)	(1.98)
D(CVC 3yr) × IPO Cluster	-0.0149***	-0.0133**	-0.0131**	-0.0134**	-0.0133**
	(-2.77)	(-2.52)	(-2.48)	(-2.54)	(-2.48)
D(CVC 3yr) × Acquisition Cluster	-0.00468	-0.00593*	-0.00587*	-0.00591*	-0.00566*
	(-1.40)	(-1.89)	(-1.87)	(-1.90)	(-1.79)
D(CVC 3yr) × Patent Cluster	-0.0000563	-0.0000576	-0.0000738	-0.0000527	-0.0000723
	(-0.19)	(-0.19)	(-0.24)	(-0.18)	(-0.24)
CLOGIT grouped by Firm-Year	✓	✓	✓	✓	✓
Industry F.E.	✓	✓	✓	✓	✓
Num. Obs.	234,539	234,539	234,539	234,539	234,539
Pseudo R^2	0.181	0.180	0.180	0.180	0.180

Table 2.8: CVC Signal Response and Adding Emerging Phrases

This table presents the analysis of CVC investments and the integration of emerging business across VEIC industries. The sample in Column (1) is at the Firm-Year-VEIC level, while the remaining columns use the Firm-Year level data. The dependent variable is the number of VEIC-*j* specific emerging phrases newly added into the 10-K. Each emerging phrase is sorted into 8 VEIC industries. 8 VEIC industries are Biotechnology; Communication and Media; Computer Hardware; Computer Software; Internet Specific; Medical and Health; Non-High-Tech; and Others. D(CVC VEIC *j*) is equal to 1 if the firm invests at least one CVC-backed startup in the VEIC Industry *j* in the past three years. T-statistics are shown in parentheses, and standard errors are clustered by firm. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: By VEIC industry	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Number of Newly Added Emerging Phrases (VEIC *j*) (with ln())								
	All VEIC	Biotech- -nology	Communic- -ation	Computer Hardware	Computer Software	Internet Specific	Medical Health	Non-High- -Tech	Others
D(CVC VEIC j)	0.142***	0.0589**	0.0809***	0.00213	0.103***	0.131**	0.0107	-0.0204*	0.0541***
	(10.89)	(2.45)	(3.01)	(0.16)	(3.04)	(2.64)	(0.68)	(-1.75)	(4.39)
Firm F.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓
VEIC*Year F.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Num. Obs.	616,544	75,578	75,578	75,578	75,578	75,578	75,578	75,578	75,578

Panel B: Interact with the signal variable	(1)	(2)	(3)	(4)	(5)
	Number of Newly Added Emerging Phrases (VEIC *j*) (with ln())				
D(CVC VEIC j)	0.120***	0.122***	0.119***	0.122***	0.122***
	(8.17)	(7.08)	(6.99)	(7.07)	(7.06)
D(CVC VEIC j) × Num(Startups IPO)	0.0216**				
	(1.99)				
D(CVC VEIC j) × Num(Startups Acquired by Third Party)		0.000			
		(0.00)			
D(CVC VEIC j) × Num(Startups Acquired by Parent Itself)			0.0251***		
			(3.51)		
D(CVC VEIC j) × Num(Startups Bankruptcy)				-0.00697	
				(-0.74)	
D(CVC VEIC j) × Patent Growth Signal					0.00360***
					(3.31)
D(CVC VEIC j) × Num(Startups Invested)	0.00818***	0.00999***	0.00935***	0.0102***	0.00997***
	(4.90)	(3.45)	(3.98)	(3.49)	(3.71)
Firm F.E.	✓	✓	✓	✓	✓
VEIC*Year F.E.	✓	✓	✓	✓	✓
Num. Obs.	616,544	616,544	616,544	616,544	616,544
Adj. R^2	0.183	0.183	0.183	0.183	0.183

Table 2.9: First Stage Regression regarding CVC Instrument

This table presents the first stage regression regarding the instrument variable of CVC investments. I use the VC fund inflow shock of those independent VC firms in the past 5-year syndicate network of CVC Firm i as the instrument of CVC investments by the CVC Firm i. Figure 2.7 provides an example about how the instrument works. The regression sample follows Table 2.4 and further requires that the firm has invested at least one CVC deal in the past five years (and thus enjoys some networks with IVCs). The instrument variable, Granular IV, is defined as the sum of the idiosyncratic fund inflow shocks of those IVCs in the past 5-year syndicating network. Num(IVC in the Network) is the natural logarithm of one plus the number of IVCs in the past 5-year syndication network of CVC Firm i. Industry VC Deal Flow is measured by the total amount of VC deals in the SIC-2 industry in Year t. The dependent variable Num(CVC Deal) (Num(CVC Initial Deal) for Column (4) to (6)) is the natural logarithm of one plus the number of CVC deals (CVC initial deals) conducted by the Firm i in Year t. The CVC initial deal is defined as the deal in which case the CVC firm invests in an entrepreneurial Start-up j for the first time, that is, not the follow-on investments. The standard errors are clustered at the CVC firm level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Num(CVC Deal)			Num(CVC Initial Deal)		
Granular IV (IVC Fund Inflow Shock)	0.100***	0.0987***	0.0937***	0.0866***	0.0858***	0.0824***
	(14.59)	(13.73)	(11.48)	(13.62)	(13.90)	(11.17)
Num(IVC in the Network)	0.182***	0.189***	0.218***	0.123***	0.128***	0.148***
	(5.00)	(5.03)	(4.69)	(4.30)	(4.33)	(3.99)
IVC's Average Age In the Network		-0.0146***	-0.0225***		-0.0144***	-0.0224***
		(-3.84)	(-4.00)		(-3.83)	(-4.04)
IVC's Average Past IPO In the Network		0.00870	-0.00176		0.0132	0.00279
		(1.00)	(-0.15)		(1.53)	(0.23)
Industry VC Deal Flow	0.00204	0.00207		0.00197	0.00203	
	(1.42)	(1.45)		(1.28)	(1.32)	
D(CVC Past 1yr)	0.311***	0.297***	0.302***	0.173***	0.160***	0.155***
	(12.42)	(12.12)	(9.17)	(7.74)	(7.15)	(4.75)
D(CVC Past 2yr)	0.0815***	0.0742***	0.0921***	0.0250	0.0179	0.0373
	(3.71)	(3.28)	(3.15)	(1.26)	(0.87)	(1.37)
D(CVC Past 3yr)	0.0166	0.0100	0.0257	-0.00219	-0.00840	-0.000671
	(0.63)	(0.38)	(0.84)	(-0.09)	(-0.33)	(-0.02)
Firm Controls:	Firm Size, Tobin's Q, ROA, R&D, Leverage, Capx., HHI, D(Conglo), Age					
Year Fixed Effect	Yes	Yes	No	Yes	Yes	No
Industry Fixed Effect	Yes	Yes	No	Yes	Yes	No
Industry*Year Fixed Effect	No	No	Yes	No	No	Yes
Num. Obs.	3,236	3,236	3,236	3,236	3,236	3,236
Adj. R^2	0.539	0.548	0.560	0.481	0.487	0.497

Table 2.10: CVC Investments and Firm Scope Change: 2SLS Estimator

This table presents the 2SLS regression regarding CVC investments and the subsequent firm scope change. The regression sample consists of all Compustat firms which are incorporated in the US and conduct at least one CVC deal in the past five years (and thus enjoy the IVC network formed by the past investments). In Column (2), the left-hand side variable, Business Change, captures the general business change of a CVC parent firm. It is defined as one minus the cosine similarity between the firm's textual business description in Year t and Year t+1. The variable construction follows Hoberg et al. [2014]. The instrument variable, Granular IV, is defined as the sum of the idiosyncratic fund inflow shocks of those IVCs in the past 5-year syndicating network. Num(CVC Initial Deal) is the natural logarithm of one plus the number of CVC deals (excluding follow-on investments) conducted by the Firm i in Year t. Industry fixed effects are defined in SIC-3 Industries. T-statistics are shown in parentheses, and standard errors are clustered at the CVC Firm level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

2SLS	(1)	(2)	(3)	(4)	(5)	(6)
	Textual Measure			Segment Dummies		
Time Period	Emerging Phrases [t+1]	Business Changes [t+1]	New Products [t+1]	New Division [t+1, t+2]	Remove Divisions [t+1, t+2]	Change Industry [t+3, t+5]
Num(CVC Initial Deals) (Instrumented by GIV)	0.851***	3.766**	0.347**	0.079**	-0.028	0.065**
	(2.728)	(2.501)	(2.364)	(2.022)	(-0.559)	(2.158)
Num(IVC in the Network)	-0.033	-0.180	-0.033	0.012	0.008	0.015
	(-0.241)	(-0.228)	(-0.528)	(0.665)	(0.366)	(0.749)
IVC's Average Age in Network	0.048**	0.128	0.020*	0.001	-0.001	0.000
	(2.303)	(1.015)	(1.793)	(0.394)	(-0.234)	(0.145)
IVC's Average Past IPO in Network	-0.207**	-0.287	-0.026	-0.001	0.008	-0.006
	(-2.069)	(-1.069)	(-0.984)	(-0.301)	(1.519)	(-1.047)
D[CVC Past 1yr]	0.072	0.540	0.114	0.007	0.000	0.007
	(0.474)	(0.517)	(1.182)	(0.355)	(0.016)	(0.367)
D[CVC Past 2yr]	-0.135	-1.329	-0.034	-0.009	0.010	0.013
	(-1.031)	(-1.526)	(-0.411)	(-0.702)	(0.638)	(1.156)
D[CVC Past 3yr]	-0.068	-0.407	0.137	0.008	0.012	0.004
	(-0.491)	(-0.439)	(1.550)	(0.585)	(0.760)	(0.323)
Kleibergen-Paap F statistic	192.46	107.99	63.21	127.06	127.06	127.06
Other Firm Controls	✓	✓	✓	✓	✓	✓
Industry*Year F.E.	✓	✓	✓	✓	✓	✓
Num. Obs.	1450	1569	567	2474	2474	2474
R^2	0.065	0.030	0.419	0.026	0.083	0.051

Table 2.11: CVC Investments and Firm Scope Change: Evidence with Airline Route

This table presents the post-CVC scope change analysis using the US airline route as a quasi-natural experiment. I match the CVC deals sample with the US T-100 Airline Domestic Segment database from 1990 to 2017. CVC deals sample only includes CVC unrelated deals and deals in which the start-up is located in the US. A CVC deal is identified as *a deal with direct flights* if there are direct airline flights, during the year right after the investment (deal) year, between the metropolitan statistical area (MSA) of the CVC firm and the MSA of the start-up's headquarter. Panel A provides summary statistics about the CVC deal sample. Deals are broken down by those with and without direct flights. I exclude deals in which the start-up and the CVC firm are located in the same MSA. Panel B and Panel C provide OLS regressions estimated with the CVC deal level sample. The dependent variable in Panel B is a dummy equal to 1 if the CVC firm creates a new division within the next two years after the deal and the newly created division is in the same SIC-3 industry of the start-up in the deal. In Panel C, the dependent variable is a dummy equal to 1 if the CVC firm changes its primary industry within 3-5 years after the deal and the industry the firm changes to is the same as the start-up's industry in the deal. CVC Parent controls are Firm Size, ROA, Book Leverage, Capx, HHI, and D(Conglomerate). T-statistics are shown in parentheses, and standard errors are clustered by CVC firm. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Summary statistics

CVC Deals Sample	With Direct Flights			Without Direct Flights			Test of Mean
	Mean	S.D.	N	Mean	S.D.	N	p value
Location Variables							
% Start-ups in CA	46.91%	0.499	2262	38.69%	0.487	765	0.000
% Start-ups in NY	9.55%	0.294	2262	3.40%	0.181	765	0.000
% Start-ups in MA	14.32%	0.350	2262	10.07%	0.301	765	0.002
% CVC Firms in CA	47.75%	0.500	2262	50.72%	0.500	765	0.155
% CVC Firms in NY	8.71%	0.282	2262	6.67%	0.250	765	0.075
% CVC Firms in MA	3.09%	0.173	2262	3.27%	0.178	765	0.812
Distance (miles)	1347.42	988.10	2222	1263.34	995.51	749	0.045
Start-up Variables							
Start-up's Age	6.625	5.826	2002	7.181	6.120	689	0.033
Num. Co-investors	5.722	3.762	2262	6.022	3.679	765	0.055
CVC Paret Variables							
Firm Size (Total Sales)	9.579	1.773	2259	9.397	1.700	765	0.013
ROA	0.156	0.266	2247	0.159	0.179	762	0.810
Book Leverage	0.079	0.213	2259	0.069	0.067	764	0.196
R&D Exp.	0.306	0.264	2237	0.334	0.248	762	0.289
HHI	0.083	0.073	2262	0.081	0.072	765	0.576

Panel B: Regression analysis – Creating New Divisions					
	(1)	(2)	(3)	(4)	(5)
OLS	D(Create New Div. *in Ind. of Startup*)[t+1,t+3]				
Num(Non-Stop Flights)	0.00345***	0.00342***	0.00469***	0.00356**	0.00353***
	(2.98)	(2.67)	(2.80)	(2.13)	(2.69)
Start-up's Age	-0.00143	-0.00175	0.000466	0.00221	0.000653
	(-0.63)	(-0.77)	(0.18)	(0.83)	(0.38)
Num. Co-investors	-0.00112	-0.00102	-0.00109	0.00116	0.00000837
	(-1.07)	(-0.96)	(-0.81)	(1.06)	(0.01)
D(Seed or Early Stage)	-0.00176	-0.00372	0.00208	0.0140	0.0105
	(-0.12)	(-0.28)	(0.15)	(0.88)	(0.76)
D(Same MSA Area)	-0.00749	-0.00217	0.00443	0.000515	0.00663
	(-0.66)	(-0.20)	(0.36)	(0.04)	(0.59)
Distance	-0.00314	-0.00341	-0.00435	0.000191	0.00200
	(-0.81)	(-0.86)	(-0.77)	(0.04)	(0.49)
CVC Parent Controls		✓	✓	✓	✓
Year F.E.	✓	✓			
Start-up MSA F.E.	✓	✓			
CVC Firm MSA F.E.	✓	✓	✓		
Start-up MSA × Year F.E.			✓	✓	✓
CVC Firm MSA × Year F.E.				✓	✓
CVC Firm F.E.					✓
Num. Obs.	3275	3212	2923	2764	2705
Adj. R^2	0.074	0.094	0.111	0.272	0.450

Panel C: Regression analysis – Changing Primary Industry					
	(1)	(2)	(3)	(4)	(5)
OLS	D(Change Industry: *Shift to Start-up*)[t+3,t+5]				
Num(Non-Stop Flights)	0.00240**	0.00244**	0.00341**	0.00218*	0.00217*
	(2.10)	(2.10)	(2.12)	(1.72)	(1.66)
Start-up's Age	-0.00162	-0.00177	-0.00146	-0.000933	-0.000663
	(-1.10)	(-1.10)	(-0.77)	(-0.67)	(-0.54)
Num. Co-investors	0.000541	0.000449	0.000476	0.00128*	0.000457
	(0.79)	(0.70)	(0.76)	(1.82)	(0.85)
D(Seed or Early Stage)	0.00105	-0.000726	-0.00419	0.000856	0.000631
	(0.13)	(-0.10)	(-0.81)	(0.11)	(0.10)
D(Same MSA Area)	-0.00697	-0.00680	-0.000688	0.00220	0.000625
	(-0.81)	(-0.90)	(-0.09)	(0.32)	(0.11)
Distance	0.000556	0.000551	0.00215	0.00222	-0.000925
	(0.20)	(0.19)	(0.57)	(0.58)	(-0.33)
CVC Parent Controls		✓	✓	✓	✓
Year F.E.	✓	✓			
Start-up MSA F.E.	✓	✓			
CVC Firm MSA F.E.	✓	✓	✓		
Start-up MSA × Year F.E.			✓	✓	✓
CVC Firm MSA × Year F.E.				✓	✓
CVC Firm F.E.					✓
Num. Obs.	3275	3212	2923	2764	2705
Adj. R^2	0.055	0.061	0.022	0.113	0.377

Table 2.12: Post CVC Firm Value Creation

This table studies the post-CVC value creation of CVC parents. The dependent variable is the difference of Tobin's Q between Year $t+h$ and Year t, where h is shown in the table. All dependent variables are winsorized at 1% and 99% level before being brought into regressions. Industry fixed effects are defined in SIC-2 industries. T-statistics are shown in parentheses, and standard errors are clustered by firm. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Change of Tobin's Q of the CVC Parent							
$\Delta =$	(t+3)-t	(t+4)-t	(t+3)-t	(t+4)-t	(t+3)-t	(t+4)-t	(t+5)-t	(t+6)-t
D(CVC Unrelated)	0.304***	0.307***						
	(3.42)	(3.21)						
D(CVC Related)	-0.186	-0.256	-0.133	-0.155	-0.142	-0.166	-0.201	-0.0983
	(-1.04)	(-1.27)	(-0.67)	(-0.79)	(-0.71)	(-0.84)	(-0.98)	(-0.40)
D(CVC Unrelated) $\times$ D(New Div.)[t+1,t+2]			0.363***	0.376***				
			(2.69)	(3.17)				
D(CVC Unrelated) $\times$ $\big(1-$D(New Div.)[t+1,t+2]$\big)$			0.0766	-0.00393				
			(0.73)	(-0.03)				
D(CVC Unrelated) $\times$ D(Div. Rem.)[t+1,t+2]					0.538**	0.592**		
					(2.11)	(2.01)		
D(CVC Unrelated) $\times$ $\big(1-$D(Div. Rem.)[t+1,t+2]$\big)$					0.0857	0.0139		
					(0.81)	(0.12)		
D(CVC Unrelated) $\times$ D(Chg. Ind.)[t+3,t+5]							0.299*	0.321*
							(1.81)	(1.70)
D(CVC Unrelated) $\times$ $\big(1-$D(Chg. Ind.)[t+3,t+5]$\big)$							0.0271	0.0660
							(0.23)	(0.58)
Firm Controls	Firm Size; ROA; Cash; R&D; Leverage; Capital Exp.; HHI; D(Conglomerate)							
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Num. Obs.	74,128	65,292	74,128	65,292	74,128	65,292	57,747	51,249
Adj. R^2	0.080	0.075	0.252	0.257	0.252	0.257	0.287	0.291

Chapter 3

Activism Pressure and the Market for Corporate Assets

Joint with Ulrich Hege (TSE)

3.1 Introduction

The rise of shareholder activism in the last two decades has spurred academics to analyze various aspects of activism, such as gains in value and economic performance following campaigns. But many of the real effects of activism campaigns remain largely unexplored, including effects on other firms, stakeholders, and markets.

This paper explores the impact of hedge fund activism on markets for corporate transactions. A small literature has analyzed the impact of activism on target firms' decisions to acquire and sell assets. Our paper extends the analysis beyond activism targets to firms that are not yet targeted by activists but indirectly exposed to activism threats, and looks at the impact on the supply and demand for corporate assets. We explore the effects of activism pressure on corporate asset markets by studying its impact on transaction volumes, real asset liquidity, transaction prices, and economic efficiency gains.

We try to answer the following questions: Does activism affect the acquisition and asset sale decisions of firms that are only indirectly affected by activists? Has activism grown sufficiently in importance that it influences the equilibrium in corporate asset markets, and what is its impact on the liquidity and efficiency of these markets? Our focus on the market externalities of activism is in contrast to most of the literature on shareholder activism that has mostly limited its investigation to effects on target firms. There is little literature on peer effects and spillovers beyond target firms. No earlier study has tried to estimate the effect of activism threats on acquisition behavior of firms, or the effect of activism on the equilibrium outcome in asset markets.

Our paper takes into account a wide range of corporate transactions: takeovers and mergers, divestitures, and acquisitions, including acquisitions of private targets. Confirming and extending earlier studies, we find that firms directly targeted in activist campaigns are more likely to receive merger bids, make more divestitures, and make fewer acquisitions. We show that the reduction in acquisition activity is due to larger firms, whereas smaller firms' frequency of making acquisitions shows no significant change.

We then consider firms' exposure to activism threats as a second channel of activism pressure and study its impact on firms' behavior in corporate asset markets. We first consider the threat impact for firms individually, by estimating their probability of becoming an activism target in the near future. However, since we want to study the effect of activism pressure on entire asset markets, our principal measures of the impact of activism threat are aggregated at the industry level (3-digit SIC codes). We use the frequency of recent activist campaigns in the industry as our main measure of changes in activism threats. We also use the jumps in activists hedge funds' stakes (both active and passive) in the industry as a second measure.

Whether we use firm-level or industry-level metrics of HFA threat exposure, we show that firms behavioral adjustment following threat increases goes in the same direction as the reaction of activism targets: firms sell more assets, are more likely to be acquired, and on average also tend to acquire less. The latter effect, however, is nuanced: only large firms make fewer acquisitions, whereas small firms maintain or increase their acquisitions activity.

Endogeneity is a concern in any study on the impact of activism. Activism targets might be selected because of unobserved characteristics that drive the observed changes in firm behavior, or because activists anticipate value-enhancing developments in those firms rather than being at the origin of those changes. We address these concerns in various ways. First, for target firms (for which such concerns are particularly important since firms exposed to activism threats are not selected firms by activists), we use an approach pioneered by Brav et al. [2015a] and look at the effect when a hedge fund, for a given hedge fund-activist pair, switches from a sizable passive stake in a given firm (Schedule 13G filing) to an activist stance (Schedule 13D filing). We show that such switches produce a significant change in firms' corporate transactions in the same direction we found earlier, providing a "clean identification of intervention beyond stock picking", in the words of Brav et al. [2015a].

Second, for firms under activism threat, by using industry-level measures of hedge fund pressure and thus assuming that all firms in an industry face the same threat level, we eliminate any effect of unobserved firm-level characteristics beyond those common to all firms in the industry. This still leaves the concern that selection effects arise at the level of industries, i.e. hedge funds select entire industries (rather than firms) because of common characteristics associated with the observed change in acquisition markets.

Third, therefore, we address this concern with an instrumental variable that is built on the idiosyncratic fund inflow shock of each activist hedge fund, and we hypothetically reassigns the new fund inflow according to the previous industry holding structure of each hedge fund, similar to the well-known instrument of mutual fund fire sales (Coval and Stafford [2007], Edmans et al. [2012]).[1] Thus, the instrument dissociates the increase in activist's targeting from their selection of industries. We find that our findings of the change in corporate asset markets remain in place when we use this instrument. We are also careful to control for any factors that explain the clustering of acquisition activity in industries, or merger waves (Harford [2005b]), in order to address potential associations with the target selection of hedge fund activists. We find no clear association between merger waves and hedge fund target selection.[2]

Having established that activism pressure affects the behavior of both target firms as well as of firms under activism threat, we try to find out which of these two channels is more important for corporate transaction markets. Activist targets change their behavior dramatically but only a few firms are targeted in a typical industry at any given time, whereas many more firms are exposed to activism threats - our main threat measures assume that *all* firms in the industry are equally exposed - , with moderate impact on their behavior. We find that the overall impact that we attribute to firms under activism threats is about the same as that attributed to activist targets, with a larger relatively effect on the demand side (acquisitions), and a smaller effect on the supply side (mergers and divestitures).

We estimate that firms in industries in the top quintile of activism pressure sell on average about 23% more assets, and make close to 12% less acquisitions, leading to a combined shift in the relation between demand and supply for corporate assets of roughly 35%. We expect this squeeze in real asset liquidity to have an effect both on transaction volume and on transaction prices.

[1]The same instrument has been used in the previous studies looking at threat effects of hedge fund activism, Gantchev et al. [2017], Feng et al. [2017].

[2]The literature on the relationship between industry takeover activity, industry concentration and industry demand provides the background for such concerns (see Mitchell and Mulherin [1996], Andrade and Stafford [2004], Bernile et al. [2012]). No earlier study has looked at determinants of merger waves predicting the selection of activist targets, but Boyson et al. [2017] find that merger waves do not lead to more activism mergers.

Hence, we consider the impact on liquidity in highly affected industries. When firms in an industry under activism pressure simultaneously aspire to sell more and buy fewer assets, then real asset liquidity dries up, creating a role for outside liquidity providers. Indeed, we find that outside acquirers - private equity funds, private firms, and listed firms in other industries - provide liquidity and that their acquisition volume increases in affected industries. We show that this difference is due to private equity providing asset liquidity only in industries with high asset redeployability, and that outside asset liquidity provision is stronger in these industries.

We then explore whether the squeeze in real asset liquidity also affects transaction prices. We find evidence consistent with this hypothesis: seller announcement returns are smaller in corporate sales when industries are affected by activist pressure (merger bids and divestiture bids), and buyer announcement returns are (weakly) larger in this case. We do not find evidence for a similar price prize effect for activist target firms - thus, unlike other firms in industries under heavy activist pressure, activist target firms themselves are not affected.

Finally, we consider whether activism pressure improves the efficiency of corporate transactions, in the sense of transactions creating more long-run value. We look at accounting measures and Tobin's Q as a stock-based measure of long-run performance. We control for the documented impact of activism campaigns and of corporate transactions on long-run performance, and isolate the incremental effect of transactions done under activism influence. We find positive long-run performance effects when corporate transactions are undertaken by activism targets. We do not find similar effect for transactions undertaken under activism threat. The direct involvement of activists appears to be a necessary ingredient for activism pressure to produce additional efficiency gains in corporate transactions.

Our paper contributes to various strands of the literature. It extends earlier work on activism targets' behavior in corporate transactions (reviewed in the next section) by showing that firms under activism threats adjust their behavior in the same direction. There is a small literature on threat effects of activism (reviewed below) to which our paper adds findings on the effect of activism threats on firms' behavior in the market for corporate assets. Our paper also contributes to the analysis of strategic interactions between firms exposed to activism and rival firms. Aslan and Kumar [2016] show that following an activist campaign, rival firms of the campaign target lose market share and have reduced profitability, akin to competition in strategic substitutes. We find that rival firms adopt behavioral changes similar to those of that of activism targets, and that the overall impact on targets and rivals is sufficiently profound so as to affect the liquidity and valuation in real asset markets. A final contribution of the paper is to the literature on firm size and acquirer performance (see Moeller et al. [2004]); we show that activism further accentuates the difference in long-run acquisition performance between large and small acquirers. We also show that there is sharp distinction in acquisition activity, with small firms making more and large firms less acquisition under activism threats.

The paper is also related to the wider literature on the real effects of hedge fund activism.[3] Academic researchers have analyzed the value gains following activism campaigns (e.g., Brav et al. [2008a], Greenwood and Schor [2009], Becht et al. [2017]) and have shown that activism campaigns improve the operations and profitability of targets (Bebchuk et al. [2015a], Aslan and Kumar [2016], Brav et al. [2015a]),[4] their competitive position in product markets (Aslan and Kumar [2016]), and the quality of their innovation effort (Brav et al. [2018]). Our paper contributes a number of aspects to the analysis of real effects of activism, for example by showing that post-activism corporate transactions improve the economic efficiency of sellers, but less so for firms acting under activism threat, and that only smaller firm seem to be able to generate performance gains from activism acquisitions.

[3]See Denes et al. [2017] and Brav et al. [2015b] for surveys. The literature has also investigated other topics to which our paper is related, such as the international expansion of activism (see Becht et al. [2017]) and the determinants of activism target selection (Brav et al. [2008a]).

[4]There is some controversy concerning the improvement in long-term performance, see deHaan et al. [2018] for size effects or Grennan [2014] for evidence on short-termism.

The paper is organized as follows. Section 3.2 discusses literature and hypotheses. We explain our sample construction and methodology in Section 3.3. Section 3.4 analyzes the impact of activism on mergers, divestitures, and acquisitions. In Section 3.5, we investigate how activism pressure alters the equilibrium in the market for corporate assets and affects real asset liquidity and asset prices. We investigate the impact on the long-run efficiency of corporate transactions in Section 3.6. Section 3.7 concludes.

3.2 Literature and Hypotheses

There are theoretical and empirical papers supporting the view that hedge fund activism affects firms' decision-making in the market for corporate assets. Theoretical models explaining why activism targets frequently become takeover targets include Burkart and Lee [2018] who show that activists reduce ex ante *and* ex post free-riding in takeovers, and Corum and Levit [2017] who demonstrate that activist toeholds act as facilitators of future takeovers. The empirical literature on activism mergers shows that activist targets have a substantially higher probability to receive merger bids (Boyson et al. [2017], Becht et al. [2017]). Gantchev et al. [2018] find that activism campaigns reduce firm's propensity to make acquisitions, increase the frequency of divestiture, and improve the quality of transactions, measured by long-run performance.

Concerning activism threats, the idea that firms react to activism pressure even if they are not target firms is related to the literature on the disciplining effect of the market for corporate control that stipulates that takeover threats influence the decisions of companies that are not takeover targets (see Grossman and Hart [1980] for a seminal theory contribution and Bertrand and Mullainathan [2003a] for evidence). The concept of activism threats has been developed theoretically e.g. in Edmans and Manso [2011] and Fos and Kahn [2016]. Thus, when facing heightened activism threat, managers should proactively adjust their behavior in anticipation of increased activism risk. Gantchev et al. [2017], Feng et al. [2017], and Bourveau and Schoenfeld [2017] present supportive evidence for this view.

Besides the disciplining effect of activism threats, there could be other motives that would lead firms under activism threat to adopt behavior similar to that of campaign targets. Firms might also simply mimic the behavior of closely watched rivals that are activist targets. Alternatively, they might react because of strategic interaction effects with activist targets in product or asset markets. Strategic interaction effects between activism targets and rivals, however, do not yield a clear prediction concerning the direction of rivals' adjustments; the optimal strategic response of rivals may have the opposite sign of the behavioral adjustment of activism targets, consistent with competition in strategic substitutes. Indeed, Aslan and Kumar [2016] study product market interactions of activism and find that activism targets increase their market share and profitability whereas product market rivals suffer reductions in market share and mark-ups. If rivals' reaction is in strategic substitutes, the strategic interaction effect would dampen rather than reinforce the impact of activism on corporate asset markets that we study.[5] Throughout, we remain agnostic about the exact motives that lead to the behavioral change on acquisition markets.

The decrease in asset purchases and the increase in asset sales in affected industries should affect asset markets. When more assets are sold and fewer are bought, real asset liquidity for sellers is reduced. The effect is related to the argument by Shleifer and Vishny [1992] that industry peers and hence insiders are the highest-value acquirer of any assets in an industry that is for sale. There is also a substantial theoretical and empirical literature on asset fire sales (see Shleifer and Vishny [2011] for a survey). The concept of real asset liquidity has been explored empirically by Schlingemann et al. [2002], Ortiz-Molina and Phillips [2014], and Kim and Kung [2017], among others.

[5]From a theoretical point of view, the sign of the predicted rival reactions in response to the changed behavior of campaign targets is not unique; it depends on whether firms compete in strategic substitutes or strategic complements.

The effect on real asset liquidity will change the industry equilibrium in the asset market. Following standard general equilibrium arguments, we expect a measurable effect both along the quantity and the price dimension. Specifically, with a high level of hedge fund activism, industry insiders that are listed firms and hence potentially also activism targets, will also feel pressure to sell assets and to curtail acquisitions. They are unlikely to be in a position to be providers of asset liquidity rather than liquidity seekers. This role should more fall to industry outsiders - private equity firms, private firms, and firms that operate predominantly in other industries - than industry insiders.

Finally, when studying the effect of activism on the efficiency of corporate transactions, the neoclassical view that corporate acquisitions serve the purpose of reallocating assets to more efficient uses has long dominated economics (Jovanovic and Rousseau [2002]), but the evidence is mixed. Maksimovic and Phillips [2001] find that plant-level efficiency improves following a merger, but studies based on Tobin's Q do not yield a clear consensus.

The theoretical and empirical literature on the relationship between corporate governance and acquisition markets is also relevant in this context. The literature has considered empire building and value-destroying acquisitions as a prominent dimension of managerial agency costs (Jensen [1986], Morck et al. [1990]), and has emphasized the disciplining role of the market for corporate control on acquisition behavior (Mitchell and Lehn [1990]). Indeed, acquirer returns in acquisitions of public targets are low, though the ex post performance of mergers and acquisitions has generally been shown to be positive (Andrade et al. [2001]). There is evidence that acquirers with better corporate governance have higher acquisitions returns (Masulis et al. [2007]), but literature directly linking the governance role of active shareholders to ex post long-term merger performance is scant. There is also a literature showing that acquirer returns and long-term post-acquisition performance are significantly higher for smaller acquirers (Moeller et al. [2004], and Gorton et al. [2009]. In view of this evidence, it seems plausible that activism targets will execute more efficient transactions since they are co-governed by activist funds, but that the efficiency of transactions done by firms under activism threat improves less since they latter do not benefit from close monitoring by activist shareholders. It seems also plausible that the role of firm size in acquirer performance extends to the analysis of acquisitions done under activism pressure.

To summarize the hypotheses that we investigate, we first expect activism targets as well as firms under activism threat to be more likely to make divestitures or to be sold, and to make fewer acquisitions compared with other firms. Small firms are possibly under less pressure to reduce acquisitions to the extent that their acquirer returns are positive.

We expect these common trends to affect the equilibrium in corporate asset markets: in industries with heightened activism pressure, the supply of real assets should increase and the demand for real assets decrease. The ensuing reduction in the liquidity of corporate asset markets should lead to a squeeze in transaction prices, and create a role for asset liquidity provision by outside market participants.

Finally, we expect corporate transactions under activism pressure to show efficiency gains, and these gains potentially to be larger for activism targets than for firms under activism threat because of the stronger governance effect of an activism campaign.

3.3 Sample Construction and Methodology

3.3.1 Samples of activism events and corporate transactions

We construct a comprehensive sample of hedge fund activism (henceforth: HFA) by combining two data sources: the sample originally studied in Brav et al. [2008a] that has been updated by Alon Brav and Wei Jiang to include the more recent time period[6] and the

[6] We are grateful to Alon Brav and Wei Jiang for generously sharing their proprietary data with us.

FactSet SharkWatch database. The two databases are only partially overlapping as they use complementary sampling strategies: Brav and Jiang identify hedge fund activism campaigns mainly through the initial (the first relevant) Schedule 13D filling submitted to the Securities and Exchange Commission (SEC)[7] whereas FactSet SharkWatch focuses on public campaigns and identifies them from various sources, such as press releases, financial news, Schedule 13D fillings and proxy statements, and thus is able to track public campaigns also when activists have ownership below 5%. When combining the two samples, we carefully screen the data and remove any duplicates. We find that 1,728 of 3,537 campaigns in Brav's extended sample are also recorded in FactSet SharkWatch.[8] We follow Boyson et al. [2017] and merge multiple hedge fund activism campaigns targeting a single firm in any calendar year as a single activism observation, starting at the first recorded announcement date. We obtain a total sample of 4,380 HFA events. We further limit the sample to HFA events that target firms incorporated in the U.S. and included in the CRSP-Compustat Merged Database. This process yields a sample of 3,551 unique HFA campaigns in the U.S. (see Table 3.1, Panel A), and a list of 862 hedge funds that operate as activist hedge funds at least once in our sample and that will be used to distinguish between activist hedge funds and other institutional investors. The activism sample constructed in this way covers the period from 1994 - 2016. We use 1994 as the start date as the earliest possible year with significant hedge fund activism activity, consistent with earlier literature.

We use SDC Platinum for data on corporate transactions for our 1994-2016 sample period and extract and construct three separate transaction samples, covering respectively (1) mergers (U.S. listed firms being acquired), (2) divestitures (sellers are U.S. listed firms), and (3) acquisitions (acquirers are U.S. listed firms.[9] For all three types of cor-

[7]A 13D filing with SEC within 10 days is mandatory when an investor (or a group of investors) owns more than 5% of any class of public shares of the company and intends to influence the management, corporate policy and control.

[8]We only retain HFA events from SharkWatch if at least one of the activists is a hedge fund and if the campaign target is not a fund (such as a closed end or real estate fund). We also drop 292 activist campaigns involving risk arbitrage as in Boyson et al. [2017].

[9]The first and second groups of transactions, mergers and divestitures, are mutually exclusive, but the

porate transactions, we use two identical filters: (*i*) we only retain transactions with an (attempted) control change, i.e. the acquirer owns less than 50% of shares before the bid and the percentage of shares sought is larger than 50%; (*ii*) we only include transactions with a (non-missing) transaction value of at least $10 million.

For the merger sample (i.e., acquisitions of U.S. based listed firms), we exclude divestitures, spinoffs, recapitalizations, self-tender offers, repurchases, partial equity stakes, acquisitions of remaining interest, privatizations, as well as deals in which the target or the acquirer is a government agency. For the divestiture sample, we only retain transactions that are marked as either "divestiture" or "division" in SDC Platinum, and for which there is no other information leading us to conclude that it is not a sale of a corporate unit or subsidiary. We exclude spinoffs and splitoffs, and require the transaction to be completed. For the acquisition sample, we start with the sample of all SDC M&A transactions of which targets are U.S. based listed firms, private firms, or subsidiaries, and the acquirer a listed firm included in the CRSP-Compustat Merged Database. We exclude transactions involving spinoffs, splitoffs, self-tenders and share repurchases.

3.3.2 Firms and industries

We use the universe of U.S. firms in the CRSP-Compustat Merged Database as our baseline sample, both to identify the firms that operate under the impact of activism (the treated sample) as for firms that we consider as unaffected by activism influence (the control sample). We exclude all firms that are not incorporated and headquartered in the U.S., and exclude firm-years with missing historical SIC codes and with missing or negative total sales. Our baseline sample contains 116,448 firm-year observations over the 23 years from 1994 to 2016. From CRSP-Compustat, we get financial and accounting data as well as CRSP stock price information.

acquisitions sample contains the buy side of many, but not all, of the transactions for which the sell side is in the merger or divestiture sample.

We complement the data for our baseline sample with data on institutional ownership from ThomsonReuters' (now Refinitiv's) 13F database. We match our list of 862 activist hedge funds with the ownership 13F database and obtain passive ownership information of those hedge funds (the majority of investments by activist hedge funds are passive investments) and for other institutional investors. Alon Brav and Song Ma graciously provided us with data on 13G filings.

We study markets for corporate assets at the industry level, using 3-digit SIC industries as the baseline to identify corporate asset markets, with a total of 277 industries in our sample. Real assets, in particular intangible assets, are often industry-specific, and industry peers are the most frequent buyers and highest-value bidders for corporate assets (see Shleifer and Vishny [1992]). Earlier work looking at the effects of activism threats also aggregates threats at the industry level (Gantchev et al. [2017], Feng et al. [2017]).

3.3.3 Measures of activism impact

We consider two channels of activism impact, HFA campaigns on one hand and the threat impact of activism on the other hand, and hence define two separate groups of firms affected by activism, firms that are HFA targets and firms under HFA threat. We define the control group as the group of all other firms. At any given point in time, the two groups of firms exposed to activism (the treated firms) are disjoint groups; however, firms frequently change their group assignment over the course of our panel study.[10]

For the first group, HFA targets, we use our sample of 3,551 HFA events described in Section 3.3. We define a dummy variable that is equal to one when an activism event is recorded in our sample, and consider that the impact of this treatment lasts for a number of years, following earlier work that shows that there are long-run effects of HFA

[10] Such transitions in group assignments are expected considering that activism threats are not permanent and that firms under HFA threat are more likely to be targeted than firms in the control group.

targeting even after the end of hedge fund campaigns (see Brav et al. [2008a], Bebchuk et al. [2015a]). Boyson et al. [2017] and Gantchev et al. [2018] show that this persistent effect can also be observed for the acquisition behavior of activism targets. We use a two-year horizon for the impact on corporate transactions following Boyson et al. [2017].

For the second group, firms under activism threat, we begin with firm-level threat measures, recognizing that the HFA threat level is not the same for all firms in an industry. Our variable of choice is the predicted probability of a firm to become a hedge fund activism target in the following year, similar to estimations used in Brav et al. [2008a], Klein and Zur [2009], Feng et al. [2017], and Gantchev et al. [2017]. We also use large passive stakes of activist hedge funds as a second firm-level threat measure. Passive stakes by activists are deemed to capture threats since activists often use passive stakes as launch pad for activism campaigns.

We construct two industry-level metrics that are identical for all firms in an industry as our main measures of the intensity of activism threats. We adopt this approach because of our focus on the impact on real asset markets that are best aggregated at the industry level, and because industry-level measures help to address concerns about selection bias.[11] Our main variable measuring industry-level activism threats is the fraction of recent HFA targets in the industry (at the 3 digit SIC level), that is the fraction of firms that have been targeted by activist hedge funds in last three years. The resulting variable, Industry HFA Frequency, exhibits a strong component of year-to-year fluctuations that should capture changes in the industry-wide threat perception.

The second variable, Industry HFStake Frequency, is constructed to measure the fraction of firms with strong increases in passive and active share holdings by activist hedge funds in the industry level. We compile information from 13F filings (using Thomson

[11]More precisely, they address endogeneity concerns about selection effects the firm level, but still leave open the possibility that hedge funds select firms as targets based on unobserved common industry characteristics and that we address with our instrumental variable approach. Since within a given industry, threat levels vary, our focus on industry-level threat measures should be conservative and weaken our estimated reactions when compared with threat measures that incorporate firm-level heterogeneity.

Reuters 13F database) that record all activist hedge funds holdings, and aggregate the quarterly total ownership by activist hedge funds in firm level. We only include 13F filings of hedge funds on our list of 832 activist funds, thus excluding all other hedge funds and institutional investors. For each firm we define an HF stake jump dummy, D[HFStake], that is equal to one in year t if the total ownership of hedge funds increases during year t by more than 5%. We then aggregate this information at the industry level. The resulting variable, Industry HFStake Frequency, records the fraction of firms (in the industry) that had at least one HF stake jump within last 3 years.

In order to address endogeneity concerns, we construct an additional plausible exogenous measure of changes in activism threats. Inspired by Edmans et al. [2012] and following Gantchev et al. [2017] and Feng et al. [2017], we construct the variable Flow Induced Fund Buy (FIFB) that removes the hedge funds' possibly endogenous decision in which industries they increase their holdings whenever they experience a discontinuous rise in inflows. We first construct a fund inflow shock dummy for each activist hedge fund that is equal to one when the hedge fund's new inflow is larger than 5% of its total net assets measured at the end of the previous year. If this variable is equal to one, we allocate the new fund inflow hypothetically to each industry exactly in the proportions that replicate the fund's industry portfolio structure in the previous year, following exactly the definition of FIFB introduced by Gantchev et al. [2017]. Finally, we sum up the new fund inflows at the industry-year level and obtain the variable FIFB that removes the endogenous firm- and industry-level allocation decision. Whereas Industry HFStake Frequency is based on hedge funds' actual industry allocations, FIFB assigns hypothetical industry weights based on the past industry structure, thus removing industry-level endogeneity.[12]

[12]This argument is supported by at least two observations: (*i*) idiosyncratic fund inflow shocks are very likely to be orthogonal to any unobservable industry characteristics since most of activist hedge funds are general investors, i.e. they diversify investments across industries; and (*ii*) we focus only on large inflows (5%) and allocate them according to the fund's past portfolio following the argument that hedge funds tend to invest quickly and in a mechanical manner when they experience large inflow Coval and Stafford [2007].

3.3.4 Summary statistics

As Panel A of Table 3.1 shows, our sample of HFA events is fairly well distributed over the sample period of 23 years, albeit with a lower intensity in the first 2 years, a peak in 2006-2008, two marked slowdowns during stock market downturns (1999-2001 and 2009-2010), and a strong rebound in HFA activity after 2011. The number of firms in our baseline sample reaches a peak of 6,850 in 1996 and then steadily decreases to 3,990 firms in 2016, largely reflecting the intense M&A activity among listed U.S. corporations (see Doidge et al. [2018]).

Panel B of Table 3.1 presents summary statistics of our threat exposure variables. On average, 6.0% of firms in an industry are activism targets in the current year or in the past 2 years. 10.1% of firms in a given industry experience an increase in hedge funds ownership of more than 5% in at least one year of the current and past 2 years, with a median of 7.7%. There is substantial variance across industries and years in both of our main measures of activism threats, as well as in the variable FIFB that we will use as instrument.

Table 3.1 also reports in Panel C a large number of commonly used firm characteristics, breaking them down between our sample of HFA target firms (N = 3,551) and the remaining firm-year observations in the baseline sample (N = 112,897). This panel provides preliminary insight into the relationship between observable firm characteristics and target selection by activist hedge funds, and the magnitude of the possible selection bias. As expected and in accordance with earlier papers (starting with Brav et al. [2008a]), we find that the differences in institutional ownership, Tobin's Q, market capitalization (in logs), as well as those in dividend yield, cash flow, ROA, sales growth, asset growth, recent stock performance (one-year CAR) and industry concentration are all significant. We discuss in Section E how these firm-level characteristics help to explain the selection of hedge fund targets, and we control for them in our regressions below.

In Panel D of Table 3.1, we present a similar comparison, but this time sort by activism threats. We sort observations into terciles according to our leading industry-level activism threat variable, Industry HFA Frequency. By construction, variations across columns reflect cross-industry differences by tercile of exposure to hedge fund pressure (industries may be assigned to different terciles in different years). Panel D reports quite a bit variation across tercile averages and medians, but the percentage differences are small, with the exception of dividends and cash holdings, and there is hardly any monotonic trend in the variables: differences between the bottom tercile and the middle tercile revert back when we move to the top tercile of industry HFA threats, with few exceptions.[13]

[Insert Table 3.1 Here]

3.3.5 Do our measures of activism threats measure heightened target probabilities?

An important question is how well our variables on industry-level activism threat perform in predicting changes in the probability of individual firms to become activism targets. We use a logit model predicting the probability to become an HFA target, similar to the models used in Brav et al. [2008a], Klein and Zur [2009], Gantchev et al. [2017], and others, and include all variables having been shown to have an impact on the target probability. We then include our industry-level variables of activism threat to see whether they significantly help to explain the probability of being targeted.

The results are presented in Table 3.2. Column (1) reports the benchmark in which we only include the known firm characteristics that help to explain the selection of activism targets. The known strong predictors are all confirmed, in particular small size,

[13] There are four exceptions, consistent with Panel A and the determinants of hedge fund targeting (see Table 3.2): hedge funds are more likely to exert pressure in industries with smaller firms, more institutional ownership, lower dividends and larger cash reserves.

low Tobin's Q, extensive institutional ownership, low dividends and cash flows or ROA, large cash holdings, and underperforming recent stock returns. These variables have some power predicting future hedge fund targeting (pseudo-$R^2 = 0.086$). The next three columns (2) to (4) look at our leading industry variables of activism threats sequentially. We find that each of our three industry measures strongly predicts that firms will become hedge fund targets in the near future, at a 1% level of significance. The contribution to the predictive power is particularly impressive for Industry HFA Frequency that we use as our main variable: our capacity to predict that individual firms will be targeted in the near future increases by 52% ($R^2 = 0.129$). The increase in the predictive power is substantially smaller for the second variable ($R^2 = 0.088$ for Industry HFStake Frequency). Even the variable FIFB that eliminates any effect of hedge funds shifting allocations across industry increases the predictive power (column (4)). These regressions confirm that our industry threat measures constitute a significant determinant of future target probabilities for individual firms in the affected industries. Importantly, the regressions show that a substantial fraction of hedge fund threats is driven by a common industry component, demonstrating that it is rational for firms to change their behavior in reaction to variations in industry threat levels, and providing microeconomic foundations for our investigation of the question whether activism pressure may affect entire corporate asset markets and not just individual firms.

[Insert Table 3.2 Here]

3.4 Deal Activity and Activism

We analyze univariate and multivariate findings of the impact of hedge fund activism on transaction frequencies for all three deal types.

3.4.1 Deal frequencies

This section discusses the univariate evidence on the transaction frequencies for the three types of corporate transactions. We begin with the frequency of merger bids. Greenwood and Schor [2009] show that the bulk of shareholder returns in the wake of activist campaigns can be attributed to activism mergers; Boyson et al. [2017] and Becht et al. [2017] find that the probability of firms being acquisition targets increases very strongly after activism campaigns are launched. Following Boyson et al. [2017], we define a merger bid to be an *activism merger* if it falls within a window of two calender years after the public announcement of the activist campaign (13D filing or announcement date).

Panel A of Table 3.3 shows year-by-year transaction frequencies for the full sample period. In any given year after 1995, between 3.75% and 8.16% of firms in the CRSP-Compustat sample are targets of a merger bid (including unsuccessful bids). The average frequency is 5.17%.[14] For HFA target firms, the average frequency is 10.19%, almost twice as large. The bid frequency is substantially higher in every single year. Panel A also tabulates the merger frequencies for firms that are under High HFA Threat, defined as industries in the top tercile of our Industry HFA Frequency variable (and excluding firms not targeted by activists in the current or the two previous years, in order to disentangle the threat effect from the HFA target effect). The average annual merger bid rate increases to 5.38 %, which is 24% higher than the 4.34% for the firms under Low HFA Threat.

[Insert Table 3.3 Here]

In Panel B, we present the same breakdown for divestitures. On average, each year 5.19 % of listed firms divest business units with a transaction value of more than $10m. This frequency rises by more than 50% to 7.81 % for *activism divestitures*, i.e. divestitures occurring in a two-year window after the start of an activist campaign.[15] For divesti-

[14] The ratios of bids per firm (not reported) are higher since some firms receive multiple bids in a given year.

[15] (Gantchev et al. [2018] also document an increase in activism divestitures.

tures under High HFA Threat (top tercile of Industry HFA Frequency), the divestiture frequency seems to be decreasing slightly when compared with the full sample, but it is 13 % higher than the frequency of low threat firms.

In Panel C, we look at acquisitions, including acquisitions of private firms and business units. On average, the annual rate of making acquisitions of more than $10m recorded is 15.06%, a percentage that decreases to 11.82% for firms with *activism acquisitions* (two-year window after an activist campaign). For acquisitions under High HFA Threat (top tercile), the acquisition frequency decreases slightly to 14.51%, 7.7% lower than for firms in the low HFA threat tercile (15.72%).

Panel D looks only at acquisitions of private targets (private acquisitions henceforth) by firms in the baseline sample. We single out private acquisitions since they represent a deal flow without overlap with the previous panels.[16] 45.8% of acquisitions in our sample are private acquisitions so their share is important. For the private acquisitions in Panel D, the (private) sellers are immune to hedge fund pressure, allowing us to isolate better fluctuations steming from the demand side. The annual rate of private acquisitions of more than $10m. is 7.68%, which decreases by 28.5%, a higher relatively decrease compared to Panel C, to 5.49% for activism acquisitions of private targets. The annual frequency of private acquisitions in the high HFA threat tercile also decreases, to 7.50%.

3.4.2 Corporate transactions of activism targets

Turning to multivariate regressions, we consider campaign targets in this subsection, and the effects of activism threats in Sections 3.4.3 and 3.4.4.

[16]Since firms under activism impact sell more assets and are more likely to be acquired, there will be a corresponding increase in the acquisition numbers in Panel C that reflects this supply-driven surge. Panel A (mergers) and Panel B (divestitures) look at the sell-side of transactions; Panel C reports the entire buy-side of the corporate asset market, and hence also includes a major part of the buy-side for the transactions for which the sell-side is reported in Panels A and B (the completed transactions sold to listed firms dominate our sample).

Table 3.4 shows logit regression results for our firm-year panel. The main explanatory variable D[Activist] is an indicator variable tracking whether the firm is an HFA campaign target in the 2 years prior to each type of transaction (a transaction event is a merger bid in Panel A, a divestiture in Panel B, etc.). In Panel A, the dependent variable is the probability of receiving a merger bid in year t.[17] In all regressions, we use an extensive array of control variables, including variables known to contribute to the frequency of corporate transactions and/or the probability of facing an activism campaign, such as Tobin's Q, size, leverage, institutional ownership, cash, dividends, cash flow, asset and sales growth, recent stock market return, industry concentration (HHI), and real asset liquidity.[18] We include industry and year fixed effects. As expected from earlier studies, the dummy D[Activist] has a very strong and robust effect on the probability of receiving a merger bid ($p < 0.01$), with t-values comprised between 8.37 and 12.99 and a change in predicted probabilities of 92 % (10.49 % vs. 5.45 %). There is no substantial difference whether when we distinguish between merger bids from strategic competitors, from financial buyer groups, or consider unsolicited bids (columns (2) to (4)).

In Panel B, we consider divestitures. We include the same array of control variables as in Panel A and industry and year fixed effects. The results are strong, with the variable of interest D[Activist] highly significant in all specification ($t = 5.22$). Regression (1) shows the baseline regression for all divestitures events. The predicted annual frequency of undertaking a divestiture increases by 41 % (6.44% vs. 4.57%) compared with the full sample. An even higher frequency of divestitures occurs among activist campaign target firms when the activists mention divestitures as an explicit campaign goal (11.63%, almost three times as high as the unconditional frequency). In regressions (3) and (4), we break the sample down by type of buyer, strategic buyer or private equity firm, and find no important difference. Regressions (5) and (6) split the sample between assets that are

[17] D[Activist] is equal to one in year t if activists launch a campaign against the firm during the 730 calendar days prior to the transaction event, or, if there is no transaction event for the firm in year t, during the 2 calendar years prior to the median date of all transaction events of other firms in year t.

[18] We use the measure of Ortiz-Molina and Phillips [2014] that in turn is based on Schlingemann et al. [2002].

related to the seller firm's core activity (3-digit SIC code), and those that are unrelated. Both are highly significant ($p < 0.01$), but show no clear difference.

In Panel C, we turn to acquisitions. Again, we find a highly significant decrease in acquisitions in our benchmark specification in regression (1) ($t = 3.56$). However, the effect is driven by acquisitions of private targets, as is clear when comparing private acquisitions (regression (3), $t = 3.57$) and acquisitions of public targets that show no significant coefficient (regression (5)). In regressions (2) and (4), we split the variable of interest D[Activist] by firm size, inspired by the literature on firm size and acquirer performance (Moeller et al. [2004]); we find that only firms with above-median size (market capitalization) significantly cut back on acquisitions, whereas the variable is insignificant for firms of below-median size. In acquisitions, firm size matters, but we do not find similar effects for sales transactions (mergers and divestitures, not reported in tables). We will return repeatedly to this distinction. We find no difference between acquisitions of related and unrelated assets (columns (6) and (7)).

We are concerned about endogeneity affecting the regression set-up of Panels A to C in Table 3.4. A major concern is that firms' selection as hedge fund target and their change of behavior in the market for corporate assets might be driven by omitted variable bias in the data, or another selection bias. To address these endogeneity concerns, we deploy in Panel D methodology first proposed by Brav et al. [2015a] and distinguish between passive (13G filing) and active stakes (13D filing switched from 13G) by the same activist hedge funds in our sample.[19] The results in Panel D show that mergers become significantly more likely and acquisitions less likely when hedge funds acquire stakes of 5% or more and declare having no activism intentions (13G filings are mandatory in this case), consistent with our hypothesis that activism threats matter and affect behavior. We find no effect on divestitures and private acquisitions. When the same activist hedge funds later on switch from passive stake to declaring activist intentions (the interaction

[19] 13G fillings are similar to 13D fillings except that the filer acquiring the stake in the company is only a passive investor and does not intend to exert control. If these criteria are not met and the size of the stake exceeds 20 percent, form 13D must be filed.

term D[Post] $\times$ D[13G to 13D Switcher] captures these events), divestitures and merger become significantly more likely, and private acquisitions significantly less likely. These findings show that it is not just the selection of firms by hedge funds that explains the association between hedge fund exposure and acquisition behavior, dissipating substantially our concerns about endogeneity.

[Insert Table 3.4 Here]

3.4.3 Firm-level activism threats

Turning to the multivariate analysis of activism threats, we first investigate the impact of activism threat on firms asset market behavior using the company-specific threat measure. Since we focus on threat perceptions, we exclude activism events, i.e. for activism targets, we exclude the HFA event year and the three following years from our panel. We use two different measures of such threat levels that are idiosyncratic for each firm and may vary widely across industries. First, we use the predicted probability of becoming an activism target according to regression (1) in Table 3.2. Panel A of Table 3.5 shows the results for all three types of corporate transactions. In addition, we aggregate the two transaction types (mergers and divestitures) that correspond to corporate sales in regression (3), and separate between acquisitions of private targets and others in regression (5). Second, we use a dummy equal to 1 if the combined passive ownership by activist hedge funds is at least 5% for the firm in year t as the firm specific threat measure. Panel B of Table 3.5 shows the results, again for all three types of corporate transactions. We find in both cases highly significant results showing an increase in merger bids and divestitures, and a small decrease in acquisition frequencies for large firms but not for small ones.[20]

[Insert Table 3.5 Here]

[20] Our two firm-level threat measures are subject to endogeneity concerns, that is our findings might be attributable to selection effects of targets by activists. We address this issue in the next subsection.

3.4.4 Corporate transactions under industry-wide activism threats

We now consider our industry-level measures of activism threats that by construction take the same value for all firms in a given industry-year. We again exclude activism events. In order to control for industry shocks driving both the activism threat and changes in asset markets, we add the industry-level controls proposed by Harford [2005b], such as industry-year median absolute change of ROA, Sales Growth, Employee Growth, and Turnover (sales scaled by lagged book assets), as well as the full set of firm-level controls used in Tables 3.4 and 3.5.

Table 3.6 presents the results. In Panel A, we consider our main threat variable, Industry HFA Frequency. Industry HFA Frequency leads to a significant increase in divestitures and in sales (mergers and divestitures combined) ($p < 0.05$), but not in mergers. When we look at acquisitions, we again split the sample according to size (median split). We find that activism threat leads to a significant decrease in acquisitions and private acquisitions only for large firms ($p < 0.01$) as predicted, whereas for below-median firms in terms of firm value, there is a highly significant *positive* effect ($p < 0.01$) on acquisitions and private acquisitions. We return to this puzzling funding in Section 6.B.

[Insert Table 3.6 Here]

Panel B looks at our alternate measure of industry activism threats, Industry HFStake Frequency, indicating the proportion of firms experiencing a more than 5% increase in exposure (active and passive) to activist hedge funds. We find even stronger results, with divestitures and mergers increasing significantly ($p < 0.05$), and an even stronger reaction when we combine them to sales of assets ($p < 0.01$). Again only for large firms do we find a negative reaction of acquisitions following heightened hedge fund threats, whereas the sign is positive and significant for small firms.

Despite our extensive effort to control for all possible industry shocks and characteristics, unobserved industry characteristics may still bias our analysis. To address this concern, we use the instrument FIFB introduced in Section 3.3. FIFB is based on idiosyncratic large fund inflow shocks ($> 5\%$), and most activist hedge funds are general investors in their passive investments, i.e. they invest in a diversified cross-section of industries and tend to invest quickly and in a mechanical manner when experiencing large inflows Coval and Stafford [2007]. Therefore, it is reasonable to assume they will not allocate these inflows to industries according to unobserved industry shocks or trends that could be associated with corporate transactions activity. In Table 3.2, columns (6) to (7) show that the variable FIFB satisfies the relevance criterion, as it is strongly associated with Industry HFA Frequency. We then apply the reduced form 2SLS approach, using FIFB as instrument for Industry HFA Frequency, our main variable of interest.[21]

The results of our reduced form 2SLS approach are presented in Panel C of Table 3.6. Panel C shows that mergers, divestitures and sales become significantly more likely and acquisitions by large firms become less likely when using the FIFB instrument.

In conclusion, we find that firms under heightened activism threat divest more and are more frequently acquired. On average, they also make fewer acquisitions. These results extend findings by Gantchev et al. [2018] and Boyson et al. [2017] and show that firms under activism threat make similar changes in their behavior compared with target firms. There are, however, two important differences: first, the effect on merger bids is strong for target firms, and, probably unsurprisingly, weak for firms under threats. Concerning acquisitions, we find that the size difference observable for target firms (where only larger firms make fewer acquisitions), is exacerbated when firms are under activism threat: large firms make fewer acquisitions, whereas smaller firms make *more* acquisitions, but they do not necessarily pursue an (inorganic) growth strategy because at the same time they divest more.

[21] The 2SLS estimator gives us qualitatively similar results.

3.5 Activism and the Market for Corporate Assets

3.5.1 The combined impact of activism on real asset markets

Our next step is to gain some perspective on the relatively importance of the two channels of activism pressure, the direct target impact and threat impact. We analyze logit regressions that investigate the joint impact of the two channels on the asset market behavior of firms. The main difference to our previous analyses is that the two groups of treated firms are now analyzed jointly, whereas they were analyzed separately in Table 3.4 and Table 3.6. Results are presented in Table 3.7. D[Activist] and D[High HFA Threat] are the variables of interest for the two disjoint groups of treated firms, and they are mutually exclusive: D[Activist] is defined as in Table 3.4 and D[High HFA Threat] is a dummy variable that is equal to one for firms in the top quintile of Industry HFA Frequency (activist targets are again excluded); we use a dummy variable instead of the continuous variable to facilitate comparisons.

In Panel A of Table 3.7, we find that both the dummy for activism targets and the dummy for high HFA threat lead to more divestitures and more corporate sales (a variable that combines mergers and divestitures); when looking at merger bids we find a significant effect of D[Activist], but no significant effect for D[High HFA Threat]. Concerning acquisitions in Panel B, the regression confirms our earlier findings that only large firms under High HFA Threat acquire less, with a strong and significant effect ($p < 0.01$). Small firms under High HFA Threat make actually more acquisitions ($p < 0.01$).

[Insert Table 3.7 Here]

The most interesting insights of Table 3.7 can be gleaned from the model's estimate of conditional probabilities of corporate transactions and marginal effects. After estimating the logit model, we calculate conditional probabilities of transactions by fixing all other controls at the mean values of the treated group. We define the marginal effect as the estimated increase in the probability of a transaction when the HFA exposure dummy (either D[Activist] or D[High HFA Threat]) is switched from 0 to 1.[22] As reported in Panel A of Table 3.7, the probability of receiving merger bids for activism targets increases by 5.31%, and for firms under High HFA Threat it increases by 0.28%. Concerning corporate sales, activism targets are 7.44% more likely to sell corporate assets according to the marginal effect of activist, and firms under High HFA Threat are 0.81% more likely to sell assets. Concerning acquisitions in Panel B, large activism targets are 4.55% less likely to undertake acquisitions, and large firms under High HFA Threat undertake 2.16% less acquisitions.

We next compare the relatively importance of the two channels of activism pressure. Activism targets exhibit a much stronger reaction, but are less frequent compared with firms under HFA threat that show a weaker reaction but are more numerous. We focus on industries with high activism pressure, that is industry-years in the top quintile of Industry HFA Frequency over the entire sample. The mean value of Industry HFA Frequency in these industry-years is around 0.25, i.e. 25% of firms in these industries are currently or in the past two years activism targets; the remaining 75% of firms are firms entering our estimates of the effect of High HFA Threat. As a result, the overall impact is that a firm in an industry under high activism pressure will increase its annual frequency of selling an asset by $0.25 \times 7.44\% + 0.75 \times 0.81\% = 2.47\%$. Since the average annual frequency of corporate sales is 10.36%,[23], this means that corporate sales in industries under high activism pressure increase by $23.84\%(= 2.47/10.36)$. On the acquisition side, we need to distinguish between small and large firms since activism pressure affects them in opposite directions. For large firms (above median in size), the overall impact of high HFA pres-

[22]Since we have two different treated groups, HFA targets and firms with High HFA Threat, we estimate the probability of transactions conditional on HFA Targets by fixing D[Activist] = 1, D[High HFA Threat] = 0, D[Mid HFA Threat] = 0, and by fixing other controls at the mean of the target firm sample; we calculate the probability conditional on High HFA Threat by fixing D[Activist] = 0, D[High HFA Threat] = 1, D[Mid HFA Threat] = 0, and by fixing other controls at the mean value of the High HFA Threat sample.

[23]See Table 3.3: we add the average frequency for mergers of 5.17% (Panel A) and for divestitures of 5.19% (Panel B).

sure is equal to $(0.25 \times -4.55\% + 0.75 \times -2.16\%) = -2.76\%$ less acquisitions; for small firms, the overall increase in acquisitions is $(0.25 \times -0.40\% + 0.75 \times +1.50\%) = 1.03\%$. Thus, the overall activism pressure effect on acquisitions in top quintile industries will be a decrease by $-2.76\% + 1.03\% = -1.73\%$. In relation to an annual frequency of acquisitions of 15.06% for the entire sample (See Table 3.3, Panel C), this means that firms in high activism pressure industries decrease their frequency of acquisitions by $-1.76/15.06 = -11.69\%$ on average. We can also estimate the combined impact on the equilibrium in corporate asset markets under activism pressure: in these industries, firms on average undertake 23.84% more corporate sales and 11.69% less acquisitions, meaning that in the top quintile of affected industry-years, activism pressure creates an imbalance of more than 35% between the supply and the demand for corporate assets.

3.5.2 Activism and real asset liquidity

We now turn to an assessment of the impact of activism on the asset market equilibrium of affected industries. We begin by investigating the impact on the industry equilibrium in terms of transaction activity. Firms in the industry with heightened hedge fund pressure tend to sell more assets and simultaneously are less willing to buy assets, as estimated in last subsection, hence they are less likely to appear as liquidity providers in corporate asset markets in industries affected by activism pressure. Our hypothesis suggests, therefore, that industry outsiders, buyers that are not affected by the industry-specific activism pressure, should be a possible source of asset liquidity. These buyers are firms outside the affected industry and financial buyers (private buyers), but also to a lesser extent private buyers located in the industry itself.

Our measure of real asset liquidity (RAL) records the total number of transactions of industry assets in a given industry-year, that is the sum of *completed* merger bids, divestitures, and acquisitions, but counts each transaction only once, following Ortiz-Molina and Phillips [2014] and Schlingemann et al. [2002]. We look both at Frequency (number of deals scaled by number of firms in the industry) as well as at Transaction Value (sum of transaction value scaled by sum of market value of public firms).

How much of the imbalance in corporate asset markets created by hedge fund activism is absorbed by insiders, and how much by outsiders? Table 3.8 presents the results of industry-year regressions to answer this question. The main explanatory variable is D[Industry HFA Freq P80], a dummy that is equal to one if Industry HFA Frequency is in the top quintile of the entire industry-year sample. We require that each industry-year must have at least 3 public firms to be included in our regression analysis. We first investigate the overall impact on real asset liquidity: Does the frequency of industry assets transactions rise or decline in industries under heightened HFA pressure? The answer is not obvious since activism leads to a simultaneous shift in supply and demand (an increase in supply and less demand) for corporate assets, and we only observe transactions in which buyers and sellers can be matched. Panel A of Table 3.8 provides the answer. We find an increase in transaction activity (measured in transaction value) in the top quintile of Industry HFA Frequency, and no effect on transaction frequency, hinting there must be some elasticity in asset demand to absorb the increased supply.

[Insert Table 3.8 Here]

We try to disentangle the source of asset liquidity provision. We sort sellers and buyers of assets in insiders and outsiders according to their relationship to the industry in which the transaction takes place (i.e., industry of the corporate asset in each transaction): buyers and/or sellers are "insiders" if they are publicly listed firms with a primary SIC 3-digit code identical to that of the transaction;[24] only publicly listed firms can be "insiders" since only listed firms can be affected by HFA pressure. All other sellers and acquirers are considered as "outsiders". Outsiders consist of three main categories includes types of buyers or sellers: (*i*) listed firms in other industries or countries; (*ii*)

[24] There are discrepancies between Compustat's and SDC's SIC classifications at the 3-digit level, see Kahle and Walkling [1996] for a discussion. We give priority to Compustat classifications, but try to also include the information content in SDC classifications. We discuss our methodology of assigning industries in the case of discrepancies that affect our insider/outsider classification in Appendix B.

private firms; (*iii*) financial buyers, in particular private equity firms. The distinction tries to isolate as "insiders" the firms affected by hedge fund activism and activist threats in the corresponding industry.

In Panel B of Table 3.8, we distinguish only by status of asset buyers, that is between insider buyers and outsider buyers, but do not yet sort transactions by seller category. We calculate the RAL absorbed by inside buyers and outsider buyers respectively. Buyers are "insiders" in 8,279 out of total of 23,704 transactions. Consistent with our hypothesis, the results reveal that real asset liquidity provided by industry outsiders increases in top-quintile industries by activism pressure (2.519% increase measured in frequency and 1.616% increase measured in transaction value). By contrast, the real asset liquidity provided by industry insiders decreases, albeit not significantly so, as indicated by the negative coefficients in all regressions.

In Panel C of Table 3.8, we sort also by seller category. We run separate regressions for each possible pairing of seller and buyer according to their status as insiders and outsiders, that is, for the four possible buyer-seller pairings as, respectively, outsider-outsider, outsider-insider, insider-outsider, and insider-insider, we calculate the sub-sample RAL. Panel C shows that assets sold by insiders will significantly more frequently be acquired by outsiders when the industry is subject to severe activism pressure (columns (1) and (2)). By contrast, we find no such increase when we look at the liquidity provided by insiders, consistent with the idea that insiders are reluctant to buy when affected by the heightened HFA pressure (columns (3) and (4)). We also find a similar positive reaction when regressing the outsider buyer's ratio in the industry as shown in Panel D.

By contrast, when the seller is also an outsider, then there is no significant impact of the industry HFA exposure on the frequency of assets transaction by outsiders (columns (5) and (6)), by insiders (columns (7) and (8)).

To conclude, Table 3.8 provides evidence for a shift from insider buyers to outsider buyers when there is an increase in activism pressure, and confirms our hypothesis: as hedge fund pressure increases in an industry, inside real asset liquidity is drying up. As a consequence, acquirers from other industries will step in and provide some real asset liquidity.

3.5.3 Asset redeployability and private equity

In Table 3.9, we report the transaction-level regressions studying industry activism pressure, asset redeployability and type of outside buyers. Panel A of Table 3.9 shows that the dearth up of asset liquidity in industries with heightened activism pressure is mainly filled by one type of industry outsiders, private equity.[25] In Panel B, we present results interacting with Kim and Kung [2017]'s asset redeployability score that measures how many industries real assets of an industry are sold in secondary markets, using a median split. Panel B, Column (1) of Table 3.9 shows that outside provision of liquidity is stronger in industries under HFA pressure and with high asset redeployability. In Panel B, Column (2), we probe further and find that this effect can be entirely attributed to private equity buyers: they will only provide real asset liquidity in industries with high asset redeployability. As a result, the squeeze in real asset liquidity should be particularly severe in industries with low asset redeployability.[26] We find similarly significant results (not reported in tables) for alternative measures of liquidity or redeployability of industry assets, such as Gopalan, Kadan, and Pevzner (2012)'s weighted asset liquidity measure (WAL), asset tangibility, or the absence of knowledge or specific assets (proxied by R&D expenditure).

[Insert Table 3.9 Here]

[25] A possible alternative explanation is that activist hedge funds might select target industries with more potential private equity buyers. However, this kind of explanation is rejected by our results in Table 3.2, where we show PE transaction waves are irrelevant or even negatively correlated with Industry HFA Freq.

[26] Indeed, we find that the transaction price reacts and decreases more when industry with low asset redeployability score is under activism pressure. See the next subsection (Table 3.10, Panel B).

3.5.4 Price pressure

We also expect the squeeze in real asset liquidity to have an impact on deal pricing. We use the two measures for transactions price effects most frequently used in the literature, deal premiums and cumulative abnormal returns (CAR) around the deal announcement. We do not observe deal premiums in divestitures, and hence can only analyze cumulative abnormal returns in this case.

We use regressions to look at the seller CARs for the two of our three transaction samples, mergers and divestitures, that allow to observe seller price reactions. Our acquisition sample adds acquisitions of private targets, but the sellers of private acquisitions are not publicly listed, so we cannot observe seller CARs in this case. The variables of interest are again our two measures of industry level activism pressure, Industry HFA Frequency and Industry HFStake Frequency, both measured in the industry of the transaction (corporate asset). We include relevant transaction level controls that are known to affect seller announcement returns.[27] We look at the divestitures and mergers sample separately, using the standard event windows in each case. For divestitures, we look at a short and a longer symmetric event window around the deal announcement (CAR[-2, +2] and CAR[-5, +5]). For mergers, we look at a long pre-announcement window of three months to account for pre-deal price run-ups in the target stock price, as well as the price premium (mark-up of offer price relatively to stock price one month before).

Table 3.10 reports our findings for sellers in Panel A. We look at HFA targets and firms under HFA threats separately, which explain our use of the interaction of the variable of interest with the dummy D[Activism on Seller] and its complement, D[No Activism].[28]

[27] The transaction level controls are dummies for payment by stock, Ortiz-Molina and Philips'(2014) TotM&A_3yr (measured in the transaction industry), Institutional Ownership, Tobin's Q, ln(MV), Book Leverage, Dividend Yield, Cash Flow, Sales Growth, Asset Growth, R&D, and Excess Cash (accounting measures are seller's in Panel A and buyer's in Panel B). In regressions of the merger sample, we also include controls (dummies) for competing bids, successful bids, and unsolicited bids.

[28] D[Activism on Seller] is a dummy equal to one if activists launch a campaign against the seller in the two calendar years prior to the merger or divestiture. D[No Activism] is its complement.

We find a significant and robust negative effect for our transactions under high industry activism pressure but the seller recently is not under the HFA campaign (Industry HFA Freq $\times$ D[No Activism] = 1) in all regressions with a level of significance of at least 5%. For divestitures, we find effects that are slightly stronger for the longer window. For mergers, we find consistently negative results (significance increase to 1% in the case of deal premiums). The effects are somewhat weaker for Industry HFStake Frequency. We find similar results for shorter run-up periods or symmetric CAR windows (not reported in Table 3.10).

By contrast, for the sample of activism targets (D[Activism on Seller] = 1), we find no significant effect of the industry activism pressure, in any of our eight regressions. This means that activists appear to succeed in isolating target firms from the adverse price pressure effect that afflict firms in industry with high exposure to activism.

Panel B shows that the negative price pressure effect is clearly much more pronounced in industries with low asset redeployability. This finding complements our result in the previous section that outsider buyers, and in particular private equity, provide real asset liquidity only in industries with highly redeployable or liquid assets (Table 3.10). Consequently, the price pressure effect is essentially driven by low asset liquidity industries in which private equity does not act as liquidity provider.

[Insert Table 3.10 Here]

In Panel C, we look at the price pressure effects on buyers, using the same samples of divestitures and mergers and regressions. The sample size shrinks because only about half of the transactions are bought by listed acquirers. We find the expected positive effect for top-quintile industries in terms of activism pressure, but the effect is rather weak since it is only statistically significant in three out of eight regressions. For the sample of HFA target firms, we find similar weak effects, significant in two cases. For buyer returns, we find similar results when the sellers is an activist target or acting under activism threat.

Overall, our analysis of deal pricing yields a picture that is consistent with our hypothesis and our previous analysis of asset liquidity: as supply of corporate assets in affected industries increases and demand decreases, asset liquidity is affected. This leads to lower seller returns and also to (weakly) higher buyer returns. Weak price reactions are to be expected since, as Table 3.8 shows, outsiders step up and provide real asset liquidity and potentially mitigate the squeeze in asset prices.

3.6 Activism and the Efficiency of Corporate Transactions

3.6.1 Evidence on post-transaction performance: asset sellers

Our final exploration addresses the question whether the involvement of activists in the corporate asset market leads to more efficient transactions. We first consider possible efficiency gains of asset sellers. We cannot analyze mergers because we cannot construct a satisfactory counterfactual allowing us to observe an independent time series of seller performance after the transaction, and we do not consider private acquisitions for the same reason (seller performance cannot be observed). Thus, we limit this analysis to divestitures, and to the long-run performance of the seller.

It is well-known that activism campaigns lead to long-run positive effects in stock market and accounting performance for seller firms (see Bebchuk et al. [2015a]). Thus, it is important to disentangle the long-run performance enhancing effect of activism campaign from that of activism divestitures. Gantchev et al. [2018] document the positive long-run stock market performance of seller firms in corporate activism divestitures, but do not address the likely overlap with the long-run performance-enhancing effect of the post-activism period.

We report our findings in Table 3.11. We consider three different long-run performance measures, each for a period of two years after the divestiture event, to provide a cross-section of accounting-based and stock market based performance measures: Tobin's Q; ROA; and the ratio of Sales/Assets (Turnover) that is correlated with economic efficiency gains. Column (1) shows a positive effect on seller's Tobin's Q after divestitures (dummy D[Post Divestiture]),and after activist campaigns, the latter consistent with findings by Gantchev et al. [2018]. The key variable of interest is the interaction term D[Post Divestiture] $\times$ D[Activism Divestiture]. This variable shows a positive value effect over two years over and above the positive effect of having done divestitures, and having gone through an activism campaign. We find a positive and significant ($p < 0.05$) response to the interaction dummy D[Post Divestiture] $\times$ D[Activism Divestiture]), for both Tobin's Q and for ROA. Only the sales/assets ratio does not show a significant long-run performance effect.

[Insert Table 3.11 Here]

Panel B repeats the analysis but looks at firms with elevated HFA threat (we look at firms in the top quintile of industry-years by of Industry HFA Frequency). We do not find an analogous performance-enhancing effect for activism divestitures when done under HFA threat: the intersection term D[Post Divestiture] $\times$ D[High HFA Threat] does not show any sign of a significant difference for any of our three performance variables. Thus, it appears that divestitures done under the menace of HFA threats do not show any indication of a long-run efficiency gains captured by sellers, whereas columns (1) and (2) in Panel A show significant differences for activism divestitures. When it comes to long-run performance, there appears to be a clear difference between activism divestitures and divestitures done under elevated HFA threat: the magic of efficiency gains is limited to corporate sales of activism targets, and does not spread to other transactions in industries under activism pressure.

3.6.2 Post-transaction performance: asset buyers and the role of small firms

We finally analyze the long-run performance effect on the buyer side for acquisitions. A particular motivation for this investigation is the question whether our data can provide a possible explanation to the puzzling observation that small firms, when acting under heightened HFA threat, appear to increase the frequency of acquisitions rather than decrease it, as large firms do and as activism targets do. Specifically, we ask: is there any hint that small firms under HFA threat make acquisitions as a restructuring tool (which might help to fend off activists)? We look for incomplete evidence consistent with such a possible explanation, by looking at the long-run performance effect of small firms that have undertaken an activism acquisitions of private targets.

Table 3.12 presents the findings. We are looking at the long-run performance effect for buyers of firms or assets. We find a strong performance-enhancing effect ($p < 0.05$) for two out of three measures of long-run performance, ROA and Sales/Assets for activism acquisitions of small firms, captured by the triple interaction term D[Post Acquisition] $\times$ D[Activism Acquisition] $\times$ D[Small], but not for the third variable, Tobin's Q. We do not find any comparable significant effect for large firms (not reported in tables).

Panel B repeats the same test for firms in industries in the top quintile in terms of activism threat. The triple interaction term [Post Acquisition] $\times$ D[Activism Acquisition] $\times$ D[Small] is positive, albeit not significant. We find a significant reaction for ROA and for Sales/Assets when we expand the subsample to the top tercile of firms under activism threat (not reported).

Measured by long-run efficiency, small firms seem to do well when undertaken acquisitions under HFA pressure. Similar to divestitures, the gains are stronger for target firms than for firms acting under HFA threats. These gains are in addition to the strong positive long-run gain that can be attributed to their smaller size. Overall, these findings are consistent with the earlier observation (Table 3.6) that only large firms react to an increase in HFA threats with a reduction in their acquisition activity.

[Insert Table 3.12 Here]

3.7 Conclusions

The paper explores the impact of hedge fund activism on corporate asset markets. We find that activist target firms are more likely to receive merger bids, and make more divestitures and fewer acquisitions, in line with earlier studies. We consider a second channel of activism pressure, the disciplining effect on firms exposed to activism threats. We propose measures of activism threats at the firm level and at the industry level, and find that firms exposed to such threats change their behavior in similar ways, but with subtle differences: they divest more, but are only marginally more likely to be sold. Only large firms under threat reduce their acquisition activity, whereas small firms expand it.

Comparing these two parallel channels of hedge fund pressure, we find that they contribute about equally to the change in deal activity in highly affected industries exposed, with activism threats being more important for acquisitions, and targets more important for corporate sales. We consider the impact on real asset liquidity: when firms in affected industries want to simultaneously sell more and buy less assets, then real asset liquidity shrinks by up to 35%, creating a role for outside liquidity providers. We find that acquirers from outside the affected industry - private equity funds and listed firms in other industries - provide liquidity, and more so in industries with high asset redeployability.

We find evidence that the squeeze on real asset liquidity also affects transaction prices: seller announcement returns are smaller in corporate sales when industries are affected by activist pressure (merger bids and divestiture bids), and buyer announcement returns are (weakly) larger in this case. The effect is stronger in industries with low redeployability. However, we find that divestitures done by activist targets resist the price pressure remarkably well.

Finally, we consider whether activist pressure leads to more efficient transactions. Isolating the incremental effect of transactions done under activism influence, we find positive long-run performance effects when corporate transactions are undertaken by activism targets; we do not find a similar effect for transactions undertaken under activism threat. Thus, the direct involvement of hedge fund activists seems necessary to create additional efficiency gains.

Our paper shows that activism creates important market externalities for firms not directly targeted, by changing the environment and behavior in acquisition markets. It is not clear that these changes are efficient, but at least small firms disciplined by activism threats seem to make better acquisitions. Our findings lead to new questions that go beyond the scope of this paper, for example whether activists reduce or magnify the cyclicality of real asset markets.

3.8 Tables

Table 3.1: Hedge fund activism and characteristics of firms under HFA impact

This table reports annual frequencies of HFA events (Panel A), summary statistics of industry HFA threat variables (Panel B), and characteristics of firms under HFA impact (Panels C and D). Panel A reports the annual number of firms and of HFA campaigns in the CRSP-Compustat universe and of campaigns in industry HFA clusters. Ind. HFA clusters is defined as a certain industry-year with at least 2 activist campaigns take place in the past 3 years and fraction of firms targeted within last 3 years is in the top quintile of the industry-year sample. Panel B presents the summary statistics of three industry HFA threat variables. Industry HFA Freq is defined as the fraction of firms in industry j and year t that have been targeted by activist hedge funds in the previous three years. Industry HFStake Freq is defined as the fraction of firms in industry j and year t that had experienced at least one activist hedge funds' stake jump within the previous three years. The third measure FIFB, constructed following Gantchev, Gredil, and Jotikasthira (2017), hypothetically assigns the fund inflow shock of activist hedge fund k to industry j and in year t according to industry weight of j in k's portfolio in year t-1. Panel C reports characteristics of firms in the year in which they are targeted by activist hedge funds (HFA Target Firms). Variables are measured in the year prior to the HFA event. The Remaining Sample is the CRSP-Compustat universe excluding the HFA Target Firms sample. We report the differences in mean and median values between the target and non-target sample of firm-years, and conduct t tests for differences in means and Wilcoxon tests for differences in medians (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Panel D reports firm characteristics sorted by terciles of Industry HFA Freq. Panels B and D exclude firm-year observations of firms that are HFA targets in year t for observations of years $[t, t+3]$.

Panel A: Frequency of HFA campaigns and industry HFA clustering

Calendar year	(1) Number of firms (all)	(2) Number of HFA campaigns	(3) Proportion of firms targeted by HFA	(4) Number of HFA campaigns in Ind. HFA clusters	(5) Fraction of industries with Ind. HFA clusters
1994	6,176	12	0.19%	1	0.00%
1995	6,372	33	0.52%	2	0.00%
1996	6,850	90	1.31%	19	1.11%
1997	6,847	170	2.48%	32	3.31%
1998	6,408	131	2.04%	26	4.04%
1999	6,226	90	1.45%	27	3.68%
2000	5,986	86	1.44%	20	3.72%
2001	5,296	79	1.49%	16	4.89%
2002	4,911	121	2.46%	27	6.04%
2003	4,635	118	2.55%	39	4.96%
2004	5,066	128	2.53%	63	6.51%
2005	4,977	211	4.24%	129	12.17%
2006	4,888	273	5.59%	186	18.11%
2007	4,758	319	6.70%	216	24.24%
2008	4,487	256	5.71%	170	25.48%
2009	4,252	134	3.15%	77	21.84%
2010	4,125	149	3.61%	67	13.90%
2011	4,002	172	4.30%	107	11.24%
2012	3,940	174	4.42%	105	14.01%
2013	4,001	197	4.92%	132	16.53%
2014	4,152	236	5.68%	163	19.11%
2015	4,103	203	4.95%	120	22.67%
2016	3,990	169	4.24%	85	19.84%
Total	116,448	3,551	3.05%	1,829	11.02%

Panel B: Summary statistics of industry HFA threat variables (Firm-year sample)

Industrial HFA Threat Variable	Mean	Min	P25	Median	P75	Max	S.D.
Industry HFA Freq	0.060	0.000	0.000	0.037	0.087	0.857	0.070
Industry HFStake Freq	0.102	0.000	0.012	0.077	0.157	1.000	0.107
FIFB (Fund Inflow / Ind Market Cap)†	0.005	0.000	0.001	0.002	0.005	13.549	0.064

†: Since FIFB is highly skewed, we use the percentile rank of FIFB throughout the whole paper.

Panel C: Characteristics of activism target firms

	HFA Target Firms (N = 3,551)			The Remaining Sample (N = 112,897)			Difference Targets - Non-targets	
	Mean	Median	S.D.	Mean	Median	S.D.	Mean	Median
Institutional Ownership	0.512	0.527	0.288	0.427	0.403	0.296	0.086***	0.124***
Tobin's Q	1.655	1.286	1.153	1.988	1.401	1.706	-0.333***	-0.115***
ln(MV)	5.499	5.314	1.821	5.626	5.599	2.026	-0.127***	-0.285***
Book Leverage	0.333	0.282	0.318	0.329	0.293	0.296	0.003	-0.011
Excess Cash	0.037	0.000	0.178	0.035	0.000	0.174	0.002	0.000
Dividend Yield	0.010	0.000	0.024	0.014	0.000	0.026	-0.004***	0.000***
Cash Flow	0.010	0.049	0.191	0.026	0.066	0.206	-0.016***	-0.017***
ROA	0.053	0.081	0.186	0.073	0.100	0.203	-0.019***	-0.019***
Sales Growth	0.106	0.044	0.389	0.160	0.081	0.441	-0.055***	-0.037***
Sales/Assets(lag)	0.984	0.831	0.781	1.016	0.844	0.872	-0.032**	-0.013
Assets Growth	0.082	0.022	0.359	0.139	0.060	0.386	-0.056***	-0.038***
R&D	0.045	0.000	0.089	0.045	0.000	0.099	0.000	0.000
HHI	0.193	0.137	0.166	0.182	0.127	0.164	0.011***	0.010***
CAR [12 months]	-0.056	-0.073	0.542	0.049	0.011	0.597	-0.105***	-0.084***
TotM&A_3yr	0.075	0.043	0.097	0.078	0.043	0.096	-0.003*	0.000

Panel D: Characteristics of firms under high, medium and low threat (Industry HFA Freq)

Tercile of Industry HFA Freq	Bottom Tercile (N = 42,908)			Medium Tercile (N = 31,552)			Top Tercile (N = 32,729)		
	Mean	Median	S.D.	Mean	Median	S.D.	Mean	Median	S.D.
Institution Ownership	0.416	0.394	0.288	0.419	0.387	0.296	0.430	0.407	0.303
Tobin's Q	1.757	1.266	1.448	2.278	1.544	2.091	2.028	1.490	1.574
ln(MV)	5.716	5.732	2.043	5.609	5.568	2.004	5.564	5.522	2.056
Book Leverage	0.379	0.377	0.285	0.279	0.203	0.291	0.316	0.268	0.300
Excess Cash	0.034	0.000	0.145	0.033	0.000	0.199	0.038	0.000	0.180
Dividend Yield	0.018	0.006	0.028	0.012	0.000	0.026	0.010	0.000	0.021
Cash Flow	0.048	0.065	0.167	0.000	0.061	0.245	0.033	0.075	0.202
ROA	0.093	0.100	0.166	0.044	0.092	0.241	0.083	0.112	0.199
Sales Growth	0.151	0.078	0.402	0.185	0.092	0.499	0.163	0.087	0.430
Sales/Assets(lag)	0.995	0.793	0.930	0.944	0.778	0.811	1.121	0.955	0.869
Assets Growth	0.140	0.064	0.359	0.155	0.065	0.421	0.136	0.061	0.380
R&D	0.023	0.000	0.072	0.073	0.008	0.122	0.044	0.000	0.092
HHI	0.225	0.154	0.208	0.129	0.100	0.091	0.181	0.133	0.141
CAR [12 months]	0.027	0.005	0.529	0.088	0.031	0.661	0.038	0.000	0.591
TotM&A_3yr	0.064	0.028	0.094	0.086	0.062	0.086	0.084	0.048	0.104

This table reports the relationship between industry measures of activism threat and the HFA target probability. Columns (1) – (7) report logit regressions for our firm-year sample. The left-hand side variable D[HFA] is a dummy that is equal to one if activists initiate a new campaign against the firm in year t. We use 3 variables to measure industry HFA threat. Industry HFA Freq is defined as fraction of firms in industry j and year t that have been targeted by activist hedge funds within last three years. Industry HFStake Freq is defined as the fraction of firms in industry j and year t that had experienced at least one activist hedge funds' stake jump within last 3 years. The last one, FIFB, hypothetically assigns the fund inflow shock of activist hedge fund k to industry j and in year t according to industry weight of j in k's portfolio in year t-1. Columns (8) – (9) report OLS regressions for the industry-year sample; in this case all controls are industry-year medians. In above regressions, all firm-level control variables are one year lagged except for industry threat measures, TotM&A_3yr, TotPE_3yr, and D[Merger Wave]. All regressions include year and industry fixed effects. Standard errors are clustered at the firm level in columns (1) - (7) and at the industry level in columns (8) – (9) (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 3.2: Industry activism threat and HFA target probability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Firm-year regression							Industry-year regression	
	Logit D[HFA]	Logit D[HFA]	Logit D[HFA]	Logit D[HFA]	Logit D[HFA]	Logit D[HFA]	Logit D[HFA]	OLS Industry HFA Freq (year t)	OLS Industry HFA Freq (year t)
Industry HFA Freq		7.752***			7.753***				
		(0.304)			(0.305)				
Industry HFStake Freq			1.825***			1.825***			
			(0.220)			(0.220)			
FIFB (Percentile Rank)				0.366***			0.366***	0.0149***	0.0151***
				(0.140)			(0.140)	(0.00570)	(0.00570)
D(Merger Wave)					0.0173	-0.0166	-0.0417		-0.00485
					(0.0839)	(0.0860)	(0.0896)		(0.00486)
TotM&A_3yr	0.472	0.164	0.436	0.458	0.157	0.442	0.473	0.0199	0.0207
	(0.381)	(0.401)	(0.381)	(0.389)	(0.400)	(0.380)	(0.389)	(0.0179)	(0.0179)
TotPE_3yr	0.0721	-0.00634	-0.155	0.0841	0.00598	-0.163	0.0598	-0.0629**	-0.0639**
	(0.660)	(0.764)	(0.663)	(0.696)	(0.763)	(0.662)	(0.696)	(0.0306)	(0.0306)
Institutional Ownership	1.459***	1.415***	1.419***	1.466***	1.416***	1.419***	1.465***	0.0178	0.0171
	(0.116)	(0.118)	(0.116)	(0.117)	(0.118)	(0.116)	(0.118)	(0.0124)	(0.0125)
Tobin's Q	-0.320***	-0.312***	-0.320***	-0.321***	-0.311***	-0.321***	-0.321***	-0.00676*	-0.00690**
	(0.0353)	(0.0355)	(0.0354)	(0.0358)	(0.0356)	(0.0355)	(0.0359)	(0.00346)	(0.00347)
ln(MV)	-0.200***	-0.194***	-0.196***	-0.200***	-0.194***	-0.196***	-0.199***	-0.00329	-0.00320
	(0.0208)	(0.0210)	(0.0208)	(0.0211)	(0.0210)	(0.0208)	(0.0211)	(0.00216)	(0.00216)
Book Leverage	0.325***	0.342***	0.330***	0.316***	0.342***	0.330***	0.316***	0.00796	0.00821

	(0.0920)	(0.0942)	(0.0919)	(0.0935)	(0.0942)	(0.0919)	(0.0935)	(0.0115)	(0.0115)
Dividend Yield	-4.046***	-4.093***	-4.014***	-3.753**	-4.091***	-4.015***	-3.757**	-0.383***	-0.379***
	(1.479)	(1.508)	(1.476)	(1.484)	(1.508)	(1.475)	(1.483)	(0.143)	(0.143)
Cash Flow	-0.285	-0.318*	-0.261	-0.303*	-0.317*	-0.262	-0.305*	-0.0226	-0.0225
	(0.177)	(0.181)	(0.177)	(0.179)	(0.181)	(0.177)	(0.179)	(0.0291)	(0.0291)
Sales Growth	-0.0642	-0.0548	-0.0537	-0.0700	-0.0552	-0.0533	-0.0690	-0.0108	-0.0105
	(0.0689)	(0.0677)	(0.0684)	(0.0698)	(0.0677)	(0.0683)	(0.0697)	(0.0121)	(0.0121)
Asset Growth	-0.176*	-0.135	-0.167*	-0.190**	-0.135	-0.167*	-0.191**	-0.0359**	-0.0361**
	(0.0907)	(0.0904)	(0.0904)	(0.0926)	(0.0904)	(0.0904)	(0.0926)	(0.0146)	(0.0146)
R&D	0.516	0.453	0.520	0.519	0.451	0.522	0.525	-0.308*	-0.301*
	(0.380)	(0.381)	(0.379)	(0.382)	(0.382)	(0.380)	(0.382)	(0.171)	(0.171)
HHI	-0.388	-0.842***	-0.313	-0.476	-0.843***	-0.311	-0.470	0.0550**	0.0547**
	(0.278)	(0.316)	(0.280)	(0.291)	(0.316)	(0.280)	(0.291)	(0.0261)	(0.0261)
Excess Cash	0.620***	0.649***	0.613***	0.631***	0.648***	0.614***	0.633***	0.0580**	0.0586**
	(0.156)	(0.157)	(0.156)	(0.157)	(0.158)	(0.156)	(0.157)	(0.0283)	(0.0283)
CAR [12 months]	-0.125***	-0.116**	-0.124***	-0.113**	-0.116**	-0.124***	-0.113**	-0.00280	-0.00281
	(0.0479)	(0.0489)	(0.0478)	(0.0485)	(0.0489)	(0.0479)	(0.0485)	(0.00552)	(0.00552)
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	68228	68228	68228	65934	68228	68228	65934	4517	4517
pseudo R^2 / adj. R^2	0.086	0.129	0.089	0.087	0.129	0.089	0.087	0.071	0.071

Table 3.3: Descriptive statistics of corporate transactions by period

This table reports descriptive statistics of corporate transaction activities by period. We report the number and annual frequencies of each type of transaction. In Panel A, we report merger bids received by CRSP-Compustat firms. In Panel B, we report divestitures in which CRSP-Compustat firms are sellers of the divested assets. Panel C reports acquisitions of public, private and subsidiary firms by CRSP-Compustat firms. Panel D reports acquisitions of private target firms only by CRSP-Compustat firms. An activism transaction (activism merger in Panel A, activism divestiture in Panel B, activism acquisition in Panels C and D) is defined as a transaction by a company targeted by activist hedge funds in the 2 years (730 days) prior to the transaction (column (3) of each panel). Column (4) of each panel is defined as the number of firms with activism transactions divided by the total number of firms that have been targeted by activists in the past 2 years. In columns (5)– (7) of each panel, we report the number of transactions sorted by industry HFA threat. Firms with high (low) HFA threat are defined as firms not targeted by activist hedge funds but with an Industry HFA Frequency measure in the top (bottom) tercile of that year.

Panel A: HFA campaigns and merger bids

Calendar year	(1) Number of merger bids	(2) % of firms with merger bids	(3) Number of activism mergers	(4) % of firms with activism mergers	(5) Number of merger bids under high HFA threat	(6) % of firms with mergers under high HFA threat	(7) % of firms with mergers under low HFA threat
1994 – 1995	378	2.92%	0	0.00%	107	2.82%	2.78%
1996 – 2000	2,209	6.58%	91	10.17%	641	6.21%	6.65%
2001 – 2005	1,192	4.62%	98	10.89%	417	5.34%	3.45%
2006 – 2010	1,317	5.58%	227	11.70%	405	5.68%	4.04%
2011 – 2016	1,137	4.57%	216	11.78%	372	4.79%	3.65%
Total	6,233	5.17%	632	10.19%	1,942	5.38%	4.34%

Panel B: HFA campaigns and divestitures

Calendar year	(1) Number of divestiture	(2) % of firms with divestiture	(3) Number of activism divestiture	(4) % of firms with activism divestiture	(5) Number of divestiture under high HFA threat	(6) % of firms with divestiture under high HFA threat	(7) % of firms with divestiture under low HFA threat
1994 – 1995	612	3.89%	3	5.26%	93	2.94%	3.96%
1996 – 2000	2,200	5.23%	63	6.25%	493	6.00%	5.42%
2001 – 2005	1,764	5.29%	98	7.84%	445	6.81%	5.26%
2006 – 2010	1,535	5.39%	185	7.51%	337	4.79%	5.33%
2011 – 2016	1,741	5.52%	225	8.60%	361	5.19%	5.62%
Total	7,852	5.19%	574	7.81%	1,729	5.16%	4.58%

Panel C: HFA campaigns and acquisitions of public, private, and subsidiary firms

Calendar year	(1) Number of acquisitions	(2) % of firms with acquisitions	(3) Number of activism acquisitions	(4) % of firms with activism acquisitions	(5) Number of acquisitions under high HFA threat	(6) % of firms with acquisitions under high HFA threat	(7) % of firms with acquisitions under low HFA threat
1994 – 1995	2,036	11.53%	4	5.26%	319	10.17%	12.09%
1996 – 2000	8,464	16.86%	238	16.54%	1,418	14.87%	17.21%
2001 – 2005	4,969	14.66%	117	10.01%	1,080	14.67%	15.51%
2006 – 2010	4,280	14.16%	214	9.49%	950	14.58%	15.60%
2011 – 2016	5,133	15.65%	265	12.00%	1,102	15.23%	17.91%
Total	24,882	15.06%	838	11.82%	4,869	14.51%	15.72%

Panel D: HFA campaigns and acquisitions of private firms

Calendar year	(1) Number of private acquisitions	(2) % of firms with private acquisitions	(3) Number of activism private acquisitions	(4) % of firms with activism private acquisitions	(5) Number of private acquisitions under high HFA threat	(6) % of firms with private acquisitions under high HFA threat	(7) % of firms with private acquisitions under low HFA threat
1994 – 1995	794	5.10%	3	2.63%	131	4.12%	5.26%
1996 – 2000	3,989	9.00%	152	6.38%	733	7.64%	8.71%
2001 – 2005	2,154	7.16%	73	4.01%	529	7.25%	7.29%
2006 – 2010	2,043	7.42%	113	4.82%	530	8.09%	7.89%
2011 – 2016	2,417	7.97%	140	5.82%	588	8.08%	9.06%
Total	11,397	7.68%	481	5.49%	2,511	7.50%	7.71%

Table 3.4: Hedge fund activism and corporate transactions

This table presents regressions investigating corporate transaction activities of activism target firms. Panel A studies the probability of receiving a merger bid following an HFA event, Panel B studies the probability of divestiture, and Panel C investigates the probability of acquisitions of public and private firms. Panel D documents the probability of mergers, divestitures, sales and acquisitions following filing switches from 13G-to-13D filings. Panel A to Panel C present logit regressions, and Panel D OLS regressions. In each panel, the left-hand side variable is a dummy that takes the value one if the firm undertakes a transaction receives in year t (a merger bid in Panel A, divestiture in Panel B, etc.) The main explanatory variable D[Activist] is an indicator variable tracking whether the firm is an HFA campaign target in the 2 years prior to each type of transaction (a transaction event is a merger bid in Panel A, a divestiture in Panel B, etc.); D[Activist] is equal to one in year t if activists launch a campaign against the firm during the 730 calendar days prior to the transaction event, or, if there is no transaction event for the firm in year t, during the 730 calendar days prior to the median date of all transaction events of other firms in year t. All panels include the following firm-level control variables: TotM&A_3yr, Institutional Ownership, Tobin's Q, ln(MV), Book Leverage, Dividend Yield, Cash Flow, Sales Growth, Asset Growth, R&D, Excess Cash, HHI, CAR[Year t-1], and D[Divestiture][t-1] (D[Divestiture][t-1] only in Panel B). All firm-level controls are one-year lagged. In Panel C, D[Large] (D[Small]) is a dummy equal to 1 if the firm's size is larger (smaller) than the industry-year median size of firms in year t-1.

In Panel D, we merge the data of 13G fillings and 13G-to-13D switchers with the CRSP-Compustat universe. The dataset includes 4,488 13G filings and 227 13G-to-13D switchers. The regression sample includes firm-year observations from 5 years prior to and 5 years post the 13G filling or 13D switcher filling. Following Brav, Jiang, Ma, and Tian (2016)'s setting, we apply the following difference in difference specification:

$$y_{i,t} = \alpha_t + \delta_j + \beta_1 D[Post] + \beta_2 D[Post] \times D[13G\ to\ 13D\ Switcher] + \beta_3 D[13G\ to\ 13D\ Switcher] + \gamma Control_{i,t} + \varepsilon_{i,t}$$

where $D[Post]$ is a dummy variable equal to 1 if the firm-year observation is within $[t+1, t+5]$ years post the event year. The event year is the year of the filing of Schedule 13G for non-switchers or the year of the switch for the switcher sub-sample. $D[13G\ to\ 13D\ Switcher]$ is a dummy variable equal to one if there is a 13-G to-13D switch for a firm during the event year (as opposed to remaining with Schedule 13G status). Sale is a dummy that is equal to one of there is a merger bid or a divestiture. Definitions of all other variables can be found in Appendix A. Industry fixed effects and year fixed effects are always included in each panel. Standard errors are clustered at the firm level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Activism targets and mergers

	(1) LOGIT Merger bids	(2) LOGIT Merger bids Strategic buyer	(3) LOGIT Merger bids Financial buyer	(4) LOGIT Merger bids Unsolicited bids
D[Activist]	0.710***	0.611***	0.858***	1.206***
	(0.0550)	(0.0620)	(0.103)	(0.160)
Firm-level control variables	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes
N	71879	71534	66332	51167
pseudo R^2	0.051	0.049	0.107	0.088
Unconditional prob.	5.45%	4.43%	0.79%	0.33%
Prob. conditional on HFA targets	10.49%	7.86%	1.86%	1.10%

Panel B: Activism targets and divestitures

	(1) LOGIT Divestiture	(2) LOGIT Divestiture	(3) LOGIT Divestiture Strategic buyer	(4) LOGIT Divestiture Financial buyer	(5) LOGIT Divestiture Core assets	(6) LOGIT Divestiture Unrelated assets
D[Activist]	0.362^{***}	0.259^{***}	0.332^{***}	0.461^{***}	0.294^{***}	0.414^{***}
	(0.0694)	(0.0746)	(0.0762)	(0.139)	(0.0967)	(0.0921)
D[Activist's Goal is Restructure]		0.748^{***}				
		(0.191)				
Firm-level control variables	Yes	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
N	68772	68772	68471	61622	64434	67666
pseudo R^2	0.182	0.183	0.176	0.192	0.169	0.194
Unconditional prob.	4.57%	–	4.34%	0.36%	1.44%	2.99%
Prob. conditional on HFA targets	6.44%	11.63%†	5.95%	0.58%	1.93%	4.46%

†: The probability is conditional on activist's goal to restructure the target firm.

Panel C: Activism targets and acquisitions of public and private firms

	(1) LOGIT Acquisition	(2) LOGIT Acquisition	(3) LOGIT Acquire Private firms	(4) LOGIT Acquire Private firms	(5) LOGIT Acquire Public firms	(6) LOGIT Acquisition Related	(7) LOGIT Acquisition Unrelated
D[Activist]	-0.210^{***}		-0.335^{***}		-0.156	-0.187^{**}	-0.152^{**}
	(0.0584)		(0.0839)		(0.122)	(0.0808)	(0.0756)
D[Activist] $\times$ D[Large]		-0.252^{***}		-0.387^{***}			
		(0.0793)		(0.119)			
D[Activist] $\times$ D[Small]		-0.0642		-0.208^{*}			
		(0.0865)		(0.126)			
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	69541	66346	69118	66069	67308	68664	69148
pseudo R^2	0.124	0.125	0.102	0.104	0.134	0.129	0.126
Unconditional prob.	14.42%	–	6.40%	–	3.65%	6.46%	7.33%
Prob. conditional on HFA targets	12.02%	–	4.66%	–	2.51%	5.41%	6.36%

Panel D: Activists' switch in filing status from 13G to 13D

	(1) OLS Merger	(2) OLS Divestiture	(3) OLS Sale	(4) OLS Acquisition Public	(5) OLS Acquisition Private
D[Post]	0.0579***	-0.00379	0.0504***	-0.0197**	-0.00799
	(0.00403)	(0.00525)	(0.00630)	(0.00773)	(0.00583)
D[Post] × D[13G to 13D Switcher]	0.0383**	0.0294**	0.0614***	-0.0179	-0.0207**
	(0.0167)	(0.0128)	(0.0191)	(0.0145)	(0.0100)
Firm-level control variables	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes
N	15933	15144	15933	15144	15144
adj. R^2	0.035	0.065	0.052	0.075	0.040

Table 3.5: Firm-level HFA threat and corporate transaction

This table provides evidence on the relationship between firm-level threats of hedge fund activism and asset transaction activities of firms not (yet) targeted by activists. The dependent variable is a dummy that is equal to one if a transaction of the designated type occurs in year t; Sale is equal to one if a merger or a divestiture occurs in year t. If a firm is targeted by an activist hedge fund in year t, we exclude for that firm years $[t, t+3]$ from the sample to eliminate the direct activism target impact. In Panel A, we use $Pr(Target)$ to measure the firm-level activism threat, where $Pr(Target)$ is the estimated probability of being targeted by an activist hedge fund. To obtain this measure, we first run a logit regression as in column 1 of Table **??**. We use the post estimation probability as $Pr(Target)$. In Panel B and C, we use D[Passive Stake] to measure the activism threat, where D[Passive Stake] is a dummy equal to 1 if the combined ownership by activist hedge funds is at least 5% in year t. All panels include the following firm-level control variables: Institutional Ownership, Tobin's Q, ln(MV), Book Leverage, Dividend Yield, Cash Flow, Sales Growth, Asset Growth, R&D, Excess Cash, HHI, CAR[Year t-1], and D[Divestiture][t-1] (D[Divestiture][t-1] only used in regression of divestiture). All firm controls are one year lagged. D[Large] (D[Small]) is a dummy equal to 1 if the firm's size is larger (smaller) than the industry-year median size of firms (all measured in year $t-1$). Industry fixed effects and year fixed effects are included in all regressions. Standard errors are clustered at the firm level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Corporate transactions and firm-level HFA threat: Pr(Target)

	(1) Merger	(2) Divestiture	(3) Sale	(4) Acquisition	(5) Acquire Private
$\widehat{Pr(Target)}$	0.622***	0.536***	1.149***		
	(0.164)	(0.140)	(0.215)		
$\widehat{Pr(Target)}$ × D[Small]				-1.108***	-0.294***
				(0.129)	(0.0955)
$\widehat{Pr(Target)}$ × D[Large]				-2.011***	-0.527***
				(0.183)	(0.141)
Firm-level control variables	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes
N	65429	62934	65429	60601	60601
adj. R^2	0.018	0.073	0.045	0.079	0.042

Panel B: Passive stake and HFA target probability

	(1) D[HFA]	(2) D[HFA]	(3) D[HFA]	(4) D[HFA]
D[Passive Stake]	1.545***		1.516***	1.520***
	(0.0519)		(0.0527)	(0.0524)
D[Passive Stake](lag)		0.722***		
		(0.0521)		
Industry HFA Freq			7.674***	
			(0.314)	
Industry HFStake Freq				0.810***
				(0.224)
Firm-level controls included	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes
N	68228	68228	68228	68228
pseudo R^2	0.135	0.096	0.175	0.136

Panel C: Corporate transactions and firm-level HFA threat: D[Passive Stake]

	(1) Merger	(2) Divestiture	(3) Sale	(4) Acquisition	(5) Acquire Private
D[Passive Stake]	0.0414***	0.0131***	0.0513***		
	(0.00347)	(0.00330)	(0.00445)		
D[Passive Stake] × D[Small]				-0.00469	-0.00545
				(0.00553)	(0.00420)
D[Passive Stake] × D[Large]				-0.0151*	-0.0167***
				(0.00795)	(0.00561)
Firm-level control variables	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes
N	65430	62935	65430	60602	60602
adj. R^2	0.021	0.069	0.047	0.086	0.044

Table 3.6: Industry HFA threat and corporate transactions

This table presents evidence on the relationship between industry activism threat and corporate transaction activities. The dependent variable is a dummy that is equal to one if a transaction of the designated type occurs in year t; Sale is equal to one if a merger or a divestiture occurs in year t. We report OLS regressions in all panels. If a firm is targeted by an activist hedge fund in year t, we exclude years $[t, t+3]$ for that firm to eliminate the direct activism target impact. Panel A and Panel B measure the industry threat with Industry HFA Freq and Industry HFStake Freq, respectively, and Panel C reports estimates from a reduced form 2SLS regression, where we use FIFB as an instrument for Industry HFA Freq and Industry HFStake Freq. All panels include firm-level controls and industry-level controls. Firm-level control variables include Institutional Ownership, Tobin's Q, ln(MV), Book Leverage, Dividend Yield, Cash Flow, Sales Growth, Asset Growth, R&D, Excess Cash, HHI, CAR[Year t-1], and D[Divestiture][t-1] (D[Divestiture][t-1] only used in regression of divestiture). All firm controls are 1 year lagged. Industry-level control variables include TotM&A_3yr, HHI, Industry-year median Tobin's Q, Industry-year S.D. of Tobin's Q, and Industry-year median absolute change of ROA, Sales Growth, Employee Growth, and Turnover (as proposed in Harford (2005); all measured in year t-1). D[Large] (D[Small]) is a dummy equal to 1 if the firm's size is larger (smaller) than the industry-year median size of firms (all measured in year $t-1$). Industry fixed effects and year fixed effects are included in all regressions. Standard errors are clustered at the firm level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Measuring industry HFA threat by Industry HFA Freq

	(1) Merger	(2) Divestiture	(3) Sale	(4) Acquisition	(5) Acquire Private
Industry HFA Freq	0.00168	0.0425***	0.0480**		
	(0.0140)	(0.0160)	(0.0213)		
Industry HFA Freq × D[Small]				0.0634**	0.0558**
				(0.0300)	(0.0227)
Industry HFA Freq × D[Large]				-0.0910**	-0.0435*
				(0.0366)	(0.0255)
Firm-level control variables	Yes	Yes	Yes	Yes	Yes
Industry-level control variables	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes
N	60618	58307	60618	56512	56512
adj. R^2	0.018	0.074	0.045	0.075	0.041

Panel B: Measuring industry HFA threat by Industry HFStake Freq

	(1) Merger	(2) Divestiture	(3) Sale	(4) Acquisition	(5) Acquire Private
Industry HFStake Freq	0.0281**	0.0280**	0.0538***		
	(0.0111)	(0.0125)	(0.0160)		
Industry HFStake Freq× D[Small]				0.0677***	0.0266*
				(0.0224)	(0.0161)
Industry HFStake Freq× D[Large]				-0.0477**	-0.0394***
				(0.0223)	(0.0151)
Firm-level control variables	Yes	Yes	Yes	Yes	Yes
Industry-level control variables	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes
N	60618	58307	60618	56512	56512
adj. R^2	0.018	0.074	0.046	0.076	0.041

Panel C: Measuring industry HFA threat by FIFB (Reduced-form 2SLS regression)

	(1) Merger	(2) Divestiture	(3) Sale	(4) Acquisition	(5) Acquire Private
FIFB (Percentile Rank)	0.0114**	0.0131**	0.0233***		
	(0.00556)	(0.00580)	(0.00769)		
FIFB (PR) × D[Small]				0.0107	0.000152
				(0.00933)	(0.00688)
FIFB (PR) × D[Large]				-0.0438***	-0.0153*
				(0.0115)	(0.00850)
Firm-level control variables	Yes	Yes	Yes	Yes	Yes
Industry-level control variables	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes
N	58898	56659	58898	54988	54988
adj. R^2	0.018	0.074	0.046	0.076	0.041

Table 3.7: Overall impact of HFA pressure on corporate transaction activity

This table reports logit regressions investigating the overall impact of HFA on corporate transactions. We estimate the HFA target effect (separately analyzed in Table **??**) and the industry HFA threat effect (separately analyzed in table **??**) in one combined framework. D[Activist] is defined as in Table **??**. D[High HFA Threat] is a dummy for high industry HFA threat, which equals 1 if the firm is in the top quintile of Industry HFA Freq and D[Activist] = 0. D[Medium HFA Threat] is a dummy for mid industry HFA threat, which equals 1 if the firm is in the second and third highest quintile of Industry HFA Freq and D[Activist] = 0. Prob. conditional on HFA targets is the estimated probability fixed the D[Activist] = 1, D[High HFA Threat] = 0, D[Mid HFA Threat] = 0, and other controls are fixed at the mean values of the HFA targets sample. Prob. conditional on High HFA Threat is calculated in the same way but fixing other controls at the mean values of the sample of High HFA Threat firms. Marginal effect is defined as the prob. conditional on HFA exposure minus the conditional probability if the exposed firms were not exposed. Firm-level control variables are the same as in Table **??**. Industry fixed effects and year fixed effects are always included. Standard errors are clustered at the firm level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Logistic regressions and marginal effects (mergers and divestitures)

	(1) Logit Merger	(2) Logit Divestiture	(3) Logit Sale
D[Activist]	0.756***	0.474***	0.676***
	(0.0656)	(0.0818)	(0.0536)
D[High HFA Threat]	0.0609	0.145**	0.106**
	(0.0592)	(0.0642)	(0.0447)
D[Medium HFA Threat]	0.0547	0.0515	0.0546
	(0.0468)	(0.0519)	(0.0352)
Firm-level control variables	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes
N	71879	68772	72357
pseudo R^2	0.051	0.173	0.071
Marginal effect of Activist	+5.31%	+2.60%	+7.44%
Prob. conditional on HFA targets	*10.56%*	*7.22%*	*16.68%*
Marginal effect of High HFA Threat	+0.28%	+0.52%	+0.81%
Prob. conditional on High HFA Threat	*4.92%*	*3.97%*	*8.64%*

Panel B: Logistic regressions and marginal effects (acquisitions)

	(1) Logit Acquisition
D[Activist] × D[Small]	-0.0610
	(0.0956)
D[High HFA Threat] × D[Small]	0.219***
	(0.0646)
D[Medium HFA Threat] × D[Small]	0.0169
	(0.0554)
D[Activist] × D[Large]	-0.317***
	(0.0901)
D[High HFA Threat] × D[Large]	-0.128***
	(0.0480)
D[Medium HFA Threat] × D[Large]	0.00613
	(0.0389)
Firm-level control variables	Yes
Industry and Year fixed effect	Yes
N	66896
pseudo R^2	0.111
For Small Firms:	
Marginal effect of Activist	-0.40%
Prob. conditional on HFA targets	*6.26%*
Marginal effect of High HFA Threat	+1.50%
Prob. conditional on High HFA Threat	*8.22%*
For Large Firms:	
Marginal effect of Activist	-4.55%
Prob. conditional on HFA targets	*15.18%*
Marginal effect of High HFA Threat	-2.16%
Prob. conditional on High HFA Threat	*20.29%*

Table 3.8: Activism pressure and industry asset liquidity

This table reports industry-year regressions linking activism pressure and industry real asset liquidity. We assign each corporate transaction to the industry in which the transaction takes place (in which the firm or asset sold is located). We require at least 3 public firms in each industry-year to be included in our regression sample. We determine the real asset liquidity (RAL) using two dimensions of deal activity, Frequency (number of transactions) and Transaction Value (sum of all transaction values). For Frequency, we define real asset liquidity as the number of transactions divided by the number of public firms in industry j and in year t (transaction frequency). For Transaction Value, we define real asset liquidity as the total value of transactions divided by the total market value of public firms in industry j and in year t, similar to Ortiz-Molina and Phillips (2014)'s measure. We only consider completed transactions, and each transaction is counted only once. Panel A reports the baseline regression of real asset liquidity, without distinction by buyer/seller relation. In Panel B, we distinguish the transactions by status of buyer (insider v. outsider), and in Panel C, we distinguish the transactions by status of buyer and status of seller (insider v. outsider). Insiders are public firms (buyers or sellers) with primary 3-digit SIC code in the same industry in which the transaction takes place; outsiders are all other buyers or sellers. Outsiders include in particular public firms in other industries, private firms, and private equity sponsors. Panel D reports regressions of ratio of transactions with outside buyers, where the dependent variable is the percentage of transactions acquired by outside buyers in industry j and in year t; regressions in Panel D only use the sample of transactions with inside sellers. The main explanatory variable, D[Industry HFA Freq P80], equals 1 if Industry HFA Freq of the industry-year is in the top quintile of the whole industry-year sample. The industry-year control variables, including HHI, Industry-year median of Tobin's Q, Leverage, Cash Flow, Sales Growth, Cash, R&D, and Assets Growth, and the Industry-year S.D. of Tobin's Q, are controlled in all panels. Industry fixed effects and year fixed effects are always included. All coefficients are multiplied by 100 for readability. Standard errors are clustered at the industry level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Total real asset liquidity

Dependent Variable: Real Asset Liquidity (ral)	(1)	(2)
Measure of RAL:	Frequency	Transaction Value
D[Industry HFA Freq P80]	2.501	1.528**
	(1.623)	(0.725)
Industry-level control variables	Yes	Yes
Industry and Year fixed effect	Yes	Yes
Number of Industry-Year obs.	4783	4783
adj. R^2	0.574	0.233
Number of transactions	23,704	23,704

Panel B: Real asset liquidity sorted by outsider/insider buyer

Dependent Variable: Real Asset Liquidity (ral)	(1)	(2)	(3)	(4)
Buyer status:	Buyer = Outsider		Buyer = Insider	
Measure of RAL:	Freq	Value	Freq	Value
D[Industry HFA Freq P80]	2.519*	1.616**	-0.0159	-0.0868
	(1.464)	(0.720)	(0.467)	(0.130)
Industry-level control variables	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes
Number of Industry-Year obs.	4783	4783	4783	4783
adj. R^2	0.584	0.230	0.158	0.149
Number of transactions	15,425	15,425	8,279	8,279

Panel C: Real asset liquidity sorted by outsider/insider buyer and seller

Dependent Variable: Real Asset Liquidity (ral)				
	(1)	(2)	(3)	(4)
Seller/buyer status:	Seller = Insider Buyer = Outsider		Seller = Insider Buyer = Insider	
Measure of RAL:	Freq	Value	Freq	Value
D[Industry HFA Freq P80]	1.706***	1.439**	0.0553	0.0416
	(0.607)	(0.653)	(0.136)	(0.105)
Number of Industry-Year obs.	4783	4783	4783	4783
Number of transactions	5,776	5,776	2,579	2,579
	(5)	(6)	(7)	(8)
Seller/buyer status:	Seller = Outsider Buyer = Outsider		Seller = Outsider Buyer = Insider	
Measure of RAL:	Freq	Value	Freq	Value
D[Industry HFA Freq P80]	0.802	0.175	-0.0619	-0.128*
	(1.229)	(0.420)	(0.445)	(0.0749)
Number of Industry-Year obs.	4783	4783	4783	4783
Number of transactions	9,649	9,649	5,700	5,700

Panel D: Regression of outsider buyer's ratio

Dependent Variable: Outsider Buyer's Ratio		
	(1)	(2)
Measure of ratio:	Ratio of Frequency	Ratio of Transaction Value
D[Industry HFA Freq P80]	4.337*	4.274*
	(2.241)	(2.450)
Industry-level control variables	Yes	Yes
Industry and Year fixed effect	Yes	Yes
Number of Industry-Year obs.	2267	2267
adj. R^2	0.145	0.144

Table 3.9: Activism pressure, asset redeployability and outsider buyers

This table reports the transaction-level regressions on the relationship between industry activism pressure, asset redeployability and type of buyer. The regression sample includes 8,355 transactions of industry assets with insiders as sellers, as defined in Table 8. We only include transactions that occur in industry-years with at least 3 public firms in the baseline sample. In Panel A, the left-hand side variable is a dummy variable equal to one if the buyer in the transaction is from outside the industry, the private equity fund outside the industry, and the strategic buyer outside the industry respectively. The main explanatory variable, D[Industry HFA Freq P80], equals one if Industry HFA Freq is in the top quintile of the sample. D[Activism on Seller] is a dummy equal to one if there is activism campaign(s) launched against the seller in the 2 years prior to the transaction announcement. In Panel B, we interact the Redeploy Score with D[Industry HFA Freq P80]. We obtain industry-level Redeploy Score from online appendix of Kim and Kung (2017). High (Low) Redeploy Score is a dummy equal to one if the industry-level Redeploy Score is above (below) the median of whole sample. Firm-level controls are the same as in Table 4. Standard errors are clustered at the industry level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Regression of probability of buyer type

	(1) D[Outsider]	(2) D[Outsider:PE]	(3) D[Outsider:SB]
D[Industry HFA Freq P80]	0.0309**	0.0290*	0.00137
	(0.0134)	(0.0167)	(0.0184)
D[Activism on Seller]	0.0173	0.0151	-0.000125
	(0.0247)	(0.0192)	(0.0276)
Firm-level control variables	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes
N	5824	5824	5824
adj. R^2	0.089	0.094	0.053

Panel B: Regression of probability of buyer type (Interaction with Redeploy Score)

	(1) D[Outsider]	(2) D[Outsider:PE]	(3) D[Outsider:SB]
D[Industry HFA Freq P80] × High Redeploy Score	0.148***	0.0889***	0.0553
	(0.0486)	(0.0309)	(0.0423)
D[Industry HFA Freq P80] × Low Redeploy Score	0.102**	0.0198	0.0806*
	(0.0395)	(0.0262)	(0.0454)
High Redeploy Score	0.0119	0.0295*	-0.0167
	(0.0420)	(0.0173)	(0.0374)
Firm-level control variables	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes
N	5452	5452	5452
adj. R^2	0.031	0.043	0.013

Table 3.10: Price pressure under HFA impact

This table reports transaction-level regressions investigating the price pressure hypothesis. We only include transactions that occur in industry-years with at least 3 public firms in the baseline sample. Panel A reports the regressions of Seller CARs and premiums, Panel B provides the estimate of interaction with Redeploy Score, and Panel C reports regressions of Buyer CARs. Industry HFA Freq and Industry HFStake Freq are both measured for the industry in which the transaction takes place (in which the firm or firm asset is located). D[Activism on Seller] is a dummy equal to one if activists launch a campaign against the seller in the 2 calendar years prior to the transaction (either merger or divestiture); D[No Activism] is equal to $1 - $ D[Activism on Seller]. The transaction level controls are a dummy for payment by stock, TotM&A_3yr (measured in asset industry), Institutional Ownership, Tobin's Q, ln(MV), Book Leverage, Dividend Yield, Cash Flow, Sales Growth, Asset Growth, R&D, and Excess Cash (accounting measures are those of the seller in Panel A and B and those of the buyer in Panel C). In regressions of the merger sample, we also include control dummies for competing bids, successful bids, and unsolicited bids. All left-hand side variables are winsorized at the 1% and 99% level. All CARs are estimated with a market model using daily stock prices data in CRSP. Asset industry fixed effects and year fixed effects are always included. Standard errors are clustered at the firm level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Price pressure for sellers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sample of Divestitures				Sample of Mergers			
	Seller's CAR [-2d, +2d]		Seller's CAR [-5d, +5d]		Premium [1 month]		Target's CAR [-43d, +1d]	
Industry HFA Freq × D[No Activism]	-0.0283**		-0.0428**		-0.272***		-0.226**	
	(0.0139)		(0.0180)		(0.102)		(0.0878)	
Industry HFA Freq × D[Activism on Seller]	0.00975		0.0317		-0.0805		-0.125	
	(0.0418)		(0.0520)		(0.170)		(0.127)	
Industry HFStake Freq × D[No Activism]		-0.0224*		-0.0277		-0.187**		-0.111
		(0.0127)		(0.0169)		(0.0837)		(0.0700)
Industry HFStake Freq × D[Activism on Seller]		0.00423		0.0362		-0.100		-0.104
		(0.0388)		(0.0422)		(0.154)		(0.105)
Transaction-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5420	5420	5422	5422	4100	4100	4024	4024
adj. R^2	0.034	0.034	0.024	0.023	0.118	0.117	0.162	0.161

Panel B: Price pressure for sellers (interaction with Redeploy Score)

	(1)	(2)	(3)	(4)
	Sample of Divestitures		Sample of Mergers	
	Seller's CAR [-2d, +2d]	Seller's CAR [-5d, +5d]	Premium [1 month]	Target's CAR [-43d, +1d]
Industry HFA Freq × D[Activism on Seller]	0.00418	0.0325	-0.151	-0.159
	(0.0448)	(0.0552)	(0.179)	(0.135)
Industry HFA Freq × D[No Activism] × High Redeploy Score	-0.0257	-0.0434	-0.288*	-0.262*
	(0.0214)	(0.0268)	(0.168)	(0.156)
Industry HFA Freq × D[No Activism] × Low Redeploy Score	-0.0361**	-0.0491**	-0.350***	-0.276***
	(0.0171)	(0.0233)	(0.123)	(0.0933)
Transaction-level controls	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes
N	5173	5176	3911	3853
adj. R^2	0.035	0.025	0.120	0.164

Panel C: Price pressure for buyers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sample of Divestitures				Sample of Mergers			
	Buyer's CAR [-2d, +2d]		Buyer's CAR [-5d, +5d]		Acquirer's CAR [-2d, +2d]		Acquirer's CAR [-5d, +5d]	
Industry HFA Freq × D[No Activism]	-0.0240		-0.0137		0.0299		0.0659*	
	(0.0254)		(0.0329)		(0.0290)		(0.0396)	
Industry HFA Freq × D[Activism on Seller]	0.116*		0.142*		-0.0540		0.00288	
	(0.0652)		(0.0762)		(0.0505)		(0.0583)	
Industry HFStake Freq × D[No Activism]		0.0370		0.0758**		0.0352		0.0644**
		(0.0286)		(0.0354)		(0.0218)		(0.0263)
Industry HFStake Freq × D[Activism on Seller]		0.0573		0.0455		-0.0426		0.0371
		(0.0570)		(0.0648)		(0.0488)		(0.0580)
Transaction-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry and Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2845	2845	2845	2845	2168	2168	2173	2173
adj. R^2	0.000	0.000	0.019	0.020	0.076	0.077	0.048	0.048

Table 3.11: HFA impact on the efficiency of acquisitions

This table studies the ex-post operating performance of acquirers in acquisitions of public and private firms and subsidiaries of public firms. We require all acquisitions to be completed. We include observations from 5 years prior to and 5 years post each completed acquisition. Panel A studies the performance of acquirers in activism acquisitions. D[Activism Acq] is a dummy variable equal to one if it is an activism acquisition, defined as an acquisition in which the acquirer was targeted by activists in the 2 years (730 days) prior to the acquisition announcement. D[Post Acquisition] is a dummy variable equal to one if the firm is within $[t+1, t+5]$ years after the acquisition announcement. D[Post HFA] is a dummy variable equal to one in the post $[t+1, t+5]$ HFA event period. Panel B investigates the ex-post operating performance of acquirers under high industry HFA threat. In Panel B, we drop all activism acquisitions from the sample. We use Industry HFA Freq as our measure of the industry HFA threat. D[High HFA Threat] is a dummy equal to one if the firm is in the top quintile of Industry HFA Freq in the year when the acquisition is announced and is not a current activism target. D[Small] is a dummy equal to one if the firm's size is smaller than the industry-year median size of firms in the year before the announcement of acquisition. Following Bebchuk, Brav, and Jiang (2015), we include ln(MV) and ln(Age) as controls in each regression. Year fixed effects and firm fixed effects are always included. Standard errors are clustered at the firm level (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Efficiency of acquisitions by HFA target firms

	(1) Tobin's Q	(2) ROA	(3) Sales/Assets(lag)
D[Post Acquisition]	-0.330***	-0.0138***	-0.109***
	(0.0213)	(0.00205)	(0.00834)
D[Post Acquisition] × D[Small]	0.238***	0.0162***	0.0583***
	(0.0271)	(0.00308)	(0.0124)
D[Post Acquisition] × D[Activism Acq]	-0.0671	-0.00576	-0.0159
	(0.0620)	(0.00615)	(0.0222)
D[Post Acquisition] × D[Activism Acq] × D[Small]	0.0257	0.0252**	0.0935**
	(0.118)	(0.0126)	(0.0380)
D[Post HFA]	0.136***	0.000345	0.0187
	(0.0283)	(0.00337)	(0.0136)
Firm-level controls	Yes	Yes	Yes
Year and Firm fixed effect	Yes	Yes	Yes
N	50335	47484	50087
adj. R^2	0.553	0.621	0.800

Panel B: Efficiency of acquisitions by firms under high HFA threat

	(1) Tobin's Q	(2) ROA	(3) Sales/Assets(lag)
D[Post Acquisition]	-0.185***	-0.0170***	-0.0931***
	(0.0331)	(0.00326)	(0.0125)
D[Post Acquisition] × D[Small]	0.195***	0.0125**	0.0401*
	(0.0597)	(0.00635)	(0.0235)
D[Post Acquisition] × D[High HFA Threat]	0.000568	-0.000647	-0.0115
	(0.0518)	(0.00494)	(0.0214)
D[Post Acquisition] × D[High HFA Threat] × D[Small]	-0.0133	0.0133	0.0962
	(0.114)	(0.0142)	(0.0600)
D[Post HFA]	0.0566	-0.00698	-0.0345*
	(0.0507)	(0.00590)	(0.0190)
Firm-level controls	Yes	Yes	Yes
Year and Firm fixed effect	Yes	Yes	Yes
N	49293	46525	49110
adj. R^2	0.556	0.620	0.800

Table 3.12: Acquirer and target characteristics in acquisitions by small acquirers

This table investigates acquirer and target characteristics in acquisitions by small acquirers. Regressions are in transaction level, in which case we require both the acquirer and target are public listed firms and their Tobin's Q and ROA information is not missing. The explained variable is always equal to target's characteristic minus acquirer's. All characteristics are measured one year before the bidding year. NumPats, NumCites, and PatValue denote number of patents, number of citations, and Kogan, et. al. (2017)'s estimated value of the patent in nominal dollars respectively. Patent's data are from Kogan, et. al. (2017). Standard errors are clustered in firm level. (standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Panel A: Target's characteristics by small acquirer

Target's − Acquirer's	(1) Tobin's Q	(2) ROA	(3) NumPats	(4) NumCites	(5) PatValue
D(Small)	0.292***	0.0289**	0.414***	1.344***	1.753***
	(0.104)	(0.0137)	(0.0372)	(0.103)	(0.108)
Year F.E.	Yes	Yes	Yes	Yes	Yes
Acquirer Industry F.E.	Yes	Yes	Yes	Yes	Yes
Target Industry F.E.	Yes	Yes	Yes	Yes	Yes
Num. Obs.	1644	1601	1782	1782	1782
Adj. R^2	0.096	0.137	0.407	0.450	0.518

Panel B: Target's characteristics by small acquirer under high HFA threat

Target's − Acquirer's	(1) Tobin's Q	(2) ROA	(3) NumPats	(4) NumCites	(5) PatValue
Industry HFA Freq × D(Large)	1.304	0.243	3.549***	7.211***	9.715***
	(1.200)	(0.154)	(0.424)	(1.199)	(1.236)
Industry HFA Freq × D(Small)	-0.437	0.0772	1.475***	3.031**	4.105***
	(1.462)	(0.187)	(0.515)	(1.454)	(1.499)
D(Small)	0.378***	0.0363**	0.522***	1.591***	1.992***
	(0.134)	(0.0177)	(0.0499)	(0.141)	(0.145)
Year F.E.	Yes	Yes	Yes	Yes	Yes
Acquirer Industry F.E.	Yes	Yes	Yes	Yes	Yes
Target Industry F.E.	Yes	Yes	Yes	Yes	Yes
Num. Obs.	1644	1601	1782	1782	1782
Adj. R^2	0.096	0.137	0.407	0.450	0.518

Chapter 4

Hedge Fund Activism, Corporate Governance, and Product Market Competition

4.1 Introduction

Previous research on hedge fund activism establishes that activist hedge funds facilitate the value improvements of target firms (Brav et al. [2008a], Brav et al. [2015a], and Brav et al. [2015b]). And financial economists have made efforts to explore the precise mechanism through which hedge fund activism improves firm value. Boyson et al. [2017] and Gantchev et al. [2020] document that hedge fund activism creates value by influencing takeover activities of target firms, i.e., more likely to be acquired, less empire-building acquisitions, and more divestitures. However, the value creation mechanism for those target firms not involved in mergers and acquisitions is still not fully understood. Moreover, little has been done to explain the cross-sectional variation of target firm's value improvement. This variation is especially important in the dimension of the quality of corporate governance measured prior to the launch of activist campaigns (ex-ante governance) as well as product market competition (again measured at the start of campaigns), as it helps to understand how activist's influence interacts with internal governance of target companies that are an indispensable part in the transmission and implementation of activist proposals and initiatives. Ex-post higher value improvements in bad governance firms imply that hedge fund activism substitutes for ex-ante governance of target firms, while a higher value improvement in good governance firms implies that it complements ex-ante governance.

My focus on governance, product market competition, and their interactions is motivated by Giroud and Mueller [2010] and Giroud and Mueller [2011], arguing that competition serves as a substitute for internal governance, an alternative device that impose discipline on managerial decisions. As a result, focusing on the interaction of governance and product market competition provides a complete picture on how the HFA target impact varies across firm's ex-ante governance.

I demonstrate that target value improvements of hedge fund activism (measured in Tobin's Q, Return on Assets (ROA), and market cumulative abnormal returns (CAR)) are strongly related to the target's corporate governance and product market competition. Following the literature, I use G-index as a measure regarding the corporate governance, always measured at the start of the campaign so as to exclude the target impacts on governance itself.[1] The level of product market competition is measured by Hircman-Herfindahl Index (HHI), and in each year, I sort the whole Compustat sample into tercile by HHI.

I use G-index in two ways: one discrete and one continuous method. First, I construct tercile dummies for G-Index: good governance firms (G-index ≤ 7), medium governance

[1] G-index is the governance index constructed by Gompers et al. [2003]. A higher index value represents worse governance and severer management entrenchment. It consists of 22 corporate-level provisions, such as poison pill, classified board, golden parachutes, and six state anti-takeover laws, such as Business Combination Law and Fair Price Law. When the firm possesses one item, for example, the poison pill index adds one point. All index components are equally weighted.

firms ($8 \leq$ G-index ≤ 10), and bad governance firms (G-index ≥ 11).[2] This classification enables me to capture in a simple way the interaction between target firms' governance and product market competition and their impacts on the value improvement of target firms. For instance, I can study specifically the value creation on target firms with good governance (G-index ≤ 7) and operated in relatively competitive industries (low HHI tercile). Second, I use G-index directly and interact it with dummies of HHI tercile. Both approaches lead to similar conclusions, implying that good governance firms and firms in competitive industries benefit the most from HFA campaigns.

Using a Difference in Difference (DID) method, I find that the target value improvements in activist campaigns are negatively correlated with the G-index measured at the start of campaigns, implying that initial better governance firms benefit more from HFA. Moreover and surprisingly, good governance firms operating in competitive industries benefit the most from HFA campaigns. This result is robust across various model specifications, industrial classification, and imposing an exogenous variation on G-index by using the state-level anti-takeover laws. The result is counter-intuitive because good governance firms operating in relatively competitive industries should suffer the least from agency costs and should have operated on the industrial efficiency frontier due to both good governance and high competition force. As a result, their further value improvements should be minimal.

To explore in more detail this puzzling result, first, I decompose the realized value creation due to HFA campaigns into two parts: (i) expected value improvements from an HFA campaign conditional on the HFA campaign success and (ii) a campaign success dummy. This decomposition relies on a simplifying assumption: hedge fund activism improves firm value only if the campaign launched by the activist hedge fund ultimately succeeds. Next, I offer a novel tradeoff: On the one hand, the quality of initial good governance (less entrenchment) raises the probability of success in an activism campaign,

[2]The 7 and 11 are the corresponding tercile thresholds of G-index among target sample. We could have used different thresholds to sort firms, and results are robust across thresholds change.

while initial bad governance (more entrenched) reduces the probability and requires more costs in the campaign process (for example, for proxy contests and taking board seats). On the other hand, a greater potential return is hidden behind when activist hedge funds target those initial bad governance firms in relatively concentrated industries (as they are the most undervalued firms due to relevant findings of Giroud and Mueller [2011]). As a result, I conjecture that this tradeoff might drive the puzzling results.

To test this idea in the data, I manually collect campaign goals and results of the 412 activism campaigns in the IRRC dataset. Among 412 campaigns, 103 can be classified as successful, with the leading activist hedge fund achieving its targeting goals. In the first set of tests, I investigate the campaign success probability. I find that HFA campaigns targeting initial good governance firms are more likely to succeed: the target firm actively responds to activist hedge funds' demands and changes its management and organization (such as through divestitures). Moreover, this success probability is the highest when HFA targets those initial good governance firms in relatively competitive industries. The results at least partially explain the previous puzzling results on value improvements. In the second set of results, I regress the value creation (again measured by Tobin's Q) but conditional on campaign success. In a small sample, I find that the aforementioned puzzling results disappear.

Overall, this paper contributes to the literature from mainly two aspects. First, this paper investigates threefold relations among the target impact of hedge fund activism, the initial governance of target firms, and product market competition. While there is a large literature on the effects and performance drivers of HFA, the combined influence of these two dimensions has not been studied.[3] In the literature, the most related work

[3]Thanks to pioneering work by Brav et al. [2008a], research on hedge fund activism flourishes rapidly among corporate governance literature. Recent studies have expanded the literature from many aspects, for example, through merge and acquisition [Boyson et al., 2017, Gantchev et al., 2020]; sequential decision and cost of activist hedge funds [Gantchev, 2013]; the long-term performance of target firms [Bebchuk et al., 2015a]; real productivity of plants and labor outcomes [Brav et al., 2015a]; international studies regarding the returns of hedge fund activism [Becht et al., 2015]; corporate innovation [Brav et al., 2018]; the threat effect of hedge fund activism on creditors [Feng et al., 2016]; and a novel discipline force even to industrial peer firms [Gantchev et al., 2016].

is Aslan and Kumar [2016] who examines the product market spillover effects of hedge fund activism (HFA) on the industry rivals of target firms. They document that HFA has negative real and stockholder wealth effects on the average rival firm, with the effects strengthened in less concentrated and low entry barrier industries.

Second, this paper contributes to the literature by systematically studying determinants of success probabilities in activist campaigns. There are very few papers among recent literature investigating the success probabilities of campaigns. An exception is Appel et al. [2016]. Using the Russell 1000/2000 index as a natural experiment, they find that the passive investors have significant influences on activism campaigns, tactics, and success of activists. Related to my paper, they document that a higher passive ownership is associated with increasing use of proxy fights and a higher probability of HFA campaign success, i.e., obtaining board seats or selling HFA target company.

The remaining article is organized as follows. Section 4.2 develops hypotheses; Section 4.3 discusses data source and the sample construction; Section 4.4 studies the average target effect of activism campaigns in IRRC sample; Section 4.5 studies the target effect across firms' initial governance and product market competition and describe the puzzles; Section 4.6 explores more details regarding the puzzle.

4.2 Hypothesis Development

In this section, I start developing hypotheses regarding the relation between the target impact of hedge fund activism and the initial governance of the target firm. First, I introduce the following decomposition "equation".

$$\text{Realized Value Improvement} \approx I(Success) \times (\Delta V | I(Success) = 1) \tag{4.1}$$

Equation 4.1 simplifies the real world and assumes that an hedge fund activism event improves firm value only if this campaign launched by an activist hedge fund ultimately succeeds.[4] The campaign is deemed as successful if one of the major goals of involved hedge funds have been achieved at the end of the campaign. As a result, the realized value improvement of a target firm can be decomposed into two parts: the Value Improvement Conditional on Campaign Success and a Dummy of Success in the Campaign. Here is the first hypothesis.

Hypothesis 1: *Firms with worse initial governance may experience higher value improvements after the activist campaigns. If the governance is measured by G-index during the time activist starts the campaign, the value improvement is increasing in G-index.*

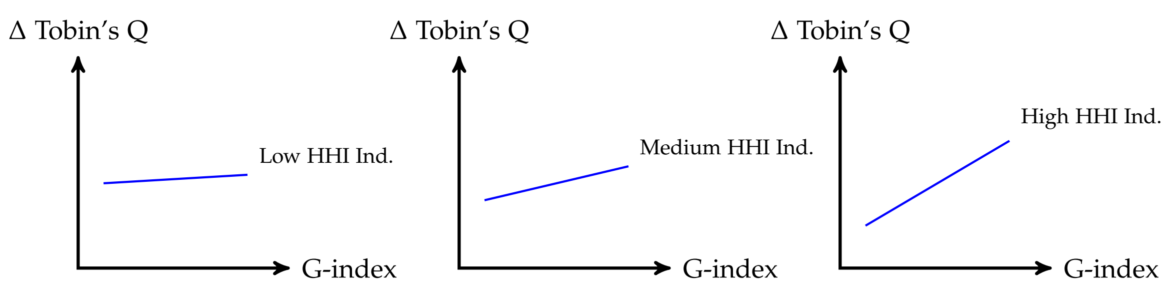
Gompers et al. [2003] document that bad governance firms (high G-index) have a lower value than good governance firms (low G-index).[5] As a result, bad governance firms might possess higher ex-post potential for value improvements if activist hedge funds do succeed in resolving the operating inefficiency. Equivalently, Value Improvement Conditional on Success of An Activist Campaign (the second term in the righthand side of equation 4.1) is higher for target firms with bad governance, which further implies, by equation 4.1, that the realized value improvement of bad governance firms (high G-index) is larger.

Hypothesis 2: *If the value improvement after an activist campaign is a decreasing function of governance quality, this relation should be stronger in relatively concentrated industries than that in relatively competitive industries.*

[4]One might suspect whether it is an excellent approximation of the real world. Is it possible that a target firm experiences a significant value improvement even if the campaign ultimately fails? I test this simplification in table 2, Panel A, and find results supporting it. Both the average and median of firm value improvements of unsuccessful campaigns are close to zero, while both the average and median of firm value improvements of successful campaigns are significantly above zero.

[5]They use Tobin's Q to measure the firm value in their regressions.

This hypothesis could be attributed to Giroud and Mueller [2011]'s relevant findings. In their seminal paper, Giroud and Mueller [2011] argue that firms in concentrated industries benefit more from good governance than do firms in relatively competitive industries. It implies a substitutional relationship between governance and product market competition which is firstly modeled in Hart [1983]. In the absence of both competition and good governance, bad governance firms in relatively concentrated industries suffer more from agency costs than bad governance firms in relatively competitive industries, and thus the former one should possess higher potential of value improvements ex-ante. As a result, Value Improvement Conditional on the Success of Activist Campaign (the first term in the righthand side of equation 4.1) is even larger for target firms with bad governance (high G-index) in relatively concentrated industries than target firms with bad governance (high G-index) but in competitive industries. In other words, the difference of observed value improvement between bad governance firms (high G-index) and good governance firms (low G-index) is larger in concentrated industries than that in competitive industries.[6] Finally, Hypothesis 2 can be illustrated in the following figure, where the value improvement is measured by Δ Q:



4.3 Data and Summary Statistics

I merge firm-level data on corporate governance and institutional ownership with hedge fund activist campaign data. I now briefly describe each data source, sample construc-

[6] As a substitute to governance, competition is redundant and should have no effect on potential value improvement for good governance firms.

tions and ultimately the summary statistics.

4.3.1 Hedge fund activist campaign

The activist hedge fund data are obtained from hand-collections of the U.S. Security and Exchange Commission (SEC) Schedule 13D fillings. The SEC gives the following definition of a SC 13D form: "When a person or group of persons acquires beneficial ownership of more than 5% of a voting class of a company's equity securities registered under Section 12 of the Securities Exchange Act of 1934 (and has an interest in influencing management of the company), they are required to file a Schedule 13D with the SEC." Names of hedge funds can be identified from "*Item 2. Identity and Background*" and activist's goals from "*Item 4. Purpose of the Transaction*" in relevant 13D fillings. The starting date of activism campaign (usually called targeting date) is defined as the filling date of the first 13D form. The activists' goals, tactics, shares percentage acquired, and the ultimate results of campaigns are hand-collected from a variety of sources, including SEC 13D, 13D(A), 14A, online business news, newspaper archives, research papers, and non-academic books. The detailed classification of goals and tactics follows those in Brav et al. [2008a]. For more details about hand-collection procedures, see Brav et al. [2008a].

Figure 4.1 plots the time series of numbers of hedge fund activism campaigns from 1994 to 2011. Figure 4.2 presents the numbers of activist campaigns across Fama and French 48 industries. Those popular industries that are frequently target by hedge funds are banking, business services, electronic equipment, pharmaceutical product and retail.

4.3.2 Corporate governance

I retrieve corporate governance data from the Investor Responsibility Research Center (IRRC) database. The IRRC database contains 24 corporate governance provisions, which include 22 corporate-level items such as poisson pill, classified board, and golden parachute and 6 state-level anti-takeover laws such as business combination law and fair price law. To test the core hypotheses, especially the hypothesis 1 and 2 in section 4.2, it is needed to firstly make sure that there exists a strong substitutional relation between corporate governance and market product competition, or else the rejection of my main hypothesis might attribute to a very weak or even no substitutional relation between corporate governance and competition in the sample that I use.

As a result, I carefully adopt all procedures of sample selections and variable definitions in Giroud and Mueller [2011]: "Following GIM, we exclude all firms with dual-class shares. To match firms to industries, we, moreover, require a nonmissing SIC code in Compustat.[7] ... The G-index is obtained from the IRRC database and is available for the years 1990, 1993, 1995, 1998, 2000, 2002, 2004, and 2006 during the sample period. For intermediate years, we always use the G-index from the latest available year. ... Our main measure of product market competition is the HHI. The HHI is computed as the sum of squared market shares,

$$HHI_{jt} = \sum_{i=1}^{N_j} s_{ijt}^2 \tag{4.2}$$

where s_{ijt} is the market share of firm i in industry j in year t. Market shares are computed from Compustat using firms' sales (item #12). When computing the HHI, we use all available Compustat firms, including those with dual-class shares. We exclude firms for which sales are either missing or negative ... We classify industries using the 48 industry classification scheme of Fama and French (1997, FF). We assign firms to industries by matching the SIC codes of Compustat to the 48 FF industries using the conversion table in the appendix of FF."

Moreover, following Giroud and Mueller [2010], I require that, in every year, each industry contains at least 5 firms to get rid of the small "spike" at the right endpoint of the empirical HHI distribution. After finishing these procedures, I compare the sum-

[7] I use the historical SIC code in Compustat, item $SICH$.

mary statistics with Giroud and Mueller [2011]'s original one and find them very similar. Furthermore, I replicate the Tobin's Q regression (see Page 586 in Giroud and Mueller [2011]) and again obtain almost the same coefficients and standard errors. The original IRRC database ranges from 1990 to 2006. I extend the IRRC sample to 2007 and 2008 by assigning the same G-index of 2006 to the same firm in 2007 and 2008.[8] In the final step, I merge the IRRC data set with the hedge fund activism data set and obtain 412 activism campaigns from 1994 to 2008. Table 4.3 summarizes classifications of goals and tactics of campaigns launched by activist hedge funds in IRRC sample, and it also reports the ratio of successful campaigns across different goals. Appendix 4.8 explains how I judge/decide the success for each HFA campaign.

4.3.3 Institutional ownership

Institutional ownership data are extracted from the Thomson Retuers 13F database, which provides the equity positions every quarter end of all institutions that exercise investment discretion over at least 100 million U.S. dollars. Since a great amount of data in the item of total numbers of share outstanding are missing in Thomson Retuers 13F database, I instead extract it from the CRSP monthly stock file. I match them with PERMNO and further drop those observations of which the institutional ownership ratio is greater than one. Furthermore, duplicated observations by mgrno, PERMNO and rdate are dropped. Because all institutional ownership ratios calculated in data are in quarterly level, I adjust to yearly by simply averaging 4 quarterly ratios in the same year. Finally, I apply Bushee [2001]'s permanent three category classifications of institutional investors as (i) "Quasi Indexers" (low turnover, high diversification), (ii) "Transient Investors" (high turnover, high diversification) and (iii) "Focused Investors" (low turnover, low diversification, also called "Dedicated").

[8] The fact that there is few cross-firm variation in G-index allows me to do that.

4.3.4 Summary statistics

Table 4.1 reports basic summary statistics for all variables that will be used in the following regression analysis. To facilitate comparison, data are classified into four groups: (1) firms in the IRRC database that have never been targeted by activist hedge funds (control group 1); (2) firms not in the IRRC database that have never been targeted by activist hedge funds (control group 2); (3) firms in the IRRC database that were targeted by activist hedge funds (treatment group 1); (4) firms that were targeted by activist hedge fund and not in the IRRC database (treatment group 2). Groups (3) and (4) are the ex-ante observations, i.e. for each target firm, the data only contains one firm-year observation that is the one year before hedge fund(s) initiate the campaign and influence its management.[9] At this point, I simply note that all variables are standard and consistent with Brav et al. [2008b]'s finding. Hedge funds tend to target firms with smaller market capitalization (MV), lower Tobin's Q, higher institutional ownership, firms with higher G-index and E-index. Table 4.2 investigates a linear probability model of HFA targeting and corroborates the results in Table 4.1.

4.4 Average Target Effect on target Firms

4.4.1 Identification strategy and results

To begin, I first investigate whether the average target effect on target firms (ATT) in the IRRC sample is similar relatively to the ATT in the whole sample.[10] Following Bebchuk

[9]Note that unions of four categories above are not tantamount to the whole sample since those firm-year observations of target firms in the years except one year before target year do not belong to any groups.

[10]The target effect can be deemed as the standard average treatment effect on the treated (ATT) in program evaluation. To clearly identify the ATT, the stable unit treatment value assumption has to be made. It requires in particular that individuals in control group are not influenced by the treatment. Specifically, peer effects and general equilibrium result are not allowed. Unfortunately, Gantchev et al. [2016] and Feng et al. [2016] provide evidence of peer effects, while Aslan and Kumar [2016] document the

et al. [2015a], I use the Tobin's Q, defined as the sum of book value of debt and market value of equity divided by the sum of book value of debt and book value of equity, to measure the firm value. I also use the return on assets (ROA), defined as EBITDA divided by assets (lag), to measure performances. More specifically, I regress the following equation,

$$AdjQ_{ijt} = \sum_{k=-2}^{2} \alpha_{k+3} D\_Target[t+k]_{ijt} + \mathbf{X'}_{ijt}\boldsymbol{\beta} + v_j + u_t + \varepsilon_{ijt} \tag{4.3}$$

where i denotes the firm, j denotes the industry, and t denotes year. The dependent variable $AdjQ$ ($AdjROA$) is the industry-year median-adjusted Tobin's Q (ROA) where industries are classified according to the Fama-French 48 industry classification. Equation 4.3 follows Bebchuk et al. [2015a], where $D\_Target[t+k]_{\{k=-2,-1,0,1,2\}}$ denotes dummy variables equal to one if firm i was or will be targeted by hedge funds in the year $t+k$. Specially, $D\_Target[t]$ where $k=0$ denotes the targeting dummy that equals 1 in the year of targeting. These five dummies fully capture the time series evolution of Tobin's Q (ROA) that is associated with the hedge fund activism event 5 years around an activism campaign. Other independent variables in $\mathbf{X'}$ are MV (natural log of market capitalization) and firm age. Year fixed effects and industry (firm) fixed effects are contained in all regressions. Standard errors are clustered in the industry level and are robust to heteroscedasticity.

Table 4.4 summarizes results from estimating the Equation 4.3. I focus on the coefficient of $D\_Target[t+2] - D\_Target[t]$. In fact, $D\_Target[t+2] - D\_Target[t]$ follows the same spirit of a traditional Difference in Difference (DID) estimator. $D\_Target[t]$ captures value differences between target firms and non-target firms in the year of targeting, while $D\_Target[t+2]$ estimates value differences between target firms and non-target firms again but in the second year after targeting. The final difference in difference gets rid of any potential selection bias and trend bias.

industrial effect. In this paper, I do not distinguish the direct target effect of hedge fund activist campaign from potential peer effect or industrial effect. Instead, I study the aggregate effect.

Columns (1) and (2) are regressions using the full sample. As for the economic magnitude of the effect, the average targeting effect (ATT) on Tobin's Q is 0.244 (significant in the 90% of confidence level), which is tantamount to a 15% increase on the basis of 1.67, the mean of Tobin's Q for target firms. Similarly, the average targeting effect (ATT) on ROA is 0.0243 (significant in 95% of confidence level), which is tantamount to a 69% increase on the basis of 0.035, the mean of the ROA for target firms.

Columns (3) and (4) show regressions with firm fixed effects instead of the industry fixed effects. Columns (5) and (6) run regressions with only the IRRC sample. All estimated average target effects are similar both in coefficients and standard errors. Next step, I compare my results with the original one in Table 4 of Bebchuk et al. [2015a]: their (t+3)-(t) estimate is 0.29 for Tobin's Q and 0.025 for ROA; both are very close to mine.[11]

4.5 Target Effect across Initial Governance and Product Market Competition

4.5.1 DID strategy and main results

This section tests hypotheses developed in Section 4.2. First, I begin with a graphical analysis of 412 activism campaigns that can be merged with the IRRC dataset. Figure 4.3 plots firms' value improvements of target firms across their initial G-index and HHI tercile. The y axis is the change of industry-year adjusted Tobin's Q from year t (the target year) to year t+2, while x axis is the level of governance measured by G-index. The industry-year adjusted Tobin's Q is computed by subtracting the industry median in a given 48 FF industry and year. Higher G-index implies more entrenchment and worse

[11] The small difference may originate from (i) they might not adjust years of targeting event to fiscal year; (ii) they use the SIC 3 digit code to classify industry while I use FF 48 industries; (iii) they might use the whole COMPUSTAT database, while I use CCM and apply some sample selection procedures following Giroud and Mueller [2010] and Giroud and Mueller [2011].

governance. In each year, all IRRC target firms are classified into tercile groups by HHI, and different HHI tercile are separately plotted. G-index and HHI are both measured in year t, the target year.

In general, the ex-post firm value improvement is decreasing in G-index, which implies that hedge fund activism complements to internal governance in the process of creating value for the target firms. Nevertheless, there is a great heterogeneity from Panel A to C. In the low HHI tercile (a relatively competitive industry), the firm value improvement drops the most in response to the rising of G-index. In the medium HHI tercile, the slope of fitted line is much smaller than the one in low HHI tercile, while in the high HHI tercile (relatively concentrated industry) the fitted line even has a small and positive slope.

In the panel of the low HHI tercile of Figure 4.3, there are some outliers with extremely large Δ Q. To exclude the possibility that those outliers drive the main findings, I conduct a quantile regression in which case I regress the difference of Tobin's Q from year t (target year) to year t+2 on G-index interacted with HHI tercile dummies. The estimate gives a result of -0.041** for the coefficient of HHI_Low × G-index, -0.004 for HHI_Medium × G-index; and 0.024 for HHI_High × G-index, which is comparable to the OLS fitted line.[12] I reach a similar conclusion in figure 4.4, which studies the change of the industry-year median-adjusted Tobin's Q from year t (the target year) to year t+3.

Next step, I test the hypotheses with formal regressions. Departing from Brav et al. [2015c]'s setting, I instead apply a new Difference in Difference (DID) setting. Specifically, I estimate the following equation,

$$\begin{aligned}\Delta AdjQ_{ij,t:t+2} = {}& \boldsymbol{\alpha}' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} + \boldsymbol{\lambda}' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} \times G\text{-}index_{ijt} + \gamma_1 HHI\_High_{jt} + \\ & \gamma_2 HHI\_Medium_{jt} + \boldsymbol{\delta}' \mathbf{HHI}_{jt} \times G\text{-}index_{ijt} + \mathbf{X}'_{ijt}\boldsymbol{\beta} + v_j + u_t + D\_Target[t]_{ijt} \times \iota_f + \varepsilon_{ijt}\end{aligned} \tag{4.4}$$

[12] Extra controls are HHI_Low, HHI_Medium, MV and year fixed effect.

where i denotes the firm, j denotes the industry, and t denotes year. The left-hand side variable $\Delta AdjQ_{t:t+2}$ is a difference of industry-year median adjusted Tobin's Q from year t to year t+2, i.e. $\Delta AdjQ_{i,t:t+2} = AdjQ_{i,t+2} - AdjQ_{i,t}$ for each firm i. The industry-year adjusted Tobin's Q is computed by subtracting the industry median in a given 48 FF industry and year. Under this setting, the first difference has been already adopted into the dependent variable, thus the coefficient of D_Target[t] alone identifies the similar Average Target Effect on Targets (ATT) as in the previous section.

Each year I sort firms' HHI into tercile, and **HHI** is a 3×1 vector containing dummies of high, medium and low HHI. To further analyze ATT across initial G-index and HHI, D_Target[t] is interacted with G-index and HHI both measured in year t. Due to a triple interaction term, D_Target[t] $\times$ **HHI** $\times$ G-index, I add D_Target[t] $\times$ **HHI**, **HHI** $\times$ G-index, HHI_High and HHI_Medium as extra controls. To avoid perfect multi-collinearity, I exclude D_Target[t], G-index, HHI_Low and D_Target[t] $\times$ G-index from the regression. Other independent variables in $\mathbf{X}'$ are MV, size, age, dummy of S&P500, D_Target[t-1], D_Target[t+1] and dummy of Delaware corporations. To control the mean reverse of Tobin's Q, I add the past change of Tobin's Q from year t-2 to t and its interaction with the target dummy as extra two controls. Year fixed effects, u_t, industrial fixed effects, v_j, and activist hedge fund fixed effects, $D\_Target[t]_{ijt} \times \iota_f$, are included in regressions. Standard errors are clustered in the industry level and are robust to heteroscedasticity.

To begin the analysis, if hedge fund activism works as a substitute to the internal governance in the process of creating value for target firms, I would expect a positive and statistically significant coefficient on D_Target[t] $\times$ HHI_High $\times$ G-index as implied in Hypothesis 2 (in section 4.2), i.e., in a concentrated industry, the positive target impact on initial bad governance firms is larger than that impact on good governance firms. Similarly, I would expect a smaller and statistically insignificant coefficient on D_Target[t] $\times$ HHI_Low $\times$ G-index since high G-index, a proxy of initial bad governance, does not add more potential gains on the target firm in the competitive industries since competition already serves as a proxy of governance to prevent managers enjoying a "quiet life".

Nevertheless, suggested by the result in Table 4.5, Columns (1) and (2), it shows the opposite. Column (1) regresses the difference of Tobin's Q from year t to t+2. In column (1), the coefficient of D_Target[t] × HHI_Low × G-index is -0.106 and significant in 95% confidence level, implying that an increase in initial G-index of target firm by one s.d. is associated with 10.6% lower target effects in a relatively competitive industry. Also, in Columns (3) and (4), I find the same pattern of coefficients in regressions of Return on Assets (ROA). For example, in Column (3), there is monotonic rise of coefficients from D_Target[t] × HHI_Low × G-index (-0.378%*), D_Target[t] × HHI_Medium × G-index (-0.177%), and D_Target[t] × HHI_High × G-index (0.002%).

In short, I document a strong negative relation between G-index and the target impact both on Tobin's Q and ROA only in the low HHI tercile. The relation is weaker for firms in the medium HHI tercile and completely disappears in the high HHI tercile.

4.5.2 Which group of target firms benefits the most from HFA?

In this section, I first conduct a new test to compare value improvements of target firms across one dimension, either across the governance or HHI. Table 4.6 shows that ex-ante better governance firms predict higher value improvements after HFA campaigns. Surprisingly, the ex-ante HHI is not correlated with value gains, which is at odds with findings of Aslan and Kumar [2016]. They document that HFA has negative real and stockholder wealth effects on the average rival firm, with the effects strengthened in less concentrated and low entry barrier industries. However, their study uses a larger sample than my IRRC sample to gauge the value improvements. Moreover, they focus on rivals' values instead of the target's value changes.

Next, I sort all IRRC firms into nine groups with the help of the tercile dummies of HHI and G-Index and run the following regressions. Finally, I investigate which group of target firms benefits the most from HFA:

$$\Delta AdjQ_{ij,t:t+2} = \lambda_1' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} \times G\_Good_{ijt} + \delta_1' \mathbf{HHI}_{jt} \times G\_Good_{ijt} + \gamma_1 HHI\_High_{jt} + \gamma_2 HHI\_Medium_{jt} + \lambda_2' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} \times G\_bad_{ijt} + \delta_2' \mathbf{HHI}_{jt} \times G\_bad_{ijt} + \mathbf{X'}_{ijt}\boldsymbol{\beta} + v_j + u_t + D\_Target[t]_{ijt} \times \iota_f + \varepsilon_{ijt} \quad (4.5)$$

In my benchmark setting, G_Good equals 1 if G-index ≤ 7, and G_bad equals 1 if G-index ≥ 11.[13] Results in table 4.7 document that good governance firms in competitive industries benefit the most from the target impacts of hedge fund activism among all target firms. Almost in each setting, D_Target $\times$ G_Good $\times$ HHI_Low has the most positive coefficient and is significant in 1% level.

Next, following Bebchuk et al. [2015a], I estimate the abnormal returns of target firms after the hedge fund activism campaigns. For each firm targeted by a hedge fund activist, I estimate a monthly alpha based on the Fama-French-Carhart four-factor model, where observations in the regressions are monthly returns within three years (36 months) after activist hedge fund(s) target the firms. In some cases, the return data in CRSP is missing in one specific month. Then, delisting return data is replaced for it. Moreover, I require a minimum of twenty-four monthly returns available following the intervention or HFA targeting. After the estimating, I sort target firms into tercile groups by HHI. Then, in each tercile, I define G_Good as one if G-index ≤ 7, and G_bad equals one if G-index ≥ 11. Table 4.8 reports the result, where I reach a similar conclusion as in Tobin's Q regression: good governance firms in relatively competitive industries experience the most substantial average and median abnormal market return.

[13] 7 and 11 are the tercile points for the empirical distribution of G-index among target firms.

4.5.3 Selection bias and robust tests

One crucial concern regarding my analyses in section 4.5.1 and 4.5.2 is endogeneity. There are two types of selection bias confounding the analysis. The first case is the selection bias between target firms and non-target firms within each cell (In Section 4.5.2, observations are sorted into terciles using both the HHI and G-index and form 9 different cells). The second source originates from selection bias across cells. To mitigate the selection bias in the first case, I apply the Mahalanobis-Metric Matching to obtain the control firms from each cell to match the target firms in that cell. The covariates used in the matching are the past change of Tobin's Q and the past level of Tobin's Q within two years before the campaigns. Then, I combine both target firms and control firms of each cell into one panel and re-run Diff-in-Diff. The result is obtained in column 1 of the online appendix Table A2, similar to the benchmark result.

To mitigate selection bias in the second case, I construct a state-level anti-takeover law index using six state law provisions in the IRRC database and use it as a plausible instrument of G-index.[14] The estimate is illustrated in table 4.9, column (5) and (6). The coefficients are similar to columns (1) and (2) but with less significance.[15]

Finally, I conduct miscellaneous robustness checks for results of table 4.9. Table

[14]The identification comes from two facts that (i) passing of anti-takeover law deteriorates governance (Relevant Condition is satisfied. See Bertrand and Mullainathan [2003b]) and (ii) there is no systematic relationship between the state of incorporation and state of location (Exclusive Condition). I further control time-varying industry shock and time-varying local economic shock in the state of the firm's location. The identification strategy is similar to Bertrand and Mullainathan [2003b]. By controlling local economic shocks, change in the anti-takeover law of New York state is very likely to be uncorrelated with characteristics of firms incorporated in New York but located in California.

[15]One potential concern about the effectiveness of this instrument is that most anti-takeover laws have been passed around 1985-1990, but activism campaign in our database ranges from 1994 to 2008. If most firms were set up after 1990, they could self-select the state of incorporation during the IPO. Moreover, state anti-takeover law varies across the state. See the relevant study by Daines [2002]. My argument about the validity of this instrument comes from the fact that there are 64% of firms established before 1990 in the IRRC database. There are 3000 firms in the IRRC database, and 1927 firms were incorporated before 1990. To further mitigate the concern, I study the subsample incorporated before 1990, and the results are the same despite fewer observations. One might also worry about how firms can change the state of incorporation. It rarely happens in our database (less than 5%).

4.9 includes the test of (i) replacing HHI with Four-firm Concentration Ratio (CR4); (ii) replacing G-Index with E-index; (iii) using 3-digit SIC codes to classify industry instead of FF 48; (iv) sorting HHI into quintile instead of tercile; (v) using change rate of Tobin's Q instead of difference of Tobin's Q as a new dependent variable, and all results are again very robust.

4.6 Success probability of activism campaign

The result in the previous section is not consistent with Hypotheses 1 and 2 developed in section 4.2. Therefore, in this section, I explain this puzzle by separately analyzing the HFA campaign success probability and target firms' value improvements conditional on the campaign success.

I decompose the realized value creation into two parts: (i) the expected value improvement from HFA campaigns conditional on campaign success and (ii) a dummy of success of the HFA campaign. Equation 4.1 simplifies the real world and assumes that hedge fund activism improves firm value if and only if the campaign launched by an activist hedge fund ultimately succeeds. On the one hand, initial good governance (less entrenchment) raises the probabilities of success in activism campaigns, while the initial bad governance reduces the probabilities and requires more costs in the campaign process (for example, for proxy contests and taking board seats). On the other hand, a greater potential return is hidden behind when activist hedge funds target those initial bad governance firms in relatively concentrated industries (as they are the most undervalued firms due to the relevant findings of Giroud and Mueller [2011]).

Table 4.10 regresses a dummy of success for each campaign in the IRRC sample on G-Index, HHI Index, and tercile dummies of G-Index and HHI separately. In Column (1), I find that HFA campaigns with initial good governance firms are more likely to succeed. This implies that the target firms actively respond to activist hedge funds' demands and change the company management following their requirements. Column 3 shows that the highest likelihood of the campaign success lies in those initial good governance firms in relatively competitive industries. In the second set of results in Table 4.11, I regress the value creation (again measured by Tobin's Q) but conditional on a campaign success sample. In this small sample, I find that the aforementioned puzzling results about value improvement finally disappear. As a result, the puzzling result is at least partially driven by the tradeoff on campaign success probabilities.

4.7 Conclusions

In this paper, I formally test two hypothesises that are implied both by intuition and by previous findings in corporate governance literature. Firstly, I test the hypothesis that the discipline force of hedge fund activism works as a substitute for company's governance and market product competition. Then I test the hypothesis that those bad governance firms in concentrated industry benefit the most from these campaigns launched by activist hedge funds. Using a hand-collected data set with 412 activism campaigns that can be merged with IRRC database, I document opposite and counterintuitive results that (i) discipline force of hedge fund activism works as a complement to firm's governance and market product competition; and (ii) those target firms in competitive industry initially having good governance benefit the most from activism campaign. I conduct various robustness checks, apply propensity score matching to the target decisions, and further construct a state anti-takeover law index in order to use it as the instrument of governance, but results remain unchanged. Furthermore, I find that the puzzle (counterintuitive results) might be attributable to the heterogeneity of success probability in activism campaigns.

4.8 Appendix: Procedures to Judge the Success in Activism Campaign

This appendix provides detailed information regarding to the definition and judgement process of success in activism campaigns launched by activist hedge funds. The definition is borrowed from Brav et al. [2008b] but with some small adjustments.[16] The hedge fund activism campaign is deemed successful if activist hedge fund(s) ultimately achieve their major stated goals in 13D forms during the campaign. If activist hedge fund has stated only one goal in SC 13D forms, then the stated goal is regarded as the major goal. If there are multiple goals stated in 13D, then the goals that conduct large changes to target company or directly bring profits to share holders are deemed as major goals. If there exist multiple major goals, the campaign is deemed as successful if activist hedge fund(s) achieve one of its goals. Here are the major goals:

(1). Directly bring profits to share holders: Share repurchase; and dividend distribution

(2). Conduct large changes to the company: Against M&A; oust CEO; force to sell the whole company or part of assets; business restructure and spin off; buyout the company; and take control

If there is no above major goal stated in 13D forms, minor goals are regarded as major goals. Here are some representative minor goals: operational efficiency; suggest reducing cost; criticize excess cash; lack of focus; rescind takeover defense; seek board independence; and excess compensation. In many cases, minor goals are only the process of achieving major goals.

To determine whether these major goals are achieved, I obtain further information from reading online business news, newspaper archives, research papers and non-

[16] The original definition from them is quite obscure. They define the success of an activist campaign if the hedge fund achieves its main stated goals.

academic books about hedge fund activism. Here is the detailed procedure to make judgement of success:

1. **Share repurchase:** The goal is achieved if (1) the target company finally announces share repurchases; and (2) by the time of announcing, the hedge fund still holds shares of company and does not exit. In some cases, the exit date of hedge fund is quite obscure and hard to pin down. For these cases, I set up a substitutional threshold requires that the target company should announce share repurchase plan within 1 year after activist hedge fund(s) demand this goal. Share repurchase information is retrieved from business news, online share buyback history, and COMPUSTAT data. Exit information about hedge funds is from 13D(A) and 13G forms.

2. **Dividend distribution:** Similar as 1. Share repurchase.

3. **Sell the company:** The goal is achieved if (1) the manager of target company ultimately signs agreement to be acquired by other companies; and (2) by the time of reaching an agreement to sell the company, the hedge fund still holds shares of company. In some cases, the exit date of hedge fund is hard to pin down. For these cases, I set up a substitutional threshold requires that the target company ultimately finds bidder to sell themselves within three years after activist hedge fund(s) demand this goal. The longer time span of three years is due to the fact that this goal always involves in proxy contest to seek board seats and hire an investment bank to evaluate strategy alternatives. As a result, it usually takes more time.

4. **Business spin off:** Similar as 3. Sell the company.

5. **Against M&A:** The goal is achieved if the stated merge or acquisition deal ultimately fails or the target firm announces they give up the merge or acquisition. In a special case when target firm is a target for M&A, it is still deemed successful if the biding price increases after activist hedge fund objects to the M&A and finally the hedge fund accepts the new bidding.

6. **Buyout the company:** The goal is achieved if the company accepts the unsolicited offer from activist hedge funds.[17]

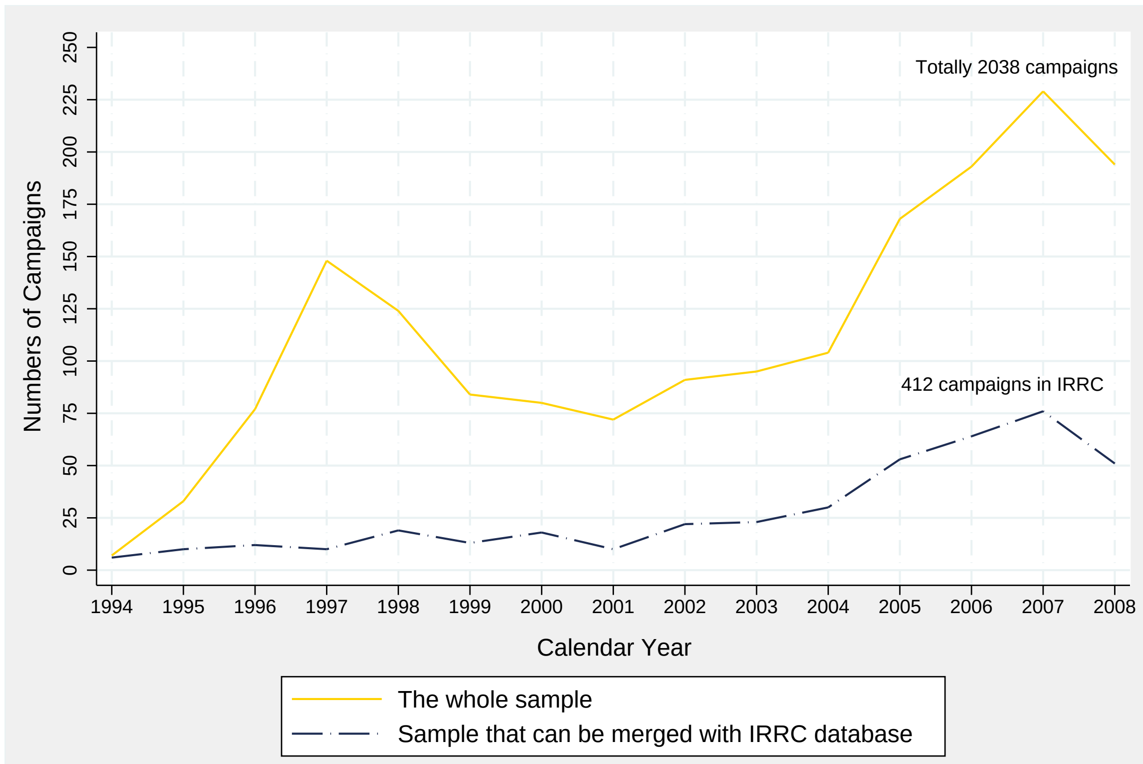
7. **Take control:** The goal is achieved if activist hedge fund wins majority boards in proxy contest or the fund controls major shares for example through a tender offer.

8. **Oust CEO:** The goal is achieved if (1) the target company finally announces to resign the CEO; and (2) by the time of announcing, the hedge fund still holds shares of company. In some cases, the exit time of hedge fund is quite obscure and hard to pin down. For these cases, I set up a substitutional threshold requires that the target company should resign the CEO within two years after activist hedge fund(s) demand this goal .

4.9 Tables and Figures

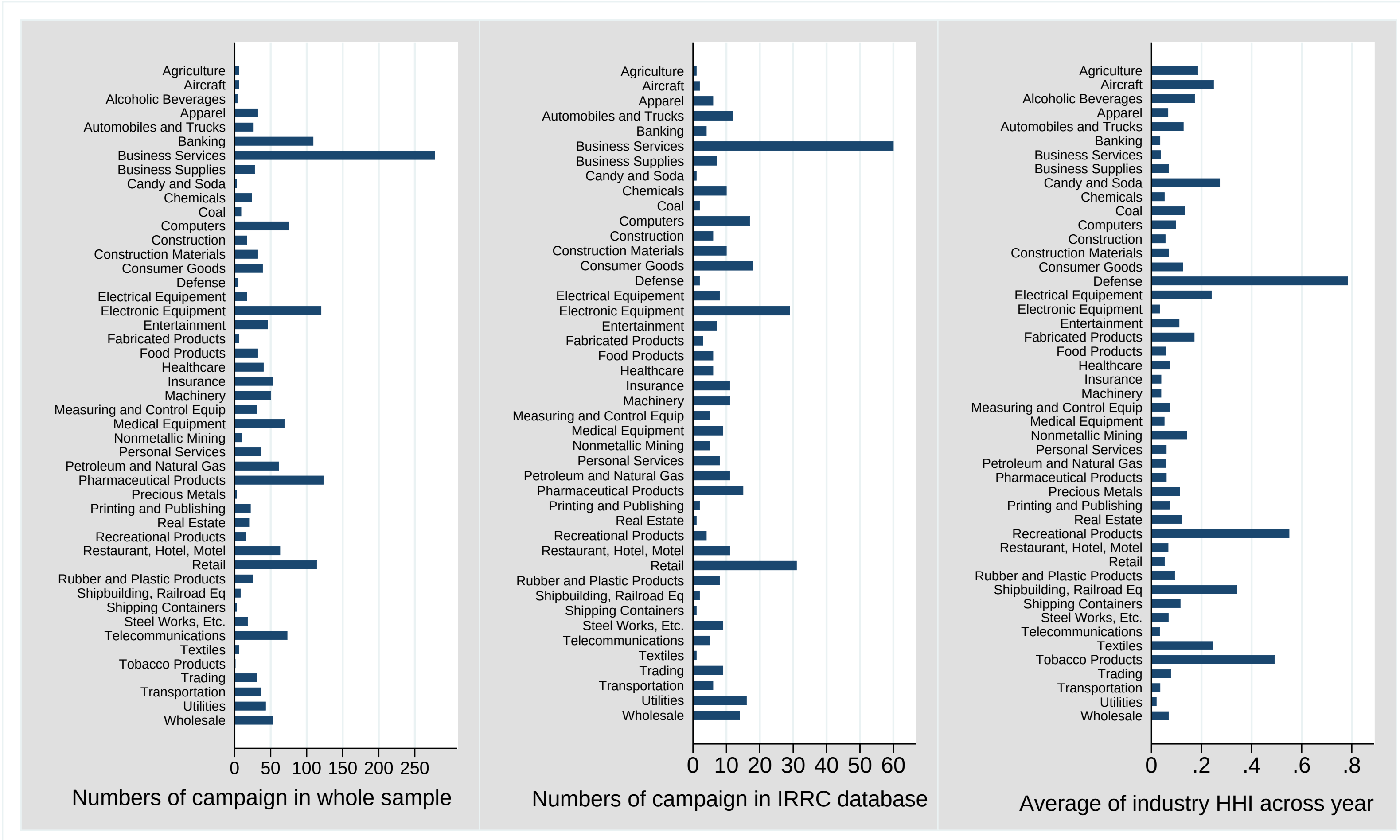
[17] In some rare cases, there appears competing bids that edging off the accepted one provided by activist hedge fund. In this case, it is still deemed as a successful campaign since the appearance of new bidders brings even bigger profits both to share holders and hedge fund.

Figure 4.1: Numbers of campaigns launched by activist hedge funds across calendar year



The figure plots the numbers of campaigns launched by activist hedge funds in the whole sample and in the sample that can be merged with the IRRC database respectively (IRRC database offers corporate governance information regarding G-index). The whole sample ranges from 1994 to 2011 with around 2000 campaigns, while the sample that can be merged with the IRRC database covers 1994-2008 with 412 campaigns. The original IRRC database ranges from 1990 to 2006. I extend the IRRC sample to 2007 and 2008 by assigning the same firm's G-index of 2006 to the same firm in 2007 and 2008.

Figure 4.2: Numbers of campaigns launched by activist hedge funds across Fama-French 48 industries



The figure plots the numbers of campaigns launched by activist hedge funds in the whole sample and in the sample that can be merged with the IRRC database respectively across Fama-French 48 industries (IRRC database offers corporate governance information such as G-index and state anti-takeover law). The whole sample ranges from 1994 to 2011 with around 2000 campaigns, while the sample that can be merged with the IRRC database covers 1994-2008 with 412 campaigns. The third panel of figure 2 plots the average Herfindahl-Hirschman index (HHI) of industry across year.

Figure 4.3: Value improvement of target firms across their initial G-index and HHI tercile

The figure plots value improvement of target firm across their initial G-index and HHI tercile. The sample contains 412 activism campaign from 1994 to 2008 that can be merged with IRRC database. The y axis is the change of industry-year adjusted Tobin's Q from **year t (the target year) to year t+2**, while x axis is corporate governance measured by G-index. Industry-year adjusted Tobins Q is computed by subtracting the industry median in a given 48 FF industry and year. Higher G-index means greater entrenchment and worse governance. In each year, target firms are classified into tercile by HHI, and different HHI tercile are separately plotted. G-index and HHI are both measured in year t (the target year).

Figure 4.4: Robustness check: value improvement of target firms across their initial G-index and HHI tercile

The figure plots value improvement of target firm across their initial G-index and HHI tercile. The sample contains 412 activism campaign from 1994 to 2008 that can be merged with IRRC database. The y axis is the change of industry-year adjusted Tobin's Q from **year t (the target year) to year t+3**, while x axis is corporate governance measured by G-index. Industry-year adjusted Tobin's Q is computed by subtracting the industry median in a given 48 FF industry and year. Higher G-index means greater entrenchment and worse governance. In each year, target firms are classified into tercile by HHI, and different HHI tercile are separately plotted. G-index and HHI are both measured in year t (the target year).

This table reports basic summary statistics of variables used in regression analysis. The mean is in coefficients and standard deviation in parentheses. To facilitate comparison, data are classified into four groups: (1) firms in IRRC database and have never been targeted by activist hedge funds (Control group); (2) firms not in IRRC database and have never been targeted by activist hedge funds (Control group); (3) firms in IRRC database and were targeted by activist hedge funds (Treatment group); (4) firms that were targeted by activist hedge fund and not in IRRC database (Treatment group). Group (3) and (4) are ”pre-campaign” observations, i.e. for each target firm, they only contain one firm-year observation that is one year before hedge fund(s) initiate campaign and influence its management.

Table 4.1: Summary statistics of variables

	(1) Not targeted in IRRC	(2) Not targeted in remaining	(3) Targeted in IRRC	(4) Targeted in remaining
MV (*ln*(*Market capitalization*))	7.500	5.178	6.688	4.819
	(1.667)	(2.179)	(1.484)	(1.533)
Q (*Tobin Q*)	1.856	2.392	1.572	1.835
	(1.421)	(14.34)	(0.932)	(3.147)
Age (*ln(Firm's age)*)	2.979	1.942	2.976	2.082
	(0.758)	(1.016)	(0.745)	(1.003)
Size (*Firm's size*)	7.769	5.366	7.030	5.138
	(1.747)	(2.290)	(1.428)	(1.670)
BM (*Book to market equity ratio*)	0.549	0.577	0.672	0.561
	(1.724)	(20.51)	(0.886)	(7.231)
Sale (*Sales*)	7.332	4.789	6.841	4.722
	(1.593)	(2.361)	(1.329)	(1.879)
salegrt (*Sale's growth*)	0.129	0.817	0.0746	0.426
	(0.803)	(46.47)	(0.238)	(4.450)
ROA (*Return of assets*)	0.138	0.0519	0.114	0.0258
	(0.130)	(1.507)	(0.126)	(0.368)
Lev (*Leverage ratio*)	0.410	0.386	0.407	0.344
	(0.580)	(5.663)	(0.473)	(0.534)
Cash (*Cash*)	0.124	0.200	0.142	0.227
	(0.161)	(0.236)	(0.182)	(0.248)
Capx (*Capital expenditure*)	0.0606	0.0695	0.0558	0.0658
	(0.0686)	(0.190)	(0.0613)	(0.161)
Rnd (*Research and development*)	0.0611	0.116	0.0548	0.124
	(0.0901)	(0.278)	(0.0701)	(0.227)
Dummy of S&P500 stocks	0.312	0.0294	0.141	0.0195
	(0.463)	(0.169)	(0.348)	(0.138)

Continued on next page

Table 1 continued from previous page

	(1) Not targeted in IRRC	(2) Not targeted in remaining	(3) Targeted in IRRC	(4) Targeted in remaining
Divyld (*Dividend yield*)	0.0184	0.0190	0.0173	0.0113
	(0.0466)	(0.113)	(0.0834)	(0.0472)
Payout (*Payout ratio*)	0.0395	0.0321	0.0392	0.0310
	(0.0527)	(0.204)	(0.0965)	(0.125)
HHI (*Herfindahl-Hirschman index*)	0.0608	0.0668	0.0749	0.0666
	(0.0633)	(0.0632)	(0.0806)	(0.0730)
Institution (*Institutional ownership*)	0.616	0.357	0.697	0.432
	(0.222)	(0.280)	(0.224)	(0.272)
G-index	9.194		9.410	
	(2.680)		(2.584)	
E-index	1.576		1.618	
	(1.103)		(1.083)	
Dummy of Delware corporation	0.546		0.611	
	(0.498)		(0.488)	
Anti-takeover Law	1.743		1.683	
	(1.291)		(1.339)	
Observations	16647	74313	398	1386

Note: Mean in coefficients; standard error in parentheses

Table 4.2: Probability of being targeted by activist hedge funds

This table presents a linear probability model about hedge fund activism targeting. All variables are defined in the same way as in Brav et al. [2008a] and are measured in one year before the potential targeting. Standard errors are clustered at the firm level.

Linear Probability Model	(1) I(HFA Target)
GIM-Index	0.00122**
	(0.000487)
MV	-0.00621***
	(0.000716)
Tobin Q	-0.00264***
	(0.00101)
Sales Growth	-0.00102
	(0.00133)
ROA	-0.0200**
	(0.00989)
Book Leverage	0.00424**
	(0.00185)
Cash	0.00553
	(0.00811)
Dividend Yield	-0.0637***
	(0.0221)
HHI	0.0133
	(0.0360)
Industry F.E.	Yes
Year F.E.	Yes
Observations	21223
Adjusted R^2	0.029

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.3: Success rates, goals and tactics of activist campaigns

In the table, Panel A shows value improvement of target firms sorted by final result of campaigns. 412 activist campaigns that can be merged with IRRC database are classified into three groups: (1) Successful campaigns; (2) Unsuccessful campaigns; and (3) Unclassified. Details regarding procedures of judging success of campaigns is documented in appendix 4.8. An activist campaign is regarded as an unclassified either because the activist hedge fund did not state the specific goals or there is no enough information to figure out its result. The value improvement of target firms is measured by difference of Tobin Q from year t (target year) to year t+2 (or t+3). In Panel B, I classify the goals of activism campaigns and calculate the success ratio for each goal category. In panel C, I classify the major tactics used by activist hedge funds.

Panel A: Value improvement of target firms sorted by final result of campaigns

	Successful campaigns	Unsuccessful campaigns	Unclassified
Numbers of activism campaigns	103	63	246
Average of ΔQ from year t (target year) to year t+2	0.105*	-0.036	0.072
Median of ΔQ from year t (target year) to year t+2	0.072	-0.061	0.015
Average of ΔQ from year t (target year) to year t+3	0.228**	-0.011	0.192***
Median of ΔQ from year t (target year) to year t+3	0.099*	-0.062	0.068

Panel B: Goals of activist campaigns

Goals of campaign in IRRC	Numbers	% of success
1. No specific goal in 13D form	239	–
2. Payout policy (share buyback/dividends distribution)	40	75.0%
3. Capital structure (for example recapitalization or restructure debt)	9	33.3%
4. Business strategy (for example against M&A or business spinning off)	77	51.9%
5. Sale of (part of) target company/liqidating	55	65.5%
6. Take control/buyout the firm/take the firm to private	29	58.6%
7. Governance (for example rescind takeover bylaw)	52	69.2%
Total in IRRC that have determinate result	166	62.0%

Panel C: Tactics of activist campaign

Tactics of campaign in IRRC	Numbers	% of whole
Friendly tactics		
1. Communicate with board/manager privately (Only 13D file)	236	57.3%
2. Seek board presentation without (threat of) proxy contest	49	11.9%
3. Send public letter or make formal shareholder proposal	65	15.8%
Hostile tactics		
4. Threaten to wage proxy contest or sue the company	31	7.5%
5. Launch proxy contest to obtain board seats or to rescind takeover defense	37	9.0%
6. Sue the company	14	3.4%
7. Launch proxy contest to take control or unsolicited offer to buy majority shares	30	7.3%

Table 4.4: Value/performance improvement of targeted firm by activist hedge funds

This table provides estimate of similar regression in Brav et al. [2015c] and Bebchuk et al. [2015b] which studies value or performance improvement of targeted firm in **2 years after** activist hedge fund initiates the campaign. The **dependent variables** are either Tobin Q or ROA. Tobin Q is defined as (book value of debt + market value of equity)/(book value of debt + book value of equity); and *ROA* is return on assets, defined as EBITDA/assets(lag). *Adj* denotes year-industry median adjusted variables. Industries are classified according to Fama-French 48 industry classification. Regressions follow equation (4.6),

$$Q_{ijt} = \sum_{k=-2}^{2} \theta_{k+2} D\_Target[t+k]_{ijt} + \mathbf{X'}_{ijt}\boldsymbol{\beta} + v_j + u_t + \varepsilon_{ijt} \tag{4.6}$$

where $D\_Target[t+k]_{\{k=-2,-1,0,1,2\}}$ are a bunch of dummy variables equal to 1 if firm i was (will be) targeted by hedge funds k years ago (after $-k$ years). Other independent variables included in X are MV and age. See variables' definition in appendix (**??**). Year fixed effects and industry fixed effects (firm fixed effect) are contained in all regressions. Standard errors are clustered in industry level. F test for difference of the coefficients are provided. Columns (1) to (4) show the regressions using the full sample; Columns (5) and (6) are the regressions using the IRRC sample; Columns (7) and (8) present the regressions after propensity score matching.

Estimate method/Sample	CRSP Compustat Full sample/OLS				IRRC sample/OLS		Full sample/PSM+OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable =	*AdjQ*	*AdjROA*	*AdjQ*	*AdjROA*	*AdjQ*	*AdjROA*	*AdjQ*	*AdjROA*
D_Target[t+2]	-0.0813	0.00973	0.291**	0.0111	0.108	0.0109	0.221	0.00756
	(0.144)	(0.0113)	(0.148)	(0.00833)	(0.0845)	(0.0108)	(0.146)	(0.0117)
D_Target[t+1]	-0.240***	-0.00471	0.139***	-0.00649	0.0470	0.00300	0.0466	-0.00558
	(0.0563)	(0.00834)	(0.0536)	(0.00849)	(0.0816)	(0.00849)	(0.0529)	(0.00697)
D_Target[t]	-0.326***	-0.0146	0.0381	-0.0143	-0.0973*	-0.0149*	-0.0423	-0.0125
	(0.0570)	(0.00983)	(0.0494)	(0.00939)	(0.0503)	(0.00752)	(0.0389)	(0.00971)
D_Target[t-1]	-0.323***	-0.00713	-0.0228	-0.0174*	-0.128**	-0.00815	-0.0371	-0.00373
	(0.0898)	(0.0122)	(0.0905)	(0.00928)	(0.0593)	(0.00647)	(0.0761)	(0.0114)
D_Target[t-2]	-0.174	0.0102	0.00899	-0.00252	-0.142***	-0.00180	0.110	0.0138
	(0.148)	(0.0124)	(0.156)	(0.00510)	(0.0519)	(0.00548)	(0.129)	(0.0114)
D_Target[t+2] – D_Target[t] (*Average Treatment Effect on Treated*)	0.244*	0.0243**	0.253*	0.0254***	0.206**	0.0258***	0.263*	0.0201***
F test(1,47)	(3.43)	(6.67)	(2.77)	(8.88)	(6.27)	(7.65)	(3.74)	(7.62)
D_Target[t] – D_Target[t-2](*Dynamics of Tobin Q/ROA before campaign*)	-0.151	-0.0248**	0.0291	-0.0118	0.0446	-0.0131*	-0.152	-0.0263***
F test(1,47)	(1.03)	(7.07)	(0.04)	(1.70)	(0.63)	(3.17)	(1.37)	(9.76)
Year fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm fixed effects	–	–	*Yes*	*Yes*	–	–	–	–
Industry fixed effects	*Yes*	*Yes*	–	–	*Yes*	*Yes*	*Yes*	*Yes*
Standard error clustered	*Industry*	*Industry*	*Firm*	*Firm*	*Industry*	*Industry*	*Industry*	*Industry*
Observations	79913	76594	79913	76594	13904	15391	6992	7080
Adjusted R^2	0.030	0.008	0.437	0.073	0.166	0.264	0.015	0.069

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table provides regressions studying the target impact of hedge fund activism across target firm's initial HHI and G-index (both measured at the start of campaign). The sample contains 412 activism campaigns from 1994 to 2008 that can be merged with IRRC database as well as other CRSP/Compustat observations with non-missing G-index. The **dependent variables** are difference of Tobin's Q from year t to **t+2**, difference of ROA from year t to **t+2**, difference of Tobin's Q from year t to **t+3**, difference of ROA from year t to **t+3** respectively. Tobin's Q is defined as (book value of debt + market value of equity)/(book value of debt + book value of equity). *Adj* denotes year-industry median adjusted variable. Industries are classified according to Fama-French 48 industry classification. Regressions follow equation (4.7),

$$\Delta AdjQ_{ij,t:t+2} = \boldsymbol{\alpha}' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} + \boldsymbol{\lambda}' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} \times Gindex_{ijt} + \gamma_1 HHI\_High_{jt} + \gamma_2 HHI\_Medium_{jt} + \delta' \mathbf{HHI}_{jt} \times Gindex_{ijt} + \mathbf{X}'_{ijt}\boldsymbol{\beta} + v_j + u_t + D\_Target[t]_{ijt} \times \iota_{fund} + \varepsilon_{ijt} \quad (4.7)$$

where $D\_Target[t]$ is the targeting dummy, which equals 1 if activist hedge fund initiates the campaign during the fiscal year t. G-index is constructed following Gompers et al. [2003]. Each year we sort firms' Herfindahl-Hirschman index (HHI) into tercile, so **HHI** is a 3×1 vector containing dummies of *high*, *medium* and *low* HHI. Activist targeting dummy $D\_Target[t]$ is interacted with G-index and HHI which are both measured in period t. Other independent variables included in X are $ln(MV)$, size, age, dummy of S&P500, dummy of Delaware corporations, and past change of Tobin's Q, $\Delta AdjQ_{t-2:t}$ (See variables' definition in appendix **??**). Year fixed effect, fund fixed effect and industry fixed effect are included in regressions. Standard errors are clustered in industry level. In the regression table, columns (1) and (2) are the OLS regressions of Tobin Q; columns (3) and (4) present the regression of ROA; and finally columns (5) and (6) show the reduced form 2SLS results in which case anti-takeover law index serves as the instrument of G-index.

Table 4.5: Ex-post improvement of target firms and their ex-ante HHI and G-index

Estimate method	OLS				2SLS	
Dependent variable =	(1) $\Delta AdjQ_{t:t+2}$	(2) $\Delta AdjQ_{t:t+3}$	(3) $\Delta AdjROA_{t:t+2}$	(4) $\Delta AdjROA_{t:t+3}$	(5) $\Delta AdjQ_{t:t+2}$	(6) $\Delta AdjQ_{t:t+3}$
D_Target[t] × HHI_High	-0.374	-0.465*	0.0102	-0.0130	0.0747	-0.354*
	(0.269)	(0.258)	(0.0360)	(0.0472)	(0.155)	(0.210)
D_Target[t] × HHI_Medium	0.521*	0.481	0.0254	-0.0115	0.262***	-0.0766
	(0.298)	(0.371)	(0.0800)	(0.0514)	(0.0908)	(0.0835)
D_Target[t] × HHI_Low	1.343***	0.722	0.0444**	0.0864*	0.627***	-0.0140
	(0.417)	(0.451)	(0.0172)	(0.0465)	(0.180)	(0.187)
D_Target[t] × G-index × HHI_High	0.0591**	0.0196	0.00000211	0.00156	0.0751*	0.0495
	(0.0225)	(0.0175)	(0.00325)	(0.00466)	(0.0429)	(0.0447)
D_Target[t] × G-index × HHI_Medium	-0.0246	-0.0584	-0.00177	0.00109	0.0202	0.00442
	(0.0285)	(0.0363)	(0.00633)	(0.00413)	(0.0398)	(0.0443)
D_Target[t] × G-index × HHI_Low	-0.106**	-0.0986**	-0.00378*	-0.00801*	-0.135*	-0.101*
	(0.0404)	(0.0418)	(0.00194)	(0.00474)	(0.0723)	(0.0563)
$\Delta AdjQ_{t-2:t}$	-0.184***	-0.222***			-0.183***	-0.222***

Continued on next page

Table 4 continued from previous page

	(0.0431)	(0.0626)			(0.0436)	(0.0633)
D_Target[t] $\times$ $\Delta AdjQ_{t-2:t}$	0.126**	-0.00436			0.0946*	-0.0718
	(0.0473)	(0.0941)			(0.0514)	(0.108)
Year fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Hedge fund fixed effect	*Yes*	*Yes*	*No*	*No*	*Yes*	*Yes*
Include state of location average Q and Δ Q	*No*	*No*	*No*	*No*	*Yes*	*Yes*
Standard error clustered	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*
Observations	14506	13546	16949	15927	14222	13275
Adjusted R^2	0.153	0.199	0.028	0.045	0.154	0.200

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.6: Targets' value improvement in one dimension

This table studies the HFA targets value improvements across target firm's ex-ante qualities of governance and the ex-ante HHI. Dependent variables are changes of the industry-year adjusted Tobin's Q measured from year t to year t+2 (t+3). The sample is the IRRC HFA targeting sample. Industry F.E. and Year F.E. are always included. Standard errors are clustered at the firm level.

Sample of Target	(1)	(2)	(3)	(4)
	Δ Tobin Q[t:t+2]	Δ Tobin Q[t:t+3]	Δ Tobin Q[t:t+2]	Δ Tobin Q[t:t+3]
G_Good	0.186**	0.213**		
	(0.0867)	(0.105)		
G_Medium	-0.0242	-0.0409		
	(0.0780)	(0.0932)		
HHI_Low			0.0369	0.00340
			(0.0817)	(0.0993)
HHI_Medium			-0.0541	0.0397
			(0.0813)	(0.0979)
Firm controls	*Yes*	*Yes*	*Yes*	*Yes*
Year fixed effect	*Yes*	*Yes*	*Yes*	*Yes*
Industry fixed effect	*Yes*	*Yes*	*Yes*	*Yes*
Observations	356	321	356	321
Adjusted R^2	0.014	0.014	0.002	0.006

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.7: Ex-post value improvement of targeted firms and firm's ex-ante HHI and G-index: Alternative test

This table provides regressions studying the target impact of hedge fund activism across target firm's initial HHI and G-index (both measured at the start of campaign). The sample contains 412 activism campaigns from 1994 to 2008 that can be merged with IRRC database as well as other CRSP/Compustat observations with non-missing G-index. The **dependent variables** are either difference of Tobin Q from year t to **t+2** or its difference from year t to **t+3**. Tobin Q is defined as (book value of debt + market value of equity)/(book value of debt + book value of equity). Adj denotes year-industry median adjusted variable. Industries are classified according to Fama-French 48 industry classification. Regression equation is as follows, which is revised from equation (4.7),

$$\Delta AdjQ_{ij,t:t+2} = \lambda_1' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} \times G\_Good_{ijt} + \delta_1' \mathbf{HHI}_{jt} \times G\_Good_{ijt} + \gamma_1 HHI\_High_{jt} + \gamma_2 HHI\_Medium_{jt} + \lambda_2' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} \times G\_Weak_{ijt} + \delta_2' \mathbf{HHI}_{jt} \times G\_Weak_{ijt} + \mathbf{X}'_{ijt}\boldsymbol{\beta} + v_j + u_t + D\_Target[t]_{ijt} \times \iota_{fund} + \varepsilon_{ijt} \quad (4.8)$$

where $D\_Target[t]$ is the targeting dummy, which equals 1 if activist hedge fund initiates the campaign during fiscal year t. In benchmark setting, G_Good equals 1 if G-index ≤ 7, and G_Weak equals 1 if G-index ≥ 11. Each year we sort firms' Herfindahl-Hirschman index (HHI) into tercile, so **HHI** is a 3×1 vector containing dummies of *high*, *medium* and *low* HHI. Activist targeting dummy $D\_Target[t]$ is interacted with dummies of G-index and HHI tercile which are both measured in period t. Other independent variables included in X are $ln(MV)$, size, age, dummy of S&P500, dummy of Delaware corporations, and past change of Tobin's Q, $\Delta AdjQ_{t-2:t}$ (See variables' definition in appendix **??**). Year fixed effect, fund fixed effect and industry fixed effect are included in regressions. Standard errors are clustered in industry level.

Setting of Governance Dummies	G_Good: G-index ≤ 7 G_Weak: G-index ≥ 11		G_Good: G-index ≤ 8 G_Weak: G-index ≥ 10		G_Good: G-index ≤ 6 G_Weak: G-index ≥ 12	
Dependent variable =	(1) $\Delta AdjQ_{t:t+2}$	(2) $\Delta AdjQ_{t:t+3}$	(3) $\Delta AdjQ_{t:t+2}$	(4) $\Delta AdjQ_{t:t+3}$	(5) $\Delta AdjQ_{t:t+2}$	(6) $\Delta AdjQ_{t:t+3}$
D_Target × G_Good × HHI_Low	0.596***	0.576***	0.602***	0.566***	1.080***	1.089**
	(0.146)	(0.185)	(0.138)	(0.151)	(0.272)	(0.455)
D_Target × G_Good × HHI_Medium	0.0749	0.450	0.312	0.709***	0.312	0.140
	(0.228)	(0.333)	(0.266)	(0.218)	(0.432)	(0.227)
D_Target × G_Good × HHI_High	0.0256	0.0645	0.0882	0.0875	0.223**	0.242*
	(0.111)	(0.103)	(0.116)	(0.116)	(0.103)	(0.144)
D_Target × G_Weak × HHI_Low	-0.0721	-0.122	-0.00532	-0.00358	-0.0273	-0.0548
	(0.145)	(0.125)	(0.0811)	(0.103)	(0.0973)	(0.132)
D_Target × G_Weak × HHI_Medium	0.0701	0.0280	0.0931	0.165	0.139	0.142
	(0.0686)	(0.125)	(0.0922)	(0.117)	(0.0879)	(0.175)
D_Target × G_Weak × HHI_High	0.0991	0.0353	0.163	0.137	0.180	0.0561
	(0.0954)	(0.102)	(0.104)	(0.115)	(0.150)	(0.124)
Year fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Hedge fund fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Standard error clustered	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*
Observations	14506	13546	14506	13546	14506	13546
Adjusted R^2	0.156	0.200	0.156	0.201	0.157	0.201

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.8: abnormal returns to target firms

This table reports statistics on abnormal returns to target firms subsequent to hedge fund activism. For each firm targeted by a hedge fund activist, I estimate a monthly alpha based on the Fama-French-Carhart four-factor model, where the observations in the regressions are monthly return within three years (36 months) after activist hedge fund targets the firm. Return data is from CRSP. In the case that return data is missing in one certain month, delisting return is replaced for it. Moreover, I require a minimum of twenty-four monthly returns following the intervention. After that, I sort targeted firms into tercile by HHI. Then in each tercile, I define G_Good equals to 1 if G-index ≤ 7, and G_Weak equals to 1 if G-index ≥ 11. The following 4 panels show mean, median, standard deviation of abnormal return and numbers of stock in each cell respectively.

	Tercile of HHI			
Governance	Low	Median	High	Total
	Mean alpha	Mean alpha	Mean alpha	Mean alpha
Weak	-0.31%	0.35%	0.33%	0.21%
Medium	0.06%	0.42%	-0.76%*	-0.11%
Good	0.98%***	0.74%	0.42%	0.73%***
Total	0.32%	0.44%**	-0.04%	0.22%**

	Tercile of HHI			
Governance	Low	Median	High	Total
	Median alpha	Median alpha	Median alpha	Median alpha
Weak	-0.42%	0.42%	0.53%	0.26%
Medium	0.04%	0.40%	-0.06%	0.18%
Good	0.74%	0.24%	0.44%	0.52%
Total	0.25%	0.40%	0.29%	0.30%

	Tercile of HHI			
Governance	Low	Median	High	Total
	Sd alpha	Sd alpha	Sd alpha	Sd alpha
Weak	0.02	0.02	0.03	0.02
Medium	0.02	0.02	0.03	0.02
Good	0.02	0.02	0.02	0.02
Total	0.02	0.02	0.03	0.02

	Tercile of HHI			
Governance	Low	Median	High	Total
	Count alpha	Count alpha	Count alpha	Count alpha
Weak	24.0	48.0	51.0	123.0
Medium	42.0	44.0	46.0	132.0
Good	39.0	18.0	31.0	88.0
Total	105.0	110.0	128.0	343.0

Table 4.9: Robustness check of Tobin Q regression

This table provides robustness check for Tobin Q regression of table 4. Regressions follow equation (4.7).

$$\Delta AdjQ_{ij,t:t+2} = \boldsymbol{\alpha}' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} + \boldsymbol{\lambda}' D\_Target[t]_{ijt} \times \mathbf{HHI}_{jt} \times Gindex_{ijt} + \gamma_1 HHI\_High_{jt} + \gamma_2 HHI\_Medium_{jt} + \\ \boldsymbol{\delta}' \mathbf{HHI}_{jt} \times Gindex_{ijt} + \mathbf{X'}_{ijt}\boldsymbol{\beta} + v_j + u_t + D\_Target[t]_{ijt} \times \iota_{fund} + \varepsilon_{ijt} \quad (4.9)$$

All settings are identical to the regression of column (1) in table 4 (**dependent variable** is difference of Tobin Q from year t to **t+2**, OLS regression) except for special illustration below. The robust check is designed as follows. Column (1): Use E-index instead of G-index; column (2): Use four-firm concentration ratio instead of HHI; column (3): Classify industries with SIC 3 digit code instead of Fama French 48 industries; column (4): "Horse race" test for HHI (i) – sort dividend yields into tercile, interact with G-index and D_Target[t] (in the same way as HHI), and add them as extra independent variables in the regression of equation (4.7); column (5): "Horse race" test for HHI (ii) – sort cash holding into tercile, interact with G-index and D_Target[t], and add them as extra independent variables in the regression of equation (4.7); column (6): "Horse race" test for HHI (iii) – sort institutional ownership into tercile, interact with G-index and D_Target[t], and add them as extra independent variables in the regression of equation (4.7); column (7): Use growth rate of Tobin Q, $\frac{Q_{t+2}-Q_t}{Q_t}$, as a new dependent variable in equation (4.7); and finally column (8): Sort HHI into quintile instead of tercile.

Robustness check design =	(1) *E-index*	(2) *CR–4*	(3) *SIC–3*	(4) *"Horse Race" Test* (i)	(5) *"Horse Race" Test* (ii)	(6) *"Horse Race" Test* (iii)	(7) *Growth Rate*	(8) *Quintile*
D_Target[t] × HHI_High	-0.177**	-0.409**	0.122	0.353	-0.721***	-0.452	-0.312***	-0.687***
	(0.0810)	(0.174)	(0.371)	(0.316)	(0.258)	(0.283)	(0.115)	(0.247)
D_Target[t] × HHI_Medium	0.0342	0.370	0.267	0.681	-0.328*	-0.0933	-0.0153	0.403
	(0.148)	(0.302)	(0.383)	(0.611)	(0.193)	(0.460)	(0.143)	(0.536)
D_Target[t] × HHI_Low	0.298***	1.260***	1.533***	1.773***	0.789*	1.159***	0.573***	1.684*
	(0.106)	(0.361)	(0.365)	(0.399)	(0.394)	(0.305)	(0.0908)	(0.873)
D_Target[t] × G(E)-index × HHI_High	0.0680*	0.0329*	-0.0127	-0.0373	0.0577**	0.0288	0.0315**	0.0627**
	(0.0395)	(0.0170)	(0.0337)	(0.0324)	(0.0254)	(0.0283)	(0.0129)	(0.0248)
D_Target[t] × G(E)-index × HHI_Medium	-0.0437	-0.0383	-0.0346	-0.0684	0.0250	-0.00135	-0.000835	-0.0425
	(0.0598)	(0.0269)	(0.0345)	(0.0610)	(0.0176)	(0.0430)	(0.0129)	(0.0466)
D_Target[t] × G(E)-index × HHI_Low	-0.139***	-0.137***	-0.167***	-0.179***	-0.0940**	-0.131***	-0.0618***	-0.186**
	(0.0503)	(0.0342)	(0.0370)	(0.0418)	(0.0366)	(0.0342)	(0.00829)	(0.0839)
Year fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Hedge fund fixed effect	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Standard error clustered	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*	*Industry*
Observations	15037	15037	15137	14051	14051	14051	15037	15037
Adjusted R^2	0.315	0.100	0.149	0.105	0.104	0.105	0.204	0.147

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.10: Success probability of activist hedge fund campaigns

This table shows regressions of success probability in activism campaigns. The sample contains 412 activism campaigns from 1994 to 2008 that can be merged with IRRC database. Observations only include activism campaigns with **determinate results**, i.e. whether the activist hedge fund achieves the main goals of campaign could be figured out. The **dependent variable** is dummy of whether the campaign is ultimately successful. Success of activism campaign is defined as activist hedge fund obtains their major goals in the end of the campaign. This definition is borrowed from Brav et al. [2008b]. If activist hedge fund has stated only one goal in SC 13D filings, then this goal is the major goal. If there are multiple goals stated in 13D, then the goals that force big changes to the company or that directly bring profits to hedge funds are deemed as major goals. Representative major goals are dividend distribution, share repurchase, sell company or part of its assets, business spinning off, against M&A and buyout firm. Appdendix 4.8 describes details regarding how to judge the success of campaign. In the table below, *Shares_hold* is the maximum share percentage acquired by hedge fund during the activism campaign; *Hostile* is dummy of whether the tactics of campaigns are hostile. Hostile tactics include (i) oust CEO, (ii) threat of proxy contest, (iii) proxy contest to replace the board, to rescind takeover bylaw or to take control, (iv) sue the company, (v) unsolicited offer to acquire the company. *Institution_QIX* is the percentage of institutional ownership by "quasi-index fund"; while *Institution_DED* is the percentage of institutional ownership by "dedicated investor". *Wolf-pack* is the dummy of whether there are multiple hedge funds collaborate in the campaign.

Dependent Variable =	**Dummy of Success in the Campaign**		
	(1)	(2)	(3)
Estimate Method =	*Probit*	*Probit*	*Probit*
GIM-Index	-0.0169*		
	(0.0100)		
HHI		-2.187	
		(1.544)	
G_Good × HHI_Low			0.225**
			(0.109)
G_Good × HHI_Medium			0.122
			(0.147)
G_Good × HHI_High			-0.260
			(0.339)
G_Medium × HHI_Low			0.168
			(0.275)
G_Medium × HHI_Medium			-0.469
			(0.315)
G_Medium × HHI_High			-0.447
			(0.337)
G_Weak × HHI_Low			-0.216
			(0.319)
G_Weak × HHI_Medium			-0.123
			(0.300)
Firm controls	*Yes*	*Yes*	*Yes*
Year fixed effect	*Yes*	*Yes*	*Yes*
Activist goal fixed effect	*Yes*	*Yes*	*Yes*
Activist fund fixed effect	*Yes*	*Yes*	*Yes*
Observations	164	164	164
Pseudo R^2	0.092	0.092	0.096

Table 4.11: Value creation conditional on the success probability

This table shows regressions of success probability in activism campaigns. The sample contains 412 activism campaigns from 1994 to 2008 that can be merged with IRRC database. Observations only include activism campaigns with **determinate results**, i.e. whether the activist hedge fund achieves the main goals of campaign could be figured out. The **dependent variable** is the value improvement conditional on campaign success. Success of activism campaign is defined as activist hedge fund obtains their major goals in the end of the campaign. This definition is borrowed from Brav et al. [2008b]. If activist hedge fund has stated only one goal in SC 13D filings, then this goal is the major goal. If there are multiple goals stated in 13D, then the goals that force big changes to the company or that directly bring profits to hedge funds are deemed as major goals. Representative major goals are dividend distribution, share repurchase, sell company or part of its assets, business spinning off, against M&A and buyout firm. Appdendix 4.8 describes details regarding how to judge the success of campaign. In the table below, *Shares_hold* is the maximum share percentage acquired by hedge fund during the activism campaign; *Hostile* is dummy of whether the tactics of campaigns are hostile. Hostile tactics include (i) oust CEO, (ii) threat of proxy contest, (iii) proxy contest to replace the board, to rescind takeover bylaw or to take control, (iv) sue the company, (v) unsolicited offer to acquire the company. *Institution_QIX* is the percentage of institutional ownership by "quasi-index fund"; while *Institution_DED* is the percentage of institutional ownership by "dedicated investor". *Wolf-pack* is the dummy of whether there are multiple hedge funds collaborate in the campaign.

Dependent Variable =	**Value creation conditional on success**		
	(1)	(2)	(3)
Estimate Method =	*OLS*	*OLS*	*OLS*
GIM-Index	0.0175		
	(0.0322)		
HHI		1.034	
		(2.639)	
G_Good × HHI_Low			0.381
			(0.409)
G_Good × HHI_Medium			-0.137
			(0.411)
G_Medium × HHI_Low			-0.174
			(0.324)
G_Medium × HHI_Medium			-0.0469
			(0.416)
G_Medium × HHI_High			0.179
			(0.423)
G_Weak × HHI_Low			-0.0171
			(0.373)
G_Weak × HHI_Medium			0.301
			(0.361)
G_Weak × HHI_High			0.485
			(0.380)
Firm controls	*Yes*	*Yes*	*Yes*
Year fixed effect	*Yes*	*Yes*	*Yes*
Activist goal fixed effect	*Yes*	*Yes*	*Yes*
Activist fund fixed effect	*Yes*	*Yes*	*Yes*
Observations	96	96	96
Adj R^2	0.095	0.095	0.095

Bibliography

Ronald H Coase. The Nature of the Firm. *The Nature of the Firm. Origins, Evolution, and Development. New York, Oxford*, 18:33, 1937.

Oliver E. Williamson. *The Economic Institutions of Capitalism: Firms, Markets, Relational Contracting*. The Free Press, 1985.

Sanford J Grossman and Oliver D. Hart. The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration. *Journal of Political Economy*, 94(4):691–719, 1986.

Oliver Hart and John Moore. Property Rights and the Nature of the Firm. *Journal of Political Economy*, 98(6):1119–1158, 1990.

Oliver Hart and Bengt Holmstrom. A Theory of Firm Scope. *The quarterly journal of economics*, 125 (2):483–513, 2010.

Gerard Hoberg and Gordon Phillips. Conglomerate Industry Choice and Product Language. *Management Science*, 64(8):3735–3755, 2018.

Jan Bena and Kai Li. Corporate Innovations and Mergers and Acquisitions. *The Journal of Finance*, 69(5):1923–1960, 2014.

Amit Seru. Firm Boundaries Matter: Evidence from Conglomerates and R&D Activity. *Journal of Financial Economics*, 111(2):381 – 405, 2014. ISSN 0304-405X. doi: https://doi.org/10.1016/j.jfineco.2013.11.001. URL http://www.sciencedirect.com/science/article/pii/S0304405X1300278X.

Laurent Frésard, Gerard Hoberg, and Gordon M. Phillips. Innovation Activities and Integration through Vertical Acquisitions. *The Review of Financial Studies*, 33(7):2937–2976, 2020.

Thomas Keil, Erkko Autio, and Gerard George. Corporate Venture Capital, Disembodied Experimentation and Capability Development. *Journal of Management Studies*, 45(8):1475–1505, 2008.

William R Kerr, Ramana Nanda, and Matthew Rhodes-Kropf. Entrepreneurship as Experimentation. *Journal of Economic Perspectives*, 28(3):25–48, 2014.

Michael Ewens, Ramana Nanda, and Matthew Rhodes-Kropf. Cost of Experimentation and the Evolution of Venture Capital. *Journal of Financial Economics*, 128(3):422–442, 2018.

Daniel McFadden. Conditional Logit Analysis of Qualitative Choice Behavior. 1973.

Thomas J. Chemmanur, Elena Loutskina, and Xuan Tian. Corporate Venture Capital, Value Creation, and Innovation. *The Review of Financial Studies*, 27(8):2434–2473, 05 2014. ISSN 0893-9454. doi: 10.1093/rfs/hhu033. URL https://doi.org/10.1093/rfs/hhu033.

Song Ma. The Life Cycle of Corporate Venture Capital. *The Review of Financial Studies*, 04 2020. ISSN 0893-9454. doi: 10.1093/rfs/hhz042. URL https://doi.org/10.1093/rfs/hhz042.

Michel Ferrary. Syndication of Venture Capital Investment: The Art of Resource Pooling. *Entrepreneurship Theory and Practice*, 34(5):885–908, 2010.

Yael V. Hochberg, Alexander Ljungqvist, and Yang Lu. Whom You Know Matters: Venture Capital Networks and Investment Performance. *The Journal of Finance*, 62(1):251–301, 2007. doi: 10.1111/j.1540-6261.2007.01207.x. URL https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1540-6261.2007.01207.x.

Yael V. Hochberg, Alexander Ljungqvist, and Yang Lu. Networking as a Barrier to Entry and the Competitive Supply of Venture Capital. *The Journal of Finance*, 65(3):829–859, 2010. doi: 10.1111/j.1540-6261.2010.01554.x. URL https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1540-6261.2010.01554.x.

Thomas Keil, Markku V.J. Maula, and Cameron Wilson. Unique Resources of Corporate Venture Capitalists as a Key to Entry into Rigid Venture Capital Syndication Networks. *Entrepreneurship Theory and Practice*, 34(1):83–103, 2010.

I. MacMillan, E. Roberts, Val Livada, and A. Wang. *Corporate Venture Capital (CVC) Seeking Innovation and Strategic Growth*. National Institute of Standards and Technology, 2008.

Xavier Gabaix and Ralph S.J. Koijen. Granular Instrumental Variables. Technical report, National Bureau of Economic Research, 2020.

Shai Bernstein, Xavier Giroud, and Richard R. Townsend. The Impact of Venture Capital Monitoring. *The Journal of Finance*, 71(4):1591–1622, 2016.

David J. Teece. Economies of Scope and the Scope of the Enterprise. *Journal of Economic Behavior & Organization*, 1(3):223–247, 1980.

John C. Panzar and Robert D. Willig. Economies of Scope. *The American Economic Review*, 71(2):268–272, 1981.

Larry H.P. Lang and Rene M. Stulz. Tobin's Q, Corporate Diversification, and Firm Performance. *Journal of political economy*, 102(6):1248–1280, 1994.

Philip G. Berger and Eli Ofek. Diversification's Effect on Firm Value. *Journal of Financial Economics*, 37(1):39 – 65, 1995. ISSN 0304-405X. doi: https://doi.org/10.1016/0304-405X(94)00798-6. URL http://www.sciencedirect.com/science/article/pii/0304405X94007986.

Owen Lamont. Cash Flow and Investment: Evidence from Internal Capital Markets. *The Journal of Finance*, 52(1):83–109, 1997.

Gerard Hoberg and Gordon M. Phillips. Scope, Scale and Competition: The 21st Century Firm. *Available at SSRN*, 2021.

Gustavo Manso. Experimentation and the Returns to Entrepreneurship. *The Review of Financial Studies*, 29(9):2319–2340, 09 2016. ISSN 0893-9454. doi: 10.1093/rfs/hhw019. URL https://doi.org/10.1093/rfs/hhw019.

Paul Gompers and Josh Lerner. Money Chasing Deals? The Impact of Fund Inflows on Private Equity Valuation. *Journal of financial economics*, 55(2):281–325, 2000.

Thomas Hellmann. A Theory of Strategic Venture Investing. *Journal of Financial Economics*, 64(2): 285 – 314, 2002. ISSN 0304-405X. doi: https://doi.org/10.1016/S0304-405X(02)00078-8. URL http://www.sciencedirect.com/science/article/pii/S0304405X02000788.

Paolo Fulghieri and Merih Sevilir. Organization and Financing of Innovation, and the Choice between Corporate and Independent Venture Capital. *Journal of Financial and Quantitative Analysis*, 44(6):1291–1321, 2009.

Keongtae Kim, Anandasivam Gopal, and Gerard Hoberg. Does Product Market Competition Drive CVC Investment? Evidence from the U.S. IT Industry. *Information Systems Research*, 27 (2):259–281, 2016.

Sandip Basu, Corey Phelps, and Suresh Kotha. Towards Understanding Who Makes Corporate Venture Capital Investments and Why. *Journal of Business Venturing*, 26(2):153–171, 2011.

Gary Dushnitsky and Michael J. Lenox. When do Firms Undertake R&D by Investing in New Ventures? *Strategic Management Journal*, 26(10):947–965, 2005.

Xuan Tian and Kailei Ye. A Dark Side of Corporate Venture Capital. *Available at SSRN*, 2018.

Hongyu Shan. Corporate Venture Capital and the Boundaries of the Firm. In *2nd Emerging Trends in Entrepreneurial Finance Conference*, 2019.

Gary Dushnitsky. Corporate Venture Capital in the Twenty-First Century: an Integral Part of Firms' Innovation Toolkit. *The Oxford Handbook of Venture Capital*, 09 2012. URL https://www.oxfordhandbooks.com/view/10.1093/oxfordhb/9780195391596.001.0001/oxfordhb-9780195391596-e-6.

Robin Siegel, Eric Siegel, and Ian C. MacMillan. Corporate Venture Capitalists: Autonomy, Obstacles, and Performance. *Journal of Business Venturing*, 3(3):233 – 247, 1988. ISSN 0883-9026. doi: https://doi.org/10.1016/0883-9026(88)90017-1. URL http://www.sciencedirect.com/science/article/pii/0883902688900171.

Henry W. Chesbrough. Making Sense of Corporate Venture Capital. *Harvard Business Review*, 80 (3):90–99, 2002.

Terry E Winters and Donald L. Murfin. Venture Capital Investing for Corporate Development Objectives. *Journal of Business Venturing*, 3(3):207–222, 1988.

Hollister B. Sykes. Corporate Venture Capital: Strategies for Success. *Journal of Business Venturing*, 5(1):37–47, 1990. ISSN 0883-9026. doi: https://doi.org/10.1016/0883-9026(90)90025-O. URL https://www.sciencedirect.com/science/article/pii/088390269090025O.

Kevin McNally. *Corporate Venture Capital: Bridging the Equity Gap in the Small Business Sector*. Routledge, 1997.

Ernst and Young. Global Corporate Venture Capital Report. 2009.

Dirk Bergemann and Ulrich Hege. The Financing of Innovation: Learning and Stopping. *RAND Journal of Economics*, pages 719–752, 2005.

Joshua Lerner. *The Architecture of Innovation: The Economics of Creative Organizations*. Harvard Business Press, 2012.

Thomas Hellmann, Laura Lindsey, and Manju Puri. Building Relationships Early: Banks in Venture Capital. *The Review of Financial Studies*, 21(2):513–541, 12 2008. ISSN 0893-9454. doi: 10.1093/rfs/hhm080. URL https://doi.org/10.1093/rfs/hhm080.

Gerard Hoberg and Gordon Phillips. Text-Based Network Industries and Endogenous Product Differentiation. *Journal of Political Economy*, 124(5):1423–1465, 2016.

Gregor Matvos, Amit Seru, and Rui C. Silva. Financial Market Frictions and Diversification. *Journal of Financial Economics*, 127(1):21–50, 2018.

Jarrad Harford. What Drives Merger Waves? *Journal of Financial Economics*, 77(3):529 – 560, 2005a. ISSN 0304-405X. doi: https://doi.org/10.1016/j.jfineco.2004.05.004. URL http://www.sciencedirect.com/science/article/pii/S0304405X04002041.

Vojislav Maksimovic and Gordon Phillips. The Industry Life Cycle, Acquisitions and Investment: Does Firm Organization Matter? *The Journal of Finance*, 63(2):673–708, 2008.

Paul A. Gompers and Josh Lerner. What Drives Venture Capital Fundraising? Technical report, National bureau of economic research, 1998.

Gerard Hoberg, Gordon Phillips, and Nagpurnanand Prabhala. Product Market Threats, Payouts, and Financial Flexibility. *The Journal of Finance*, 69(1):293–324, 2014.

Gary Dushnitsky and Michael J. Lenox. When does Corporate Venture Capital Investment Create Firm Value? *Journal of Business Venturing*, 21(6):753 – 772, 2006. ISSN 0883-9026. doi: https://doi.org/10.1016/j.jbusvent.2005.04.012. URL http://www.sciencedirect.com/science/article/pii/S0883902605000583.

Alon Brav, Wei Jiang, and Hyunseob Kim. The real effects of hedge fund activism: Productivity, asset allocation, and labor outcomes. *Review of Financial Studies*, 28(10):2723–2769, 2015a.

Joshua Coval and Erik Stafford. Asset fire sales (and purchases) in equity markets. *Journal of Financial Economics*, 86(2):479–512, 2007.

Alex Edmans, Itay Goldstein, and Wei Jiang. The real effects of financial markets: The impact of prices on takeovers. *The Journal of Finance*, 67(3):933–971, 2012.

Nickolay Gantchev, Oleg Gredil, and Chotibhak Jotikasthira. Governance under the gun: Spillover effects of hedge fund activism. SSRN Working Paper, 2017.

Felix Zhiyu Feng, Qiping Xu, and Heqing Zhu. Caught in the cross-fire: How the threat of hedge fund activism affects creditors. Working Paper, 2017.

Jarrad Harford. What drives merger waves? *Journal of Financial Economics*, 77(3):529–560, 2005b.

Mark Mitchell and Harold Mulherin. The impact of industry shocks on takeover and restructuring activity. *Journal of Financial Economics*, 41(2):193–229, 1996.

Gregor Andrade and Erik Stafford. Investigating the economic role of mergers. *Journal of Corporate Finance*, 10:1 – 36, 2004.

Gennaro Bernile, Evgeny Lyandres, and Alexei Zhdanov. A theory of strategic mergers. *Review of Finance*, 16:517 – 575, 2012.

Nicole M Boyson, Nickolay Gantchev, and Anil Shivdasani. Activism mergers. *Journal of Financial Economics*, 126(1):54–73, 2017.

Hadiye Aslan and Praveen Kumar. The product market effects of hedge fund activism. *Journal of Financial Economics*, 119(1):226–248, 2016.

Sara B Moeller, Frederik P Schlingemann, and René M Stulz. Firm size and the gains from acquisitions. *Journal of financial economics*, 73(2):201–228, 2004.

Matthew R. Denes, Jonathan M. Karpoff, and Victoria B. McWilliams. Thirty years of shareholder acitvism: A survey of empirical research. *Journal of Corporate Finance*, 44:405–424, 2017.

Alon Brav, Wei Jiang, and Hyunseob Kim. Recent advances in research on hedge fund activism: Value creation and identification. *Annual Review of Financial Economics*, 7:579–595, 2015b.

Marco Becht, Julian Franks, Jeremy Grant, and Hannes F Wagner. Returns to hedge fund activism: An international study. *The Review of Financial Studies*, 30(9):2933–2971, 2017.

Alon Brav, Wei Jiang, Frank Partnoy, and Randall Thomas. Hedge fund activism, corporate governance, and firm performance. *The Journal of Finance*, 63(4):1729–1775, 2008a.

Robin Greenwood and Michael Schor. Investor activism and takeovers. *Journal of Financial Economics*, 92(3):362–375, 2009.

Lucian Bebchuk, Alon Brav, and Wei Jiang. Long-term effects of hedge funds activism. *Columbia Law Review*, 115:1085 – 1155, 2015a.

Ed deHaan, David F Larcker, and Charles McClure. Long-term economic consequences of hedge fund activist interventions. *Rock Center for Corporate Governance at Stanford University Working Paper*, (236), 2018.

Jillian Grennan. A corporate culture channel: How increased shareholder governance reduces firm value. SSRN Working Paper, available at https://ssrn.com/abstract=2345384, 2014.

Alon Brav, Wei Jiang, Song Ma, and Xuan Tian. How does hedge fund activism reshape corporate innovation? *Journal of Financial Economics*, 130:237–264, 2018.

Mike Burkart and Samuel Lee. Activism and takeovers. ECGI Finance Working Paper 543/2018, 2018.

Adrian Aycan Corum and Doron Levit. Corporate control activism. Working Paper, 2017.

Nickolay Gantchev, Merih Sevilir, and Anil Shivdasani. Activism and empire building. ECGI Finance Working Paper 575/2018, 2018.

Sanford J. Grossman and Oliver D. Hart. Takeover bids, the free-rider problem and the theory of the corporation. *Bell Journal of Economics*, 11:42–64, 1980.

Marianne Bertrand and Sendhil Mullainathan. Enjoying the quiet life? corporate governance and managerial preferences. *Journal of Political Economy*, 111(5):1043–1075, 2003a.

Alex Edmans and Gustavo Manso. Governance through trading and intervention: A theory of multiple blockholders. *The Review of Financial Studies*, 24(7):2395–2428, 2011.

Vyacheslav Fos and Charles M. Kahn. Governance through threats of intervention and exit. SSRN Working Paper, available at SSRN: https://ssrn.com/abstract=2527710, 2016.

Thomas Bourveau and Jordan Schoenfeld. Shareholder activism and voluntary disclosure. *Review of Accounting Studies*, 22(3):1307–1339, 2017.

Andrei Shleifer and Robert Vishny. Liquidation values and debt capacity: A market equilibrium approach. *Journal of Finance*, 47(4):1343–1366, 1992.

Andrei Shleifer and Robert Vishny. Fire sales in finance and macroeconomics. *Journal of Economic Perspectives*, 25(1):29–48, 2011.

Frederik P. Schlingemann, Rene M. Stulz, and Ralph A. Walkling. Divestitures and the liquidity of the market for corporate assets. *Journal of Financial Economics*, 64(1):117–144, 2002.

Hernán Ortiz-Molina and Gordon M Phillips. Real asset illiquidity and the cost of capital. *Journal of Financial and Quantitative Analysis*, 49(1):1–32, 2014.

Hyunseob Kim and Howard Kung. The asset redeployability channel: How uncertainty affects corporate investment. *The Review of Financial Studies*, 30(1):245–280, 2017.

Boyan Jovanovic and P Rousseau. The q-theory of mergers. *American Economic Review*, 92(2): 198–204, 2002.

Vojislav Maksimovic and Gordon Phillips. The market for corporate assets: Who engages in mergers and asset sales and are there efficiency gains? *Journal of Finance*, 56(6):2019–2065, 2001.

Michael Jensen. Agency costs of free cash flow, corporate finance, and takeovers. *American Economic Review*, 76:323–329, 1986.

Randall Morck, Andrei Shleifer, and Robert Vishny. Do managerial motives drive bad acquisitions. *Journal of Finance*, 45:31–48, 1990.

Mark Mitchell and Kenneth Lehn. Do bad bidders become good targets? *Journal of Political Economy*, 98(2):372 – 398, 1990.

Gregor Andrade, Mark Mitchell, and Erik Stafford. New evidence and perspectives on mergers. *Journal of Economic Perspectives*, 15:103 – 120, 2001.

Ronald W. Masulis, Cong Wang, and Fei Xie. Corporate governance and acquirer returns. *Journal of Finance*, 62(4):1851 – 1889, 2007.

Gary Gorton, Matthias Kahl, and Richard R. Rosen. Eat or be eaten: A theory of mergers and firm size. *Journal of Finance*, 54(3):1291 – 1344, 2009.

April Klein and Emanuel Zur. Entrepreneurial shareholder activism: Hedge funds and other private investors. *The Journal of Finance*, 64(1):187–229, 2009.

Craig Doidge, Kathleen M Kahle, G Andrew Karolyi, and René M Stulz. Eclipse of the public corporation or eclipse of the public markets? *Journal of Applied Corporate Finance*, 30(1):8–16, 2018.

Kathleen Kahle and Ralph Walkling. The impact of industry classifications on financial research. *Journal of Financial and Quantitative Analysis*, 31(3):309–335, 1996.

Nickolay Gantchev, Merih Sevilir, and Anil Shivdasani. Activism and empire building. *Journal of Financial Economics*, 138(2):526–548, 2020.

Xavier Giroud and Holger M Mueller. Does corporate governance matter in competitive industries? *Journal of Financial Economics*, 95(3):312–331, 2010.

Xavier Giroud and Holger M. Mueller. Corporate governance, product market competition, and equity prices. *The Journal of Finance*, 66(2):563–600, 2011.

Paul Gompers, Joy Ishii, and Andrew Metrick. Corporate governance and equity prices. *The Quarterly Journal of Economics*, 118(1):107–156, 2003.

Nickolay Gantchev. The costs of shareholder activism: Evidence from a sequential decision model. *Journal of Financial Economics*, 107(3):610–631, 2013.

Marco Becht, J Franks, Jeremy Grant, and H Wagner. The returns to hedge fund activism: An international study. european corporate governance institute finance working paper 402/2014, 2015.

Felix Zhiyu Feng, Qiping Xu, and Heqing Zhu. Caught in the cross-fire: How the threat of hedge fund activism affects creditors. 2016.

Nickolay Gantchev, Oleg Gredil, and Chotibhak Jotikasthira. Governance under the gun: Spillover effects of hedge fund activism. *SSRN Working Paper*, 2016.

Ian R Appel, Todd A Gormley, and Donald B Keim. Standing on the shoulders of giants: The effect of passive investors on activism. Technical report, National Bureau of Economic Research, 2016.

Alon Brav, Wei Jiang, Frank Partnoy, and Randall Thomas. Hedge fund activism, corporate governance, and firm performance. *The Journal of Finance*, 63(4):1729–1775, 2008b.

Oliver D Hart. The market mechanism as an incentive scheme. *The Bell Journal of Economics*, pages 366–382, 1983.

Brian J Bushee. Do institutional investors prefer near-term earnings over long-run value? *Contemporary Accounting Research*, 18(2):207–246, 2001.

Cornelius Schmidt and Rudiger Fahlenbrach. Do exogenous changes in passive institutional ownership affect corporate governance and firm value? *Journal of Financial Economics*, 124(2): 285 – 306, 2017. ISSN 0304-405X. doi: http://dx.doi.org/10.1016/j.jfineco.2017.01.005. URL http://www.sciencedirect.com/science/article/pii/S0304405X17300053.

Alon Brav, Wei Jiang, and Hyunseob Kim. The real effects of hedge fund activism: Productivity, asset allocation, and labor outcomes. *Review of Financial Studies*, 28(10):2723–2769, 2015c.

Stephen Nickell, Daphne Nicolitsas, and Neil Dryden. What makes firms perform well? *European Economic Review*, 41(3):783 – 796, 1997. ISSN 0014-2921. doi: https://doi.org/10.1016/S0014-2921(97)00037-8. URL http://www.sciencedirect.com/science/article/pii/S0014292197000378. Paper and Proceedings of the Eleventh Annual Congress of the European Economic Association.

Marianne Bertrand and Sendhil Mullainathan. Enjoying the quiet life? corporate governance and managerial preferences. *Journal of Political Economy*, 111(5):1043–1075, 2003b.

Robert Daines. The incorporation choices of ipo firms. *NYUL Rev.*, 77:1559, 2002.

Lucian A. Bebchuk, Alon Brav, and Wei Jiang. The long-term effects of hedge fund activism. *Columbia Law Review*, 115(5):1085–1155, 2015b. ISSN 00101958. URL http://www.jstor.org/stable/43582424.