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Three Essays on Financial Economics

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Three Essays on Financial Economics

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An abstract of
A dissertation submitted to the Faculty of the
James T. Laney School of Graduate Studies of Emory University
in partial fulfillment of the requirements for the degree of
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2019

Abstract

Three Essays on Financial Economics

By Cong (Roman) Wang

This dissertation contains three essays on financial economics. The first paper (Information Acquisition of Institutional Investors: Implications for Institutional Herding) studies the extent and implication of institutional investors acquiring holdings information of other institutional investors. Using a novel data on institutional investors' access of 13-F filings, I provide the first direct evidence of institutional investors seeking institutional holding information. Surprisingly, institutional investors follow the institutional crowd, but trade against other institutional investors whose holding information was acquired. This 13F-contrarian strategy manifests strongly in a sell-buy relationship and is warranted by abnormal stock returns. I find evidence consistent with institutional investors use 13-F filings to identify stocks experienced institutional selling price pressure. The second paper (Public Market Players in the Private World: Implications for the Going-Public Process) studies a new trend in the private financial market. Recent years have seen a dramatic increase in investment by public market institutional investors in startups. We study the economic consequences of these investments for the initial public offerings of startups. We find that institutions' pre-IPO participation is associated with lower IPO underpricing for VC-backed startups. Our further analysis shows that the reduction in IPO underpricing does not appear to be driven by endogenous matching between startups and institutions. We explore the underlying economic mechanisms, and our results are consistent with a substitution effect between institutions and all-star analysts. The third paper (CEO vs. Consumer Confidence: Investment, Financing, and Firm Performance) examines to what degree corporate managers take cues for investors. Using similarly constructed measures of CEO optimism and consumer optimism, our analysis provides evidence that, holding CEO optimism constant, CEOs substantially increase their capital expenditures and net financing when investors are more optimistic. CEOs, however, trade against investor optimism in their own personal trading accounts. And, while CEO optimism positively predicts firm performance, investor optimism negatively predicts firm performance and subsequent earnings surprises. Taken together, our findings suggest that investor beliefs strongly affect corporate investment; in particular, it appears that better-informed managers sometimes succumb to investor pressure or use times of high investor optimism to empire build.

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To my grandpa. May flights of angels sing thee to thy rest.

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Information Acquisition of Institutional Investors: Implications for Institutional Herding *

Cong (Roman) Wang †

Abstract

This paper studies the extent and implication of institutional investors acquiring holdings information of other institutional investors. Using a novel data on institutional investors' access of 13-F filings, I provide the first direct evidence of institutional investors seeking institutional holding information. Surprisingly, institutional investors follow the institutional crowd, but trade against other institutional investors whose holding information was acquired. This 13F-contrarian strategy manifests strongly in a sell-buy relationship and is warrantied by abnormal stock returns. I find evidence consistent with institutional investors use 13-F filings to identify stocks experienced institutional selling price pressure.

Keywords: Institutional Investor, Information Acquisition, Institutional Herding, EDGAR

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1 Introduction

Long history of research on institutional investors show that institutional investors have the tendency to go into and out of securities together (Lakonishok, Shleifer, and Vishny (1992), Wermers (1999), Sias (2004)). Theoretical studies on this herding behavior provides many potential reasons why individual institutional investors tend to follow the institutional crowd. One strand of theories suggests that institutional herding is an unintentional results of investors making their own investment decisions that are correlated (Froot, Scharfstein, and Stein (1993), Hirshleifer, Subrahmanyam, and Titman (1994)). Another strand of literature argues that institutional investors intentionally copy investment decision of others (Scharfstein and Stein (1990), Banerjee (1992), Bikhchandani, Hirshleifer, and Welch (1992)). Empirically, previous studies mostly rely on institutional holdings and offers a mixed view (Choi and Sias (2009), Falkenstein (1996), Sias (2004)). This paper attempts to empirically disentangle potential reasons of institutional herding (unintentional v.s. intentional) by identifying information acquisition activities where one institutional investor acquire the holdings information of other institutional investors.

In this paper, I attempt to answer three questions. First, I examine the extent to which institutional investors acquire information about the holdings of other institutional investors. Second, I investigate how the acquired holdings information affects the institutional trading behavior. Last, I study the incentive behind acquiring and using holding information of other institutional investors. Specifically, I examine whether such 13F-based trading is warranted by subsequent returns and explore possible ways that institutional investors use holdings information of their peers.

As researchers, we usually do not observe information acquisition activities of institutional investors. Thus, it is difficult to understand whether institutional investors acquire holdings information of their peers and how they make herding decisions based on peers' holdings information. I overcome this obstacle by using a novel dataset containing historical records of viewing activities all accesses of 13-F Holdings Reports via the SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) server.[1] This dataset allows me to pinpoint specific information acquisition activities of institutional investors. By "unmasking" the anonymized Internet Protocol (IP) addresses, I am able to uncover institutional investors who acquire holdings information (henceforth, viewing institutional investors) and institutional investors whose holdings information is acquired (henceforth, filing institutional investors).

[1] In this paper, I use "view", "download" and "access" interchangeably.

First, I document stylized facts about the extent to which institutional investors acquire information about holdings of other institutional investors. I uncover 55,286 instances where one institutional investor accesses the 13-F filings of other institutional investors on the EDGAR server from the third quarter of 2006 to the last quarter of 2016. 13-F access activities grow over time. For example, from the last quarter of 2006 to the last quarter of 2016, quarterly 13-F accesses increased from 533 to 2,151. Forty percent of all downloads occurred within three days after 13-F filings are available. This is consistent with institutional investors actively seeking holdings information via 13-F filings. Over the entire sample period, 485 unique viewing institutional investors accessed 13-F filings from 3,375 filing institutional investors. On average, 114 unique viewing institutional investors accessed 13-F filings from 545 filing institutional investors per quarter. Compared to filing institutional investors, viewing institutional investors manage more assets and hold a larger number of stocks, but are similar in terms of performance.

For a given viewing institutional investor, the source of information about institutional holdings is persistent over time. For example, 13-F access from a filing institutional investor in the previous quarter predicts a 24.6 percent higher likelihood of accessing 13-F filings from the same institutional investor in the next quarter. This persistence is economically meaningful, considering that the unconditional probability of 13-F access of a given viewing-filing pair is only 0.11 percent. Institutional characteristics also affects the choice of information source. For example, institutional investors who manage more assets and trade more frequently have a higher likelihood of being accessed. Furthermore, institutional investors who hold winner stocks, high turnover stocks, and more volatile stocks are more likely to be viewed.

Second, I examine the implication of 13-F access on institutional trading. In particular, I study how the trading behavior of a viewing institutional investor are affected by the trades of institutional investors whose holding information was accessed. If institutional herding is an unintentional results of independent institutional investment decisions, the trades of filing institutional investors should not positively predict viewing institutional trading behavior. On the other hand, if institutional investors indeed intentionally copy investment decision of others, we should expect positive predictability of filing institutional trades on viewing institutional trades.

For each stock held by a viewing institutional investors, I measure the change in the number of shares held (*Viewing Trade*), scaled by the total number of shares outstanding. I calculate the same measure for filing institutional investors in the previous quarter (*Filing Trade*). Since each viewing institutional investor can access the 13-F filings from multiple filing institutional investors, I average across all viewed filing institutional investors, weighted equally or by total asset under management. Intuitively, *Filing Trade* measures the information that is acquired by a viewing institutional investor and *Viewing Trade* measures the corresponding reaction.

I run a stock-viewer-quarter level predictive regression of *Viewing Trade* on *Filing Trade*, controlling for stock lagged characteristics, lagged and contemporaneous aggregate institutional trade and quarter-viewer-industry fixed effects. Surprisingly, viewing institutional investors trade against other institutional investors whose holding information was acquired. That is, *Filing Trade* negatively predicts *Viewing Trade*. This 13F- contrarian strategy is statistically significant at the one percent level. Economically, a one standard deviation increase in filing institutional trades moves the median viewing trade to the 41^{st} percentile. The statistical and economical significance is robust to the inclusion of stock characteristics, aggregate institutional trades, and quarter-viewer-industry fixed effects. Consistent with the herding literature, I find that viewing institutional investors do herd with the institutional crowd. That is, lagged and contemporaneous aggregate institutional trades are positively associated with *Viewing Trade*. When comparing the effect of *Filing Trade* and aggregate institutional trades on *Viewing Trade*, the effect of a one standard deviation increase in *Filing Trade* is one eighth of the effect of a one standard deviation decrease in aggregate institutional trades.

To ensure the previous results are not caused by large trades from large viewing institutional investors, I use dummy variables indicating the direction of institutional investors and repeat the previous analysis. I find that when filing institutional investors increase shares held of a stock, viewing institutional investors have a higher probability to decrease shares held of the same stock. Combined with the previous results, I find that institutional investors herd with the aggregate of institutional investors but trade against a subset of institutional investors whose holdings information is acquired. The fact the institutional investors herd with the institutional crowd, but follow a 13F- contrarian trading strategy with respect to peers whose holdings information is acquired is more consistent with institutional investors unintentionally herd with the crowd, but less consistent with institutional investors copy each others' investment decisions.

To further understand the relationship between *Viewing Trade* and *Filing Trade*, I conduct subsample analyses. Specifically, I split the previous sample into two subsamples: Filing Buy and Filing Sell. For a viewing institutional investor, if a stock experienced an increase in shares held from corresponding filings institutional investors, this stock is put in the Filing Buy subsample. On the other hand, if a stock experienced an decrease in shares held from corresponding filings institutional investors, this stock is put in the Filing Sell subsample. I find that the 13F-contrarian strategy manifests more in a sell-buy relationship. In other words, when trading against filing institutional investors, viewing institutional investors predominantly purchase the stocks that were sold by the filing institutional investors in the previous quarter. This is could be driven by the fact that many institutional investors face short-sell constraints. At the same time, it is also possible that viewing institutional investors indeed take short positions, but are not required to report on 13-F filings.

The previous results are consistent with viewing institutional investors acquire information about filing institutional investors holdings and use such information to make trading decisions. I explore two alternative explanations that are seemingly consistent with the previous findings. First, I examine the possibility that the previous findings of negative relationship between *Viewing Trade* and *Filing Trade* is mechanically driven. That is, instead of viewing institutional investors acquiring and trading on information about the holdings of other institutional investors, the trading behaviors of viewing and filing institutional investors could be mechanically negatively correlated. The act of accessing 13-F filings merely coincides their pre-determined trading strategies, but do not affect their decision making process. To address this concern, I conduct a placebo test using the propensity score matching methodology. I match each *viewed* filing institutional investor with a *placebo* filing institutional investor who has the closest propensity score of being viewed by the same institutional investor. If the previously documented contrarian relationship is mechanical, then the viewing and placebo filing institutional trades also should be negatively correlated. Using the placebo sample, I find no significant relationship between viewing and placebo filing institutional trades, which is inconsistent with the hypothesis that the contrarian trading strategy is mechanical.

Another concern is that viewing institutional investors make trading decisions based on common information that filing institutional investors also observes. Controlling for the contemporaneous aggregate institutional trades mitigates this concern to the extent that the common information is observed by all institutional investors. However, this does not shield the previous results from being driven by common information received only by the viewing-filing institutional pairs. To address this issue, I control the contemporaneous filing institutional trades in the previous regressions. I find that the negative relationship between filing and viewing institutional trades is robust and stronger after controlling for contemporaneous filing institutional trades.

Finally, given that viewing institutional investors acquire holdings information of filing institutional investors, and use such information to trade against filing institutional investors, a natural question to ask is what are the incentives to do so. I begin by examining whether such 13F-contrarian trading strategy is warranted by subsequent returns. Focusing on Filing Sell subsample, I categorize each stock as a 13F-contrarian stock or a 13F-confirmation stock. If a stock's filing institutional trade at quarter $t-1$ negatively (positively) predicts viewing institutional trades at quarter t , this stock is deemed to be a 13F-contrarian (13F-confirmation) stock. I form 13F-contrarian (13F-confirmation) portfolios by buying 13F-contrarian (13F-confirmation) stocks at the end of quarter t and hold the portfolio throughout quarter $t+1$. On average, the 13F-contrarian portfolio earns 0.91 percent quarterly Carhart alpha and 0.66 percent quarterly Fama-French five-factor alpha, significant at the one and five percent levels, respectively. This profitability is stronger for small, low volume, and illiquid stocks. In contrast, the 13F-confirmation portfolio does not earn significant returns.

Next, I explore possible ways that viewing institutional investors use holdings information of their peers.

Since the 13F-contrarian strategy is stronger in the Filing Sell subsample, it is possible that viewing institutional investors use 13-F filing to identify stocks that are sold by filing institutional investors. Coval and Stafford (2007) show that institutional fire sales lead to negative price pressures that last for more than 12 months. Therefore, filing institutional investors might exert a downward price pressure and the positive abnormal in quarter $t+1$ could be the resultant return reversal. If so, we should observe negative abnormal returns prior to portfolio formation. Similar to the previous analysis, the negative price pressure should be concentrated in small, low volume, and illiquid stocks. To this end, I calculated the 13F-contrarian portfolio returns during two quarters prior to portfolio formation. I find significant negative abnormal returns for small and low volume stocks. This finding is consistent with viewing institutional investors profiting from the return reversal following filing institutional investor sales.

A reminding questions is that how do viewing institutional investors decide what filing sales to trade against? If a filing sale is driven by unfavorable information about the stock, the stock price is unlikely to rebound due to price pressure. On the other hand, if a filing sales is not driven by negative information (e.g. driven by liquidity reasons), the stock price is likely to rebound. Do viewing institutional investors blindly take contrary positions of all filings sales or do they conduct more in-depth research. If viewing institutional investors exercise due diligence, we should expect that viewing institutional investors acquire more information about stocks that appear in 13-F filings. I examine the EDGAR activities of viewing institutional investors after 13-F searches. Consistent with this hypothesis, I find that when a viewing institutional investor observes a stock via 13-F filings, this institutional investor is 31.9 bps more likely to access firm disclosures from the same stock during the next week. As a benchmark, the unconditional probability of accessing at least one firm filing is 1.11 percent. In addition, the 13F-contrarian strategy is stronger for stocks whose disclosure is accessed by viewing institutional investors.

This study speaks to three lines of research. First, this paper relates to the literature that studies how institutions investors acquire and use information. Earlier studies find trading behavior that is consistent with institutional investor obtain information various sources (e.g. Irvine, Lipson, and Puckett (2006), Mikhail, Walther, and Willis (2007), Cohen, Frazzini, and Malloy (2008) and Gao and Huang (2016)). More recently, a number of studies focus on uncover specific information acquisition activities using EDGAR log files. These studies primarily focus on institutional investors acquiring information from firm disclosures. For example, Chen, Cohen, Gurun, Lou, and Malloy (2018) study how mutual funds acquire information via insider-trading filings and earn abnormal returns. Dyer (2018) argues that institutional investors make more profitable trading decisions based on local information sources. Crane, Crotty, and Umar (2018) show that hedge funds acquire and profit from public information.[2] This paper examines information from a different source, namely institutional investors themselves. My findings suggest that holdings of other institutional investor contains valuable information for institutional investors.

Second, this paper contributes to the institutional herding literature. Although it is well-documented that institutional investors herd with each other (Wermers (1999) and Sias (2004)), the mechanism is unclear. One strand of research argues that certain institutional investors observe and follow others' investment decisions (Banerjee (1992), Bikhchandani, Hirshleifer, and Welch (1992), Scharfstein and Stein (1990)). Another strand of research argues that herding can arise from institutional investors receiving similar information signals or having similar preferences of stocks (Hirshleifer, Subrahmanyam, and Titman (1994), Froot, Scharfstein, and Stein (1993), Falkenstein (1996)). This paper adds to this

[2] In some analyses in Crane, Crotty, and Umar (2018), the authors also examine whether hedge funds access 13-F filings. Unlike this study, the authors do not examine how information in 13-F filings affects institutional trades.

literature by directly identifying the information acquired by institutional investors and by examining how the acquired information affects institutional trades. My findings show that although institutional investors do herd with the institutional crowd, they trade against other institutional investors from whom they access holding information. This is more consistent with institutional investors herding on common information or preference but less consistent with institutional investors copying others' trades.[3]

Similar to this finding, Jiang and Verardo (2018) show that some mutual funds exhibit "antiherding" behaviors. My results differ from these findings in two ways. First, the viewing institutional investors, on average, do not "antiherd." That is, the aggregate institutional trades are positively correlated with the viewing institutional trades. Second, Jiang and Verardo (2018) consider a "gradual information acquisition framework" in which antiherding mutual funds are informed earlier and thus trade ahead of others and then unwind their positions. In this paper, viewing institutional investors acquire the holding information of other institutional investors, and then choose to not follow the others.

Lastly, this paper adds to the vast literature studying how institutional investors pick stocks (Cohen, Frazzini, and Malloy (2008), Hong, Kubik, and Stein (2004), Solomon, Soltes, and Sosyura (2014), Coval and Moskowitz (1999), Gao and Huang (2016)). A recent paper by Chen, Cohen, Gurun, Lou, and Malloy (2018) suggests that mutual fund managers reduce the dimensionality of their information acquisition by actively tracking a subset of corporate insiders. This paper adds to this literature by documenting another mechanism through which institutional investors limit their attention to a subset of stocks. In particular, I find evidence consistent with institutional investors using 13-F filings as a shortlist of stocks for in-depth research.

The paper proceeds as follows. Section 2 discusses data and sample construction. Section 3 provides stylized facts about how institutional investors acquire information about institutional holdings. Section 4 investigates how institutional holdings affect the trading behavior of institutional investors. Section 5 examines whether the trading strategy based on other institutional holdings is profitable and explores potential mechanisms. I conclude the paper in Section 6.

[3]This interpretation is limited to the specific channel of information acquisition, namely 13-F access on the EDGAR server, and does not necessarily generalize to all forms of information acquisition. For example, Pool, Stoffman, and Yonker (2015) find that fund managers that live in the same neighborhood tend to hold the same stocks.

2 Data and Variables

I compile data from three main sources. I obtain information acquisition related variables from the EDGAR Log File Database. I uncover the identifies of EDGAR users using the Maxmind Domain Name Database, which contains registrants information for all public IP addresses. Finally, I obtain institutional holdings information from the Thomson Reuters 13-F Database. A major challenge is of this study is to identify institutional investors among the anonymized EDGAR server users. In the reminder of this section, I describe how I "unmask" the EDGAR users as well as other steps taken to construct the main data used in this paper. I also describe how I construct variables used in this paper.

2.1 EDGAR Log Files and "Unmasking" Users

The EDGAR Log File Database contains records of all search traffic for SEC filings starting in 2003. Specially, each log file observation contains following information: the time-stamp of the access, the user identifier (*masked* IP address), the SEC filing that is accessed (SEC accession number), and the entity (CIK) that is associated with the filing. Following Lee, Ma, and Wang (2015) and Li and Sun (2018), I takes several steps to clean the data. First, I filter the raw data to eliminate the requests potentially made by robots or automated webcrawlers. Specifically, I exclude EDGAR server activities of those users who access more than 25 filing per minute, more than 500 filings per day, or filings from 50 or more unique firms per day. Second, I also remove activities that access index files, since index files only provide links to filings rather than actual fillings. Third, I only keep observations with successful document delivery (code=200) to make sure information acquisition actually take place. Fourth, to avoid counting duplicated access of the same document by the same user, I keep one filing-user pair per day. Finally, for the purpose of this study, I keep records accessing 13-F filings only.

To illustrate how I uncover the information acquisition where one institutional investor search the holdings of another one, consider the following example. On July 17th of 2006, IP address "12.47.208.iei" accessed filing "0000950129-06-005536" from company "1067983". Using EDGAR index files, I obtain the filing-related information, including filing type, company name, and filing date.[4] I match the company name to institutional investor names from Thomson Reuters 13-F Database. Continuing with the previous example, on July 17th of 2006, IP address "12.47.208.iei" downloaded a 13-F filing from Berkshire Hathaway that was filed in May 15th of 2006.

[4]The index files are accessible from: https://www.sec.gov/Archives/edgar/full-index/

Notice, the IP address is composed of four groups of octets, where the last octet is masked using alphabets. I "unmask" the IP address by exploiting the fact that institutional investors often own entire netblocks or ranges of IP addresses. I rely on the MaxMind Domain Name Database to link netblocks to institutional investors. Specifically, MaxMind provides data on IP registrant names starting from the third quarter of 2006, covering all public IP addresses. Considering that some IPs are dynamic and institutional investors might enter and exit the market, I update the IP registrant data on a monthly basis. To identify institutional investors more accurately, I discard all netblocks that are registered by more than one registrant. I match each masked IP address from the EDGAR log files to single-registrant netblocks from MaxMind. I then match registrant names to institutional investor names from Thomson Reuters 13-F Database.

In the previous example, IP address ranging from 12.47.208.0 to 12.47.215.255 all belong to one institutional investor, namely Goldman Sachs. Since the entire netblock is registered under one registrant, the masked octet is obsolete. After "unmasking" the IP address, I completely pin down the information acquisition channel as the following: on July 17th of 2006, Goldman Sachs downloaded a 13-F filing that was filed by Berkshire Hathaway filed on May 15th of 2006. [5]

For a 13-F access to be included in the sample, I require both the viewing and filing institutional investors to be identified. I keep observations where an institutional investor accesses a 13-F filing that is filed in the same quarter as the access date. Since the SEC requires institutional investors to file 13-F filings within 45 days of each quarter-end, this ensures that viewing institutional investors are accessing 13-F filings containing holdings information for the previous quarter.[6] I exclude cases where institutional investors access their own filings. Finally, to avoid influence from penny stocks, I keep only common stocks listed on NYSE, Amex, and NASDAQ with prices greater than one dollar. In the final sample, I uncover 485 viewing institutional investors and 3,375 filing institutional investors, constituting 55,286 13-F searches on the EDGAR server.

[5] One caveat of this method is that MaxMind reports Internet Service Provider (ISP) names when registrant names are missing. This is unlikely to be a concern for the purpose of this study, since ISP names are unlikely to be mistakenly matched to institutional investor names.

[6] Institutional investors can delay filing their 13-F longer than 45 days. However, the incidence of delaying for more than one quarter (90 days) is rare. For example, Christoffersen, Danesh, and Musto (2015) show that 96.45 percent of 13-F filings are filed within 60 days of the end of the previous quarter.

2.2 Institutional Trade Variables

To capture the trading behavior of viewing institutional investors, I measure the scaled change in the number of shares held (*Viewing Trade*$_{i,j,t}$). Specifically, for stock i held by a viewing institutional investor j during quarter t, I calculate the split-adjusted change in the number of shares from the beginning to the end of quarter t, scaled by the total number of shares outstanding of stock i.

For each filing institutional investor, I construct the same measure for the previous quarter. Since each viewing institutional investor j can acquire information from more than one filing institutional investor, I average across all institutional investors whose holding information is acquired, weighted equally (henceforth equal-weighted) or by total asset under management(henceforth value-weighted). That is,

$$\textit{Filing Trade}_{i,j,t-1} = \sum_{f \in j_f} w_f \textit{Filing Trade}_{i,f,t-1},$$

where j_f is the set of filing institutional investors whose holdings information was acquired by institutional investor j. Intuitively, *Filing Trade*$_{i,j,t-1}$ measures the holdings information that is acquired by a viewing institutional investor j about stock i. Intuitively, *Filing Trade*$_{i,j,t-1}$ measures the holdings information that is acquired by a viewing institutional investor and *Viewing Trade*$_{i,j,t}$ measures the its trading behavior in the viewing quarter.

2.3 Other Variables

To gain insights into the nature of the viewing and filing institutional investors, I construct other institution-level characteristics. *Portfolio Size* is the net asset under management, in millions of dollars. *Number of Stocks Held* is the number of stocks held by an institutional investor. Following Carhart (1997), *Portfolio Turnover* is the minimum of purchases and sales, scaled by the average asset under management over the current and past quarter. *Portfolio Net Flows* is the growth in total asset under management. *Excess Return* is the value-weighted excess return over the risk-free rate across all portfolio holdings. *FF3 Alpha* is the value-weighted alpha across all portfolio holdings using the Fama and French (1993) model. *Carhart Alpha* is the value-weighted alpha across all portfolio holdings using the Carhart (1997) model. *FF5 Alpha* is the value-weighted alpha across all portfolio holdings using the Fama and French (2016) model.

Following Gompers and Metrick (2001) and Jiang and Verardo (2018), I construct a number of stock characteristics that are related to institutional trading. *Ln(Size)* is the natural log of market capitalization of the stock. *Ln(BM)* is the natural log of book-to-market ratio of the stock. *Momentum* is stock return over the previous 11 months. *Turnover* is the average trading volume over shares outstanding during the past 12 months. *Idiosyncratic Volatility* is the idiosyncratic volatility of the stock, calculated following Ang, Hodrick, Xing, and Zhang (2006). *Excess Return* is the stock return during the previous quarter in excess of the risk free rate. *Ln(age)* is the nature log of the firm's age. ΔIO_{t-1} and ΔIO_t are the change in total institutional ownership during the past and current quarter.

3 Direct Evidence of Information Acquisition

3.1 Descriptive Statistics

Figure 1 shows the time trend of the quarterly number of 13-F searches by institutional investors on SEC's EDGAR server. The number of 13-F downloads per quarter increase from 2006 to 2016. For example, there are 606 downloads in the last quarter of 2006, whereas 2,151 downloads in the last quarter of 2014. The upward trend is consistent with the increase in overall activities in the EDGAR server documented in Li and Sun (2018) and Crane, Crotty, and Umar (2018). Breaking down this trend into the number of viewing and filing institutional investors, the top plot in Figure 2 shows that the number of viewing institutional investors increased steadily over time. On a quarterly basis, there are around 70 unique institutional investors that access at least one 13-F filings during the first part of the sample and around 140 unique institutional investors in the latter part. The raise in the number of filing institutional investors is more salient. For example, in 2006, there are around 450 unique institutional investors whose 13-F filings are downloaded by at least one other institutional investor. This number grows to 1,603 by the end of the 2014. Not surprisingly, the number of unique viewing-filing pairs also increases over time, as shown in the bottom plot of Figure 2. This is consistent with the drop in information acquisition cost over time.

Furthermore, I also explore how quickly viewing institutional investors access 13-F filings on the EDGAR server. If viewing institutional investors are indeed actively seeking holdings information of other institutional investors, we should expect them to access 13-F filings shortly after the filings become available. Figure 3 plots the histogram of days between the date that 13-F filings are filed and the date that they are downloaded. Consistent with viewing institutional investors acquiring information in a timely fashion, approximately 40 percent of all downloads take place within three days of the file date, and more than half of the accesses take place within one week. Consistent with this view, I find stronger results using a subsample containing accesses take place within one week. Overall, these stylized facts show that institutional investors indeed acquire information about the holdings of other institutional investors.

3.2 Institution Characteristics

Next, I examine the characteristics of the viewing and filing institutional investors and whether there is any systemic difference between them. I compare institution-level characteristics, including *Portfolio Size*, *Number of Stocks Held*, *Portfolio Turnover*, *Portfolio Size*, *Excess Return*, *FF3 Alpha*, *Carhart Alpha*, and *FF5 Alpha* in Panel A of Table 1. I also compare holdings-based characteristics, including, *Size BM*, *Momentum*, *Turnover*, *Idiosyncratic Volatility*, *Age*, and *Institutional Ownership* in Panel B. The holdings-based characteristics are calculated using the stock characteristics, averaged across all portfolio holdings. To provide a benchmark, I also calculate the same characteristics for the entire Thomson Reuters 13-F Database.

Panel A of Table 1 shows that both the average viewing and filing institutional investor are larger than the average institutional investor in the Thomson Reuters universe. For example, the average viewing (filing) institutional investor has \$9.62 (\$4.06) billion in assets under management and holds 415 (193) stocks, whereas the global average institutional investor has \$2.40 billion in assets under management and holds 178 stocks. Compared to viewing institutional investors, filing institutional investors are smaller in size and hold fewer less number of stocks, but have statistically similar performance. Panel B of Table 1 reports the average holdings-based characteristics. Viewing and filing institutional investors are similar to the average institutional investor in the Thomson Reuters universe, with the exception of stock size. The average viewing institutional investor holds stocks with smaller market capitalization than the average filing institutional investor. Overall, larger and more resourceful institutional investors tend to acquire holdings information from relatively smaller institutional investors.

3.3 Determinant of Information Acquisition Activities

Finally, I examine how viewing institutional investors determine whose filings to access. Specifically, I examine what characteristic of institutional investors affects the probability of being searched on the EDGAR server by estimating the following regression:

$$\textit{13-F Access}_{j,f,t} = \alpha + \beta \textit{13-F Access}_{j,f,t-1} + \gamma Z_{f,t-1} + \phi_{j,t} + \epsilon_{j,f,t}, \tag{1}$$

where *13-F Access*$_{f,j,t}$ is a dummy variable that equals one if viewing institutional investor j accessed a 13-F filing from institutional investor f in quarter t. For independent variables, I include the lagged *13-F Access*, lagged filing institutional characteristics, and quarter-viewer fixed effects. The coefficient estimates are multiplied by 100 to facilitate interpretation and standard errors are double clustered by quarter and viewer.

Table 2 presents the results. Column (1) shows the coefficient estimate of the lagged *13-F Access* is 24.626, indicating viewing institutional investors tend to access 13-F filings from the same institutions. For example, a viewing institutional investor that accessed 13-F filings from a given filing institutional investor in a quarter has a 24.6 percent higher likelihood of downloading from the same filing institutional investor in the next quarter. The persistence is not only statistically significant, but also economically strong, given that the unconditional probability of a viewing institutional investor accessing a 13-F from a filing institutional investor is only 0.11 percent.[7] Columns (2) and (3) present how the probability of accessing 13-F filings varies with institution-level and holding-based characteristics. Filing institutional investors that manage more asset and trade more frequently have a higher likelihood of being viewed. Furthermore, filing institutional investors that hold winner stocks, high turnover stocks, and more volatile stocks have a higher likelihood of being viewed as well.

4 Implications of Information Acquisition

4.1 Baseline Analysis

The previous section shows that institutional investors indeed acquire holdings information from other institutional investors. In this section, I study how the acquired information affects trading behaviors of viewing institutional investors. Particularly, how do filing institutional trades affect viewing institutional trades? On the one hand, the herding literature suggest that institutional investors might follow others. If so, the filing institutional trades should positively predict viewing institutional trades. On the other hand, Avery and Chevalier (1999) and Jiang and Verardo (2018) show that investors who are

[7]This finding is consistent with the findings in Chen, Cohen, Gurun, Lou, and Malloy (2018). The authors argue that institutional investors tend to acquire information from the same set of sources over time.

skilled or possess more precise information have an incentive to deviate from the institutional crowd. In this case, filing institutional trades should negatively predict viewing institutional trades. To test these two predictions, I investigate the relationship between the trading behaviors of viewing and filing institutional investors by estimating the following model:

$$\textit{Viewing Trade}_{i,j,t} = \alpha + \beta \textit{Filing Trade}_{i,j,t-1} + \Gamma Z_i + \phi_{k,j,t} + \epsilon_{i,j,t}. \tag{2}$$

The dependent variable is *Viewing Trade*$_{i,j,t}$, which measures the split-adjusted change in the number of shares held of stock i by institutional investor j in quarter t, scaled by the total number of shares outstanding. The key independent variable is *Filing Trade*$_{i,j,t-1}$, which measures the change in shares held of stock i of institutional investors *viewed* by j in quarter $t-1$. Similar to the *Viewing Trade*$_{i,j,t}$, *Filing Trade*$_{i,j,t-1}$ is split-adjusted and scaled using the total shares outstanding. For each viewing institutional investor, the filing trades are averaged across all of its viewed filing institutional investors, weighted equally or by total asset under management.

Following Gompers and Metrick (2001) and Jiang and Verardo (2018), I included the following stock characteristics that potentially affect viewing institutional trades: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, *Idiosyncratic Volatility*, *Excess Return*, *Ln(age)*.[8] Accounting for the institutional herding, I also control the change in aggregate institutional ownership during the past and current quarter (ΔIO_{t-1} and ΔIO_t). Finally, I include quarter-viewer-industry fixed effects ($\phi_{j,t,k}$). Standard errors are double clustered by industry of the stock i and quarter t. All variables are standardized to facilitate interpretation.

The regression results are reported in Table 3. Viewing institutional investors tend to trade against filing institutional investors whose holdings information was acquired. For example, the baseline regressions (column (1) and (2)) show that both value-weighted and equal-weighted *Filing Trade* negatively predict *Viewing Trade*. The coefficient estimates for value-weighted and equal-weighted *Filing Trade* are -0.006 and -0.005, significant at the one and five percent level. This negative predictability is robust to the inclusion of various stock characteristics (columns (3)-(8)). In terms of economic magnitude, a one standard deviation increase in *Filing Trade* predicts a 0.006 standard deviation decrease in *Viewing Trade*, controlling for stock characteristics. Such a negative effect moves the median viewing trade to the 41^{th} percentile.

[8]Results are robust when using Carhart alpha.

Consistent with the herding literature (Wermers (1999) and Sias (2004)), *Viewing Trade* is positively associated with the lagged and current aggregate institutional trade. This shows that viewing institutional investors herd with the institutional crowd. Since all variables are standardized, the coefficient estimates of *Filing Trade* and ΔIO_t are comparable. The effect of a one standard deviation increase in *Filing Trade* accounts for 12% to 17% of a one standard deviation decrease in ΔIO_t. This is economically important, considering the median viewing institutional investor views only 5 filing institutional investors, whereas the institutional crowd consists thousands of institutional investors.

The fact that viewing institutional investors herd with the crowd, but trade against institutional investors whose holding information is acquired sheds light on the mechanism underlying the well-documented institutional herding phenomenon (Wermers (1999) and Sias (2004)). One strand of research argues that institutional investors observe and follow others' investment decisions (Banerjee (1992), Bikhchandani, Hirshleifer, and Welch (1992), Scharfstein and Stein (1990)). Another strand of research argues that institutional investors obtain common information or have similar preferences of stocks (Hirshleifer, Subrahmanyam, and Titman (1994), Froot, Scharfstein, and Stein (1993), Falkenstein (1996)). The previous findings are more consistent with institutional investors herding on common information or preference, because viewing institutional investors trade against filing institutional investors whose holdings information is acquire. This, however, does not rule out institutional investors obtain others' investment decisions via other channels and follow those decisions. For example, Pool, Stoffman, and Yonker (2015) find that fund managers that live in the same neighborhood interact socially and tend to hold the same stocks.

Related to the previous findings, Jiang and Verardo (2018) show that some mutual funds exhibit "antiherding" behaviors. My results differ in two ways. First, this paper studies how institutional investors trade with respect to other institutional investors whose holdings information is acquired, whereas Jiang and Verardo (2018) focus on how institutional investors trade with respect to the aggregate institutional investor. In fact, the previous analysis shows that viewing institutional investors are not "antiherding" investors, since the aggregate institutional trades are positively correlated with viewing institutional trades. Second, this paper considers a sequential decision-making framework, where viewing institutional investors observe the investment decisions of other institutional investors and subsequently make their own investment decisions. Jiang and Verardo (2018) consider a gradual information acquisition framework, where antiherding mutual funds are informed earlier than others.

To ensure the previous finding is robust, I use two alternative measures of institutional trading behavior. First, I use a dummy variables indicating the direction of institutional trade. I estimate the following model:

$$\textit{Viewing Buy}_{i,j,t} = \alpha + \beta \textit{Filing Buy}_{i,j,t-1} + \Gamma Z_i + \phi_{k,j,t} + \epsilon_{i,j,t}. \tag{3}$$

where $\textit{Viewing Buy}_{i,j,t}$ is a dummy variable that equals one if the viewing institutional investor j increased its position for stock i in quarter t and $\textit{Filing Buy}_{i,j,t-1}$ is a dummy variable that equals one if the filing institutional investor viewed by investors j increased their position for stock i in quarter $t-1$. [9]

Second, I measure institutional trade as changes in portfolio weight from the beginning to the end of the quarter, accounting for stock returns. That is,

$$\Delta \textit{Weight}_{i,j,t} = \frac{\$\text{Held}_{i,j,t}}{\sum_i \$\text{Held}_{i,j,t}} - \frac{\$\text{Held}^*_{i,j,t}}{\sum_i \$\text{Held}^*_{i,j,t}} \tag{4}$$

where $\$\text{Held}_{i,j,t}$ is the dollar amount held of stock i by institutional investor j by the end of quarter t and $\$\text{Held}^*_{i,j,t} = \$\text{Held}_{i,j,t-1} \times (1 + r_{i,t})$. Intuitively, $\$\text{Held}^*_{i,j,t}$ measures the dollar amount held of stock i by institutional investor j by the end of quarter t, if there is not change shares held. I estimate a similar model as Equation (2) with changes in portfolio weight:

$$\Delta \textit{Viewing Weight}_{i,j,t} = \alpha + \beta \Delta \textit{Filing Weight}_{i,j,t-1} + \Gamma Z_i + \phi_{k,j,t} + \epsilon_{i,j,t}. \tag{5}$$

Table 4 and 5 present the estimation of Equation (3) and Equation (5). Both *Filing Buy* and Δ*Filing Weight* load negatively and significantly. This is consistent with filing and viewing institutional investors trading in the opposite direction. Similar to the previous findings, this pattern is robust to different model specifications.

4.2 Subsample Analysis

To further expand our understanding on the relationship between viewing and filing institutional trades, I split the previous sample into two subsamples: Filing Buy and Filing Sell. For each stock i held by viewing institutional investor j at quarter t, I assign this stock to one of the two subsamples,

[9] To make a clean interpretation, I exclude observations where viewing or filing institutional investors have no change in shares held.

depending on the sign of *Filing Trade*$_{i,j,t-1}$.[10] The Filing Buy (Filing Sell) subsample contain stocks that experienced a positive (negative) *Filing Trade*$_{i,j,t-1}$.

I estimate Equation (2) in these two subsamples and results are presented in Table 6. Panel A shows that when only considering stocks that experienced a positive change in shares held by filing institutional investors are, viewing institutions tend to trade against filing institutional investors. This result is, however, weak both statistically and economically. For example, the coefficient estimates on value-weighted and equal-weighted *Filing Trade* are -0.004 and -0.003. Neither is statistically significant. The economic magnitude is only half of what is shown in the full sample analysis. In sharp contrast, the negative association between filing institutional trades and viewing institutional trades is strongly present in the Filing Sell subsample, as shown in Panel B. For example, the coefficient estimates on value-weighted and equal-weighted *Filing Trade* are -0.09 and -0.08, both significant at the five percent level. Overall, when trading against filing institutional investors, viewing institutional investors predominantly purchase the stocks that are sold by the filing institutional investors. The stronger result in the Filing Sell subsample is consistent with many institutional investors (e.g. mutual funds and pension funds) facing short-sale constraints. It is also possible that viewing institutional investors indeed take short positions, but do not report on 13-F filings.

4.3 Alternative Explanations

The previous results shown that the filing institutional trade negatively predicts the viewing institutional trade, particularly in a sell-buy relationship. This is consistent with the hypothesis that the viewing institutional investors obtain information about the filing institutional investor and subsequently trade on the acquired information. In this section, I consider two alternative hypotheses that are seemingly consistent with these findings, but find inconsistent evidence.

4.3.1 Propensity Score Matching Analysis

One potential mechanism of the previous results is that the viewing and filing institutional investors trade in certain ways that are mechanically negatively correlated with each other. Accessing 13-F filings merely confirms their pre-determined trading strategy. In this case, viewing institutional investors do not actually "use" the information acquired from 13-F filings. For example, suppose that viewing insti-

[10] The stocks are assigned using the value-weighted *Filing Trade*. In a robustness check, I also assign stocks based on their equal-weighted filing trade. The results are similar to those that using the value-weighted *Filing Trade*.

tutional investors are mostly momentum traders and filing institutional investors mostly bet against the momentum strategy. Their trades could be mechanically negatively correlated, even in the absence of 13-F access. To address this concern, I carry out a placebo test in a propensity-score-matched sample, following methodologies described in Rosenbaum and Rubin (1983) and Lemmon and Roberts (2010).

I first prepare a list of variables that could potentially determine the propensity of a particular filing institutional investor being viewing by a given viewing institutional investor. For each institutional investor, I compute its *Portfolio Size*, *Portfolio Turnover*, *Portfolio Net Flow*, and *Carhart Alpha*. I also include holding-based characteristics, including *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, *Idiosyncratic Volatility*, *Ln(Age)*, and *Institutional Ownership*. Holding-based characteristics are computed at the stock level and averages across all stocks held.

For each quarter t, I compute the propensity score of a viewing institutional investor j accessing the 13-F filing from an institutional investor f. Specifically, I run a logistics model of the *13-F Access* dummy on the characteristics of filing institutional investors. I match each *viewed* filing institutional investor with a placebo filing institutional investor that has the closest propensity score.[11]

Using the propensity-score-matched sample, I repeat the previous analyses and present the results in Table 7. Panel A columns (1) and (2) show the results of regressing *Viewing Trade* on the placebo *Filing Trade* using the same specification as in Equation 2. The coefficient estimate of *Filing Trade* is not significant statistically. Analogous to Table 6, Column (3) to Column (6) show the results of subsample analyses. In both full sample and subsample analyses, *Filing Trade* does not load significantly. This shows that viewing institutional investors does not trade in anyway that is correlated with the placebo filing institutional investors. Overall, the insignificant relationship between viewing and filing institutional trades in the propensity-score-matched sample is inconsistent with the conjecture that previous findings are mechanically driven by a pre-determined trading strategy.

4.3.2 Common Information

Another concern is that the negative predictability due to viewing and filing institutions react to the same information signal. Controlling for the contemporaneous aggregate institutional trades addresses the concern of viewing and filing institutional investors make their trading decision based on information that is observed by all institutional investors. However, this does not shield the previous results from

[11] Matching diagnostics can be found in Table A1.

being driven by common information received only by the viewing and filing institutional pair. To address this issue, I control for the contemporaneous filing institutional trades in the Equation 2.

Table 8 presents the regression results after controlling for the contemporaneous filing institutional trades. Columns (1) and (2) show the full sample analysis, using *Viewing Trade* and *Filing Trade*. The previous findings are robust to the inclusion of contemporaneous filing institutional investor trades ($\textit{Filing Trade}_{i,j,t}$), the negative predictability is also economically stronger. For example, the coefficient estimates for value-weighted and equal-weighted *Filing Trade* are -0.010 and -0.008, both significant at the one percent level. Controlling for contemporaneous filing trades, a one standard deviation increase in $\textit{Filing Trade}_{i,j,t-1}$ moves the median viewing trade to the 39^{th} percentile. Column (3) to Column (6) show the results of subsample analyses. Similar to previous results, I find stronger negative predictability in the Filing Sell subsample.

5 Profitability and Mechanism

5.1 Return Predictability

The previous analyses show evidence consistent with institutional investors acquiring and using holdings information from other institutional investors. A natural question to ask is what is the incentive to do so. Ex ante, it is unclear whether 13-F filing can benefit institutional investors in generating returns. On the one hand, institutional investors who access and process 13-F filings could gain informational advantages. On the other hand, given their infrequent nature and limited content, 13-Fs might make only a limited marginal contribution to the institutional investors' existing information set. I examining whether such 13F-contrarian trading strategy is warranted by subsequent returns at the stock level

Since the contrarian strategy manifests mainly in a sell-buy relationship, I focus on the Filing Sell subsample. I categorize each stock as a contrarian stock or a confirmation stock in quarter t by estimating the following model[12]:

$$\textit{Viewing Trade}_{i,j,t} = \alpha_{i,t} + \beta_{i,t}\textit{Filing Trade}_{i,j,t-1} + \epsilon_{i,j,t}. \tag{6}$$

For stock i at quarter t, if the estimated $\beta_{i,t}$ is negative ($\hat{\beta}_{i,t} < 0$), I define stock i as a 13F-contrarian stock. Conversely, if $\hat{\beta}_{i,t} > 0$, stock i at quarter t is deemed to be a 13F-confirmation stock. Intuitively,

[12]The stocks are assigned using the value-weighted filing trade. In robustness check, I also assign stocks based on their equal-weighted filing trade. The results are similar to those that using value-weighted target trade.

if viewing and filing institutional trades are negatively (positively) correlated, the stock is deemed to be a 13F-contrarian (13F-confirmation) stock. I form 13F-contrarian (13F-confirmation) portfolios by buying 13F-contrarian (13F-confirmation) stocks at the end of quarter t and track the portfolio for the next quarter. To better capture the actions of viewing institutional investors, I weigh each stock by its $\hat{\beta}_{i,t}$. The intuition is to put more weight on the stocks that viewing institutional investors trade more aggressively against filing institutional investors. In similar spirit, I also form tercile portfolios, sorted on the magnitude of $\hat{\beta}_{i,t}$, within in the 13F-contrarian and 13F-confirmation portfolios. I measure portfolio performance using the excess return over the risk-free rate, the excess return over the market return , the Fama-French 3-factor alpha, the Carhart 4-factor alpha, as well as the Fama-French 5-factor alpha.

Panel A of Table 9 presents the returns for the 13F-contrarian portfolios. On average, 13F-contrarian stocks earn positive significant excess and abnormal returns. For example, the first row of Panel A shows that the 13F-contrarian portfolio earns 0.91 percent quarterly Carhart alpha and 0.66 percent quarterly Fama-French five-factor alpha. When splitting 13F-contrarian stocks into $|\hat{\beta}|$-sorted portfolios, significant profitability is stronger for stocks with larger $|\hat{\beta}|$. This is consistent with viewing institutional investors trading more aggressively on stocks that earn positive abnormal returns. In sharp contrast, the 13F-confirmation portfolio earns insignificant excess and abnormal returns, as shown in Panel B.

I expand the analysis by exploring how the profitability of the 13F-contrarian strategy varies with stock characteristics. Similar to the $|\hat{\beta}|$-sorted portfolios, I divide the 13F-contrarian stocks into terciles based on the stocks' market capitalization, trading volume and illiquidity. Trading volume is measured using total dollar volume during the last quarter. Illiquidity is measured as in Amihud (2002). Results are presented in Table 10. Panel A shows the returns for three size-sorted portfolios. The small market capitalization tercile earns a significant 1.48 percent quarterly Carhart Alpha, whereas the large tercile earns a insignificant Carhart Alpha. Similar results are found for volume- and liquidity-sorted stocks. The low volume and high illiquidity portfolio earns 1.67 percent and 1.49 percent Carhart Alpha, respectively. Overall, the profitability of 13F-contrarian stocks is stronger for small, low turnover, and less liquid stocks.

5.2 Potential Mechanism

The previous results show institutional investors acquire and profit from the holdings information of other institutional investors. Since we do not observe the exact decision-making process of viewing institutional investors, it is hard to pin down the exact channel. Nevertheless, in this section, I explore several possible explanations.

5.2.1 Buying Price Pressure

One simple case is that the viewing institutional investors are not using for stock picking, but buying price pressure from viewing institutional investors drives the previous results. In this case, the positive abnormal return of the contrarian stocks may simply reflect price pressure from the viewing institutional trading. Indeed, past studies show that institutional trading can cause price impacts. For example, Lou (2012) shows that trades from mutual funds can partially explain momentum. Since the positive abnormal return is earned by stocks in a sell-buy relationship, it is possible that the positive abnormal return is caused by viewing institutional purchases. If this is the case, we should observe return reversal after the positive abnormal return. Furthermore, return reversal should concentrate in small stocks, low volume stocks, and illiquid stocks, as they are more susceptible to price pressure.

Table 11 shows portfolio returns the results three quarters after the previously documented positive abnormal return is measured ($t + 1$). Panel A show that there is no significant negative abnormal return in the ensuing three quarters for stocks with small market capitalization. Similarly, there is no significant return reversal for low volume stocks (Panel B) and illiquid stocks (Panel C). The lack of return reversal is inconsistent with buying price pressure from viewing institutional investors causing of the profitability of contrarian strategy.

5.2.2 Return Reversal

Previous results show that the profitability of 13F-contrarian stocks is stronger for small, low turnover, and less liquid stocks. One commonality amount these stocks is that they are more susceptible to price pressure. Since the 13F-contrarian stocks are in the Filing Sell subsample, it is possible that filing institutional investors create downward price pressure and viewing institutional investors use 13-F filing to identify such stocks and, subsequently, profit from the return reversal. Indeed, previous studies show that selling from institutional investors can have negative impact on stock prices. For example, Coval and Stafford (2007) show that mutual fund fire sales lead to negative price pressure. Such negative price pressure lasts for longer than 12 months. If viewing institutional investors are indeed trading on stocks that experience selling price pressure, we should observe negative abnormal returns prior to the positive abnormal returns. Furthermore, the negative price pressure should be concentrated in small stocks, low volume stocks, and illiquid stocks.

To examine this possibility, I calculated the contrarian portfolio abnormal returns in quarter $t-1$, where the filing institutional investors trade, and in quarter t, where viewing institutional investors trade. Table 12 presents the results. Panel A shows that small stocks experience significant downward price pressure. Taking Cahart alpha as an example, small stocks earn -1.18 percent alpha in quarter $t-1$ and -1.79 percent in quarter t. Although statistically insignificant, low volume stocks and illiquid stocks also experience negative price pressure, as shown in Panel B and Panel C. The negative abnormal return suggests that institutional investors use 13-F filings to identify stocks that are sold by other institutional investors and profit from the subsequent return reversal.

A reminding questions is that how do viewing institutional investors decide what filing sales to trade against? If a filing sale is driven by unfavorable information about the stock, the stock price could fall in the ensuing quarters. Thus, viewing institutional investors should exercise due diligence to examine whether the filing sales are information driven. If so, we should expect that viewing institutional investors acquire more information about stocks that appear in 13-F filings. If so, we should expect institutional investors to acquire more information about stocks that appeared in 13-F filings, particularly upon viewing said 13-F filings. Furthermore, viewing institutional investors' contrarian strategy is stronger for the stocks that they acquired stock-related informations.

First, I study whether viewing institutional investors acquire additional stock-related information. Taking advantage of the richness of the EDGAR search traffic, I expand the information acquisition activities of institutional investors beyond the 13-F filings to examine the acquisition of information directly related to stocks held by a given institutional investor. In particular, I estimate the following models:

$$\textit{Direct Access}_{i,j,t} = \alpha + \beta \textit{13-F Access}_{i,j,t} + \gamma Z_i + \phi_{j,t,k} + \epsilon_{i,j,t}.$$

$$\textit{Direct Access}_{i,j,t+1} = \alpha + \beta \textit{13-F Access}_{i,j,t} + \gamma Z_i + \phi_{j,t+1,k} + \epsilon_{i,j,t}. \tag{7}$$

The dependent variable is $\textit{Direct Access}_{i,j,t}$, which measures the information acquisition activities of institutional investor j holding stock i in week t. In particular, I measure this with a dummy variable that equals one if an institutional investor j accesses filings from stock i in week t. I consider different types of filings, including: proxy statements, 8-K, insider-trading filings, and 10-K/Q. They key independent variable is *13-F Access*$_{i,j,t-1}$, which is a dummy variable that equals one if stock i appeared in the 13-F filings accessed by an institutional investor j in week t.

Following Li and Sun (2018) and Crane, Crotty, and Umar (2018), I included the following lagged stock characteristics that potentially affect the probability of *Direct Access*, including *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, *Idiosyncratic Volatility*, *Excess Return*, *Ln(age)*, *Institutional Ownership*.[13]. Finally, I include week-viewer-industry fixed effects ($\phi_{j,t,k}$). I report double clustered standard errors by industry and quarter. All variables are standardized to facilitate interpretation. The coefficient estimates are multiplied by 100 to facilitate interpretation.

Panel A of Table 13 presents the results from the contemporaneous regression. *13-F Access* is positively and significantly associated with *Direct Access*. For example, column (1) shows the regression results examine *Direct Access* of any of four types of filings. The coefficient estimate of *13-F Access* is 0.307 and significant at the one percent level. Economically, indirect access via 13-F filings increases the likelihood of direct access by 30.7 bps. As a benchmark, the unconditional probability of *Direct Access* is 1.11 percent. This positive association is present when considering 8-K, insider-trading, and 10-K/Q filings individually. Notably, the effect is stronger for fundamental related information (e.g. 10-K and 10-Q). For instance, indirect access via 13-F filings increases the likelihood of accessing 10-K/Q filings by 20.9 bps, significant at the one percent level (column (5)), whereas the increase in likelihood of accessing proxy statements is only 1.7 bps, significant at the five percent level (column (3)). The predictive regression shows even stronger results (Panel B). For example, *13-F Access* predicts a 33.3 bps increase in the likelihood of *Direct Access* (column (1)) and an 28.2 bps increase in the likelihood of accessing 10-K/Q (column (5)). Both are significant at the one percent level. The results are consistent with viewing institutional investors acquire additional stock-related information to identify information driven filing trades.

Next, I study how whether viewing institutional investors' contrarian strategy is stronger for the stocks that they acquired stock-related informations. Specifically, I estimate the following model:

$$\begin{aligned} \textit{Viewing Trade}_{i,j,t} = \alpha + \beta_1 \textit{Filing Trade}_{i,j,t-1} + \beta_2 \textit{Direct Access}_{i,j,t} \\ + \beta_3 \textit{Filing Trade}_{i,j,t-1} \times \textit{Direct Access}_{i,j,t} + \Gamma Z_i + \phi_{k,j,t} + \epsilon_{i,j,t}, \end{aligned} \tag{8}$$

where *Direct Access*$_{i,j,t}$ a dummy variable that equals one if an institutional investor j accesses filings from stock i within two weeks of 13-F access. If viewing institutional investors search stock filings

[13] Results are robust when using the four-factor alpha following Carhart (1997)

are due diligent exercise, we should expect stronger contraian trading for those stock whose filings are searched on the EDGAR server. In this case, the interaction term of *Filing Trade* and *Direct Access* should load negatively and significantly.

I report the results in Table 14. Consistent with previous results, *Filing Trade* loads negatively and significantly. More importantly, in column (1), the coefficient estimate of the interaction term between *Filing Trade* and *Direct Access* is negative and significant. For example, when any of four types of filings (Column (1)), the coefficient estimate of the interaction term is -0.016 and significant at the five percent level. Similar to the previous findings, effect is stronger for fundamental related information (e.g. 10-K and 10-Q). Overall, the results are consistent with viewing institutional investors exercise caution by acquire additional stock-related information and trade accordingly.

6 Conclusion

Despite the informational value of institutional holdings, there is no direct study on whether and how institutional investors use institutional holdings in their decision-making process. This study focuses on the extent and implications of institutional investors' use of institutional holdings as an informational source. Using a novel dataset, I pin down a specific channel of how institutional investors acquire information about other institutional holdings, namely, accessing 13-F filing on the SEC EDGAR server. I demonstrate that institutional investors indeed seek out information about the holdings of other institutional investors. In addition, 13-F access on the EDGAR server takes place shortly after 13-F filings are filed and the total number of searches becomes more prolific over time. The results reveal that 13-F access has surprising effects on institutional trading. Although viewing institutional investors herd with the aggregate institutional investors, they trade in the opposition direction as the institutional investors whose information is acquired. This contrarian strategy is stronger for sell-buy relationships. This suggest that institutional herding is more likely due to institutional investors have common information or similar preference of stocks, but less likely due to institutional investors copy each others investment decisions. Further analysis shows that this contrarian strategy earns positive abnormal returns, especially for small stocks, low volume stocks, and illiquid stocks. I found suggestive evidence that viewing institutional investors profit from the return reversal caused by filing institutional sales. I also find that institutional investors conduct more stock-specific information acquisition upon viewing the given stock in 13-F filings and trade accordingly.

Public Market Players in the Private World: Implications for the Going-Public Process *

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Abstract

Recent years have seen a dramatic increase in investment by public market institutional investors in the private market. We study the economic consequences of these investments for the initial public offerings of startups. We find that institutions' pre-IPO participation is associated with lower IPO underpricing for VC-backed startups. Our further analysis shows that the reduction in IPO underpricing does not appear to be driven by endogenous matching between startups and institutions. We explore the underlying economic mechanisms, and our results are consistent with a substitution effect between institutions and all-star analysts.

Keywords: IPO Underpricing, Venture Capital, Institutions
JEL Classification: G23; G24; L13.

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1 Introduction

Recent years have seen a dramatic change in the financing of startups. Startups that used to be financed primarily by venture capitalists (VCs) are increasingly receiving capital from public market institutional investors, such as mutual funds and hedge funds (henceforth referred to as "institutions") (see Figure 4).[1] This change in the financing of startups is intriguing and seems surprising at first glance.[2] First, unlike traditional VCs, public market institutions do not specialize in nurturing startups. These institutional investors typically hold large portfolios in the public market, and venture capital investment accounts for only a small portion of their portfolios, which limits their involvement in entrepreneurial firms.[3] Second, because some public market institutions, such as mutual and hedge funds, have an open-end nature, startup financing from these institutions may be vulnerable to fund outflow. Third, given that the amount of private money from VC and private equity (PE) funds has increased dramatically recently (Ewens and Farre-Mensa, 2018), startups do not necessarily need financing from institutions. So far, it is not clear how institutions help startups grow or what the economic consequences of this change in financing are.

This paper attempts to fill this gap in the literature. The answer to the above questions ultimately lies in the unique features of institutions relative to traditional VCs and how these unique features help startups. One clear competitive advantage of institutions is that they specialize in the public market. This specialization could benefit startups in various ways. First, unlike traditional VCs, which normally liquidate their shares in a startup within six months to one year after its initial public offering (IPO), institutions do not necessarily liquidate their shares after a startup goes public. Thus the presence of institutions could stabilize the demand for company shares in the post-IPO market. Furthermore, this expected demand from institutions could even benefit startups in the primary markets. In particular, institutions may help startups the most during the IPO, which is a pivotal point between public status and private status.[4] This argument suggests that a startup may be in particular need of institutions'

[1] We define public market institutional investors as those required to file a 13-F report.

[2] Large mutual funds, such as Fidelity, T. Rowe Price, and Blackrock, are increasingly showing a keen interest in young tech private firms ("Mutual Funds are Bypassing IPOs and Going Straight for the Main Course", *QUARTZ*, April 2014). For example, while venture capitalists poured 11.3 billion US dollars into startups in the first quarter of 2015, up only 11% from a year earlier, the non-traditional funds, including hedge funds and mutual funds, invested 6.4 billion US dollars, a 167% increase from the pervious year("Hedge Fund Money Going to Venture-Backed Startups Is Skyrocketing", *Yahoo Finance*, April 2015).

[3] The median ratio of amount invested in entrepreneurial firms to institution public equity market holding is 0.1%. In fact, Chernenko, Lerner, and Zeng (2017) provide evidence that mutual funds appear to be less involved in corporate governance of the startups than traditional VCs are.

[4] Our argument is consistent with some anecdotal evidence. For example, a *Wall Street Journal* article of February 2, 2017,

participation when it is in late-stage financing with a forthcoming IPO. Consistent with this evidence, our summary statistics in Table 1 show that 26.78 % of startups involved with institutions exit via IPO, but only 12.65 % of startups backed only by traditional VCs exit via IPO.[5]

When examining how institutions benefit startups during the IPO process, we focus on how institutions help startups reduce underpricing. Going public is one of the most important milestones for startups. During the IPO process, firms often leave a large amount of money on the table, which is defined as first-day underpricing. Loughran and Ritter (2002) find that, from 1990 to 1998, firms going public in the U.S. left more than $27 billion on the table, which is twice as large as the $13 billion in investment banker fees.[6]

The institutions, as the public market experts, cross the border to invest in pre-IPO deals and may dramatically change the dynamics among the issuing firms, underwriters, and investors during the IPO process. First, institutions' pre-IPO participation may affect the bargaining power between the underwriters and the issuing firms by reducing the role of influential analysts in the secondary market and thus reducing IPO underpricing. Based on the ”analyst lust” theory proposed in Liu and Ritter (2011), influential analysts could attract institutions to increase the post-IPO market value of the startup, and thus underwriters with these analysts have bargaining powers in increasing IPO underpricing.[7] When these analysts' public market clients (i.e., public market institutions) participate directly in pre-IPO startups, they potentially help the issuing firms gain bargaining power against underwriters. Second, given institutional investors' expertise in the public market, they may be able to provide certification of the startup quality, and therefore reduce the information asymmetry during the IPO process. Third, institu-

"More Mutual Funds Are Pumping Money into Small Firms", lists various benefits for startups funded by institutions, including "**IPO prep**. The advice is not just there when there is a misstep. Perhaps most important, the advice and coaching can help companies with their debut on the stock market, aka the IPO...Mr. Kalra says he and his team try to prepare company managers for what to expect when their stock is listed. They hold mock earnings conference calls, and mock roadshows where company leaders will talk with investors...**Longer-term capital.** Venture-capital investors are typically involved for only a small part of a company's life cycle. 'As soon as the company goes public the VC exits,' meaning they sell their stake, says Mr. Kalra. 'Whereas when the company goes public we'll probably invest more capital.' In other words, the relationship continues beyond the IPO."

[5] Table 1 shows that the percentages of startups that exit after merger and acquisition (M&A) are comparable for those involved with and without institutions' involvement (40.50% for those with institutions' participation vs. 40.22% for those without institutions' participation). Table A3 shows that institutions' participation is still highly associated with IPO deals even after controlling for startup characteristics and year/industry/state fixed effects. In contrast, institutions' participation is insignificantly associated with startups with M&A exits.

[6] As a comparison, these firms generated about $8 billion in profits in the year before going public.

[7] Liu and Ritter (2011) explicitly argue that the ”analyst lust” theory works when some early investors in startups, especially VCs, are particularly concerned with post-IPO stock prices. These early investors are usually restricted from liquidating their shares until several months after the IPOs, and thus care about the post-IPO stock prices. The direct prediction is that VC-backed startups lust for underwriters who can provide services bundled with coverage from these analysts and thus reward such underwriters with significant IPO underpricing.

tions' pre-IPO financing may enable startups to stay private longer, and as a result these startups tend to be more mature during the IPO, which is naturally associated with lower IPO underpricing.[8]

To understand how institutions' pre-IPO participation affects IPO underpricing, we take several steps. First, we examine the relation between IPO underpricing and institutions' pre-IPO participation. Second, to isolate the institutions' pre-IPO participation from other factors, we use propensity score matching and a plausibly exogeneous shock to institutions' participation—the 2003 mutual fund scandal. Third, we explore the underlying economic mechanisms through which institutions' pre-IPO participation affects IPO underpricing.

First, in the baseline results, we document that institutions' pre-IPO participation reduces IPO underpricing for VC-backed IPOs.[9] The economic magnitude is sizable: a one standard deviation increase in the proportion of institutional investment in startups reduces their IPO underpricing by 1.6%, or 6.4% of the mean IPO underpricing. When we use an institution-backed dummy, we find that institutions' pre-IPO participation reduces IPO underpricing by 3.4%.[10] This magnitude is comparable to the underpricing effect generated by top-tier underwriters or underwriters with all-star analysts. For example, Liu and Ritter (2011) find that firms that issue stocks using top-tier underwriters are subject to 2.4% more IPO underpricing, and those using a bookrunner that bundles underwriting with influential analyst coverage are subject to 9% more underpricing.

There are some endogeneity issues regarding the baseline finding. For example, the reduced IPO underpricing that we document might be driven by endogenous matches between institutions and deals. To disentangle an institutions' pre-IPO participation from deal characteristics, an ideal experiment would be to evaluate the IPO underpricing of startups under the random assignment of institutions' participation. While such an experiment is challenging, we still conduct two groups of tests, which allow us a quasi-random environment. First, we use the propensity score matching procedure, which allows us to minimize the difference in observable characteristics between institution-backed and non-institution-backed startups. We match these two groups at the IPO year using a wide set of factors known to affect IPO underpricing. Our propensity score matching analysis results shows that institutions' participation still significantly reduces IPO underpricing after matching issuing firm characteristics.

[8] There may also be some other mechanisms through which institutions' pre-IPO participation affects IPO underpricing. See section 4.4 for a detailed discussion.

[9] This paper focuses on VC-backed startups because we are studying how institutions' participation in startups as VCs affects IPO underpricing. We also examine non-VC-backed startups as a placebo test later in the paper.

[10] This untabulated result is available upon request.

Furthermore, we use the 2003 mutual fund scandal as a plausibly exogenous shock to mutual funds' participation in pre-IPO VC deals. Given that the mutual fund scandal had a negative impact on fund flows (McCabe, 2009; Anton and Polk, 2014; Koch, Ruenzi, and Starks, 2016) but had no bearing on startup characteristics, we hypothesize that the scandal reduced the propensity of mutual funds to invest in startups, and thus affected IPO underpricing only through mutual fund investment. Consistent with our conjecture, we find that the mutual fund scandal significantly reduced the likelihood of mutual funds' investment in startups. Moreover, the reduced mutual fund investment led to greater IPO underpricing.

Having established the causal impact of institutions' pre-IPO participation on IPO underpricing, we explore three potential mechanisms for why institutions' pre-IPO participation reduces IPO underpricing. First, we examine an analyst substitution hypothesis that builds upon the analyst lust theory in Liu and Ritter (2011). According to the analyst lust theory, because all-star analyst coverage could boost a startup's post-IPO stock price by improving publicity and attracting institutional investors, VC-backed startups reward underwriters with all-star analysts with greater IPO underpricing. When institutions (i.e., all-star analysts' target clients in public markets) participate directly in primary markets, the role of all-star analysts in attracting institutional investors in post-IPO markets weakens. The cross-sectional prediction is that there will be a weaker relationship between IPO underpricing and all-star analyst coverage when institutions participate in a pre-IPO startup. Furthermore, because VCs usually liquidate their startup shares several months after an IPO (because of a lock-up period), the effect of all-star analysts should only be mitigated by institutions with a long-term commitment to holding the company's shares in the secondary market.

To examine this hypothesis, we first classify institutions into dedicated and non-dedicated investors following Bushee (1998). Dedicated investors tend to have longer horizons than non-dedicated investors and are therefore less likely to liquidate their shares after a startup's IPO. Non-dedicated investors include quasi indexers and transient investors.[11] Quasi Indexers are unlikely to stay in startups because they have index tracking behavior and a startup is unlikely to be included in indexes immediately after an IPO; transient investors are likely to liquidate early given their high portfolio turnover.[12] As a result,

[11]Normally quasi indexers track indexes and are less likely to invest in startups. Although the study of the quasi indexers' motivation to invest in startups is out of our scope, it is highly likely that these quasi indexers seek alphas in startups.

[12]Although we do not have detailed information on how institutions liquidate their shares when firms go to pubic, in untabulated results, we find some suggestive evidence that dedicated investors indeed have a relatively longer horizon than non-dedicated investors. Specifically, during the pre-IPO stage, dedicated investors and non-dedicated investors contribute similar proportions of the investment, but in the post-IPO market the share holdings of dedicated investors are six times those of non-dedicated investors. The difference in their pre-IPO investment is statistically insignificant, and the difference in their

the reduction in underpricing is largely driven by dedicated investors rather than by non-dedicated investors. Furthermore, the effect of all-star analysts on IPO underpricing is weakened by the presence of dedicated investors, but not by the presence of non-dedicated investors. These findings are consistent with the analyst substitution hypothesis.

We further use non-VC-backed deals as a placebo test, and find no significant relation between IPO underpricing and institutions' pre-IPO participation. This result strengthens the analyst substitution hypothesis. According to Liu and Ritter (2011), underwriters with all-star analysts tends to be associated with IPO underpricing in VC-backed samples, as VCs care about exit prices. Therefore, if institutions indeed reduce IPO underpricing through their substitution for all-star analysts, we should only observe the effect on VC-backed deals.

The second possible mechanism for why institutions' pre-IPO participation reduces IPO underpricing is that institutions reduce the information asymmetry related to the startups' quality. For example, institutions' public market expertise may help identify and certify a startup's quality. If this is true, we should expect to see that the association between IPO underpricing and institutional investment are stronger when institutions have better public market investment performances, particularly in the startup's industry. However, we find no support for this hypothesis. In addition, if institutions are able to certify a startup's quality, their pre-IPO participation could reduce the uncertainty before a startup's IPO process (e.g., filling date). Thus we should expect to see a smaller absolute change in offer prices relative to the initial filing price estimate (Hanley and Hoberg, 2010). However, we find that institutions' participation is not significantly associated with offer price changes. Finally, if institutions have the ability to reduce IPO underpricing by certifying startups, we should observe a negative relationship between IPO underpricing and institutions' pre-IPO participation for both VC-backed and non-VC-backed deals (or the effect should be stronger for non-VC-backed deals). However, we find an insignificant relation between institutions' participation and IPO underpricing for non-VC-backed deals. Overall, we find no evidence of the information asymmetry hypothesis.

The third possible mechanism for the reduced IPO underpricing effect of institutions' pre-IPO participation is related to financing. Institutions' participation can relax startups' financing constraints, and as a consequence allow the startups to stay private longer. Those more mature startups may tend to have

post-IPO holdings is statistically significant. This indicates that dedicated investors tend to hold on to their shares longer or potentially purchase more shares post IPOs.

lower IPO underpricing when they go public. However, in propensity score matching, we have already matched the startups with and without institutions' participation using their age. Therefore, the lower IPO underpricing does not seem to be driven by a more mature startup. Furthermore, we carry out a test to show that the financing channel is not the main driver for IPO underpricing. This test is based on how institutions provide capital to startups: as general partners (GPs) or as limited partners (LPs).[13] If the institutions' sole effect on startups is to provide capital, there should be little difference between the capital provided by institutions as general partners (GPs) and that provided by limited partners (LPs). We find that institutions' participation as LPs does not significantly affect IPO underpricing. Furthermore, as shown in Figure 10 of Ewens and Farre-Mensa (2018), there are simultaneous increases in the capital from private equity (PE) funds, corporate venture capital, and institutions. Given that institutions are usually not the major contributor of capital for startups, it is not likely that they reduce IPO underpricing via the incremental capital provision hypothesis.

While we focus on the aforementioned three mechanisms, we are aware that there could be other potential mechanisms that explain the relation between institutions' pre-IPO participation and IPO underpricing as well. However, we find little support for these mechanisms both theoretically and empirically. For example, one argument could be that IPO underpricing is reduced because, by providing incremental governance relative to VCs, institutions' pre-IPO participation improves the startup's transparency. However, given that institutions specialize in the public market, there is no theoretical foundation for why institutions would be able to provide services beyond VCs. In addition, as shown by Chernenko, Lerner, and Zeng (2017), institutions tend to have weaker cash flow rights, are less involved in corporate governance, and are under-represented on boards of directors in startups. Moreover, institutions typically hold large portfolios in the public market, and venture capital investment accounts for only a small portion of their portfolios, which limits their incentive to monitor startups. For example, the median ratio of amount invested in entrepreneurial firms to institutional public equity market holding is 0.1. Overall, there is little support for the governance hypothesis.

Another potential argument is that the association between IPO underpricing and institutions' pre-IPO participation is related to startups' unobserved preference concerning dispersed ownership structure. Specifically, it is likely that firms in which institutions participate may prefer a more concentrated

[13] When institutions act as GPs, they are directly involved in investing in startups; when institutions act as LPs, they provide funding to GPs. Note that "institutions" refers to public market institutional investors in this paper.

ownership structure. Following the argument on the relation between dispersed ownership and IPO underpricing (Booth and Chua, 1996), startups backed by institutions need not use underpricing extensively to ensure broad ownership. However, using propensity score matching and exploiting the 2003 mutual fund scandal have already addressed this concern. Furthermore, we directly compare the ownership concentration in the post-IPO markets for startups with and without institutions, and find that there is no significant difference between their ownership concentration (measured by Herfindahl index). Thus, this suggests that institutions' participation does not reflect startups' preference on ownership structure.

This paper sheds light on the intriguing phenomenon of institutional investment in private startups. This phenomenon is puzzling on both the supply side and the demand side. On the supply side, given that liquidating shares is difficult in primary markets, investing in startups is not compatible with institutions' liquidity requirement. This is particularly true at the present moment, when startups are staying private longer (Doidge, Karolyi, and Stulz, 2013, 2017; Gao, Ritter, and Zhu, 2013). On the demand side, given that the amount of private money from VC and PE funds has increased dramatically recently (Ewens and Farre-Mensa, 2018), startups do not necessarily need financing from institutions, which, unlike traditional VCs, do not specialize in nurturing startups.[14]

The literature provides some potential explanations on the supply side. First, it may be easier for institutions to find counter-parties (e.g., private capital) when liquidating shares in the primary markets these days. Indeed, the amount of private capital in startups has increased significantly owing to regulatory changes (e.g., the National Securities Markets Improvement Act of 1996) and technological improvements (Ewens and Farre-Mensa, 2018). Second, the prospect of high returns or diversification benefits in their primary markets could also be potential motivations for institutions.[15] In addition, Agarwal, Barber, Cheng, Hameed, and Yasuda (2018) provide evidence consistent with the strategic marking of startups by mutual funds. Aragon and Lindsey (2018) show that hedge funds exploit their stock selection skills in venture deals, and the venture experience is valuable for them since it predicts greater public equity alpha. However, the supply side alone does not justify the increase in institutions' involvement in startups. If startups do not need institutions, the financing from institutions does not necessarily

[14] Ewens and Farre-Mensa (2018) argue that startups' ability to finance their late-stage growth while remaining private has been facilitated by a marked increase in the supply of private entrepreneurial capital, both from traditional and non-traditional startup investors. In a similar spirit, Kwon, Lowry, and Qian (2017) argue that mutual fund investment allows startups to stay private longer.

[15] There is no direct evidence yet. Given the greater competition from private capital, whether the primary markets actually have high returns is an open question. Even if the primary market does provide a high return, whether this return is still high after adjusting for liquidity risk is not clear.

increase even if institutions are willing to invest in startups. Startups with abundant funding would only seek institutional funding if institutions could benefit them on dimensions other than capital. However, the literature provides no answer to the question of how institutions help startups grow. In fact, Chernenko, Lerner, and Zeng (2017) provide evidence that mutual funds appear to be less involved in the corporate governance of startups than traditional VCs are. This paper complements the existing studies by providing a novel demand-side explanation for institutional investment in startups. Specifically, we argue that institutions reduce IPO underpricing by playing the role of all-star analysts in the secondary market. Although we do not completely rule out all other potential mechanisms, we attempt to show that institutional investors' substitution for all-star analysts in the secondary market is a non-negligible factor in IPO underpricing.

This research also contributes to the literature on IPO underpricing. The literature offers several theories for IPO underpricing. For example, one strand of studies suggests that high-quality firms underprice their issues to signal their quality to the market (Allen and Faulhaber, 1989; Grinblatt and Hwang, 1989; Welch, 1989). A second strand of studies explains IPO underpricing as caused by information asymmetry between various parties (Rock, 1986; Benveniste and Spindt, 1989; Chemmanur, 1993). A third strand of the literature models how underwriters are associated with IPO underpricing (Hoberg, 2007; Liu and Ritter, 2011; Bajo, Chemmanur, Simonyan, and Tehranian, 2016). Among them, Liu and Ritter (2011) argue theoretically that underwriters' non-price dimensions of underwriting could generate excessive underpricing even with severe competition in the underwriting industry. Although underwriters play a crucial role in price discovery in IPOs (Aggarwal and Conroy, 2000), price stabilization in the aftermarket (Aggarwal, 2000), attracting investor attention (Bajo, Chemmanur, Simonyan, and Tehranian, 2016), and institutional allocations (Aggarwal, Prabhala, and Puri, 2002), our theoretical foundation starts from the non-IPO price dimension function of underwriters with all-star analysts in the secondary market. In particular, our argument builds on Liu and Ritter (2011), and we argue that institutions serve as a substitute for the secondary market services of the underwriters, which could reduce IPO underpricing.

Finally, we contribute to the literature on the role played by institutional investors in helping firms. There are three strands in this literature. First, institutional investors play an important role in the stock market by enhancing price discovery (Jiambalvo, Rajgopal, and Venkatachalam, 2002; Nagel, 2005; Boehmer and Kelley, 2009) and improving market liquidity (Rubin, 2007; Blume and Keim, 2012;

Anand, Irvine, Puckett, and Venkataraman, 2013). Second, institutional investors help public firms with corporate governance (Brav, Jiang, and Kim, 2010; Brav, Jiang, Ma, and Tian, 2018; Chen, Harford, and Li, 2007). Third, institutional investors play an important role during IPOs (Aggarwal, 2003; Chemmanur, Hu, and Huang, 2010). Our paper is closely related to the third strand of the literature, which examines institutions that participate in IPO allocation. We focus on one specific group of institutional investors: those that cross the border between the public and private markets to invest in pre-IPO startups. This unique setting allows us to examine how institutional investment benefit startups.

The rest of the paper is organized as follows. Section 2 describes our data and sample, and reports summary statistics. Section 3 presents our main empirical results. Section 4 examines mechanisms through which institutions' pre-IPO participation reduces IPO underpricing. Section 5 concludes the paper.

2 Data and Summary Statistics

2.1 IPO Data

We construct our data from several sources. First, we obtain our IPO-related variables from the SDC Global New Issues Databases. We consider U.S. IPOs from 1980 to 2015, excluding closed-end funds/trusts, depositary issues, dual-class IPOs, and unit IPOs (Loughran and Ritter, 2004). We also restrict our sample to common shares, ordinary shares, and class A common shares. We merge our IPO list with VentureXpert to identify VC-backed IPOs. Following prior studies on IPO underpricing (Megginson and Weiss, 1991; Hanley and Hoberg, 2010; Liu and Ritter, 2011), we require the IPO offer price to be at least five dollars and have more than three million dollars in total proceeds. We obtain IPO underwriter reputation, IPO firm founding dates (Loughran and Ritter, 2004), and IPO all-star analyst coverage (Liu and Ritter, 2011) from Prof. Jay Ritter's website.[16]

2.2 IPO Underpricing

Our primary dependent variable is the level of IPO underpricing, measured by the return from the offer price to the closing price on the first trading day (*Initial Return*). In the internet appendix, we also examine the effect of institutions' participation on IPO cost. We measure IPO cost using the gross underwriting spread, scaled by the gross proceeds dollar amount of issuance (*Gross Spread*) and the

[16] See: https://site.warrington.ufl.edu/ritter/files/2015/06/IPO-Analyst-Data-Online-1993-2009-2011-04-01.xls.

ratio of the net proceeds to the gross proceeds (*Proceed Retention*) following (Megginson and Weiss, 1991; Hanley and Hoberg, 2010).

2.3 Institutions' Participation

Our primary independent variable is the level of public market institutions' participation in pre-IPO startups. For each IPO startup we obtain a list of all investors from VentureXpert. We identify the public market institutions by matching investor names from VentureXpert to the Thomson Financial Institutional Holdings databases.[17] We cross-reference with the available information from the investor's website and the relevant financial websites, such as Bloomberg, to ensure accuracy. For each startup, we measure public market institutions' participation as the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors (*Institution Shares*) and the total number of institutions, scaled by the total number of investors (*Institution Numbers*).

2.4 Measure of Successful Exit

We consider a startup as having a successful exit if it goes public or is acquired. In particular, we measure exits for startups that receive a first round of VC investment from the beginning of 1980 to the end of 2012. Following the literature (Gompers and Lerner, 2000; Hochberg, Ljungqvist, and Lu, 2007; Nahata, 2008), we mark a company as written-off if the company is marked as written-off in SDC or has not exited as of July 2016 with at least four years of operation before that date.

2.5 Control Variables

We follow the IPO literature (e.g., Liu and Ritter, 2011) to construct a number of firm characteristics that are related to IPO underpricing. These control variables include a dummy variable indicating that the IPO firm is a technology firm (*Tech Dummy*), a dummy variable indicating that the IPO firm is associated with a top-tier underwriter (*Top-tier Dummy*), the fraction of the offer that is secondary shares being sold by pre-issue shareholders (*Secondary Fraction*), the natural log of the firm's age at IPO (*Ln(age)*), and the natural log of gross proceeds in millions of dollars (*Ln(Proceeds)*).[18]

[17] We identify the public market institutions among the VC investors using a matching program based on the Thomson Financial Institutional Holdings databases. For each VC investor, the program finds the longest common strings between the investor name and the 13-F institution names. We require that this common string has to be at least 90% of the average length of the two names to be considered a match.

[18] Since we only examine VC-backed IPOs, we define an underwriter with an updated ranking of nine or higher as a top-tier underwriter, rather than eight or higher as in (Liu and Ritter, 2011).

We also control for market condition at the time of the IPO, measured as the 30-day Market Return Prior to IPO (*Prior Market Return*). In addition, we control for *Lead VC Reputation*, measured as the dollar amount invested by a given VC in all startups over the previous three years, scaled by the total amount raised by all startups (*Lead VC Reputation*). We first define the lead VC as the VC with the earliest investment date. If multiple VCs qualify according to that criterion, then the one with the largest investment amount is designated as the lead VC. If multiple VCs qualify according to the first two criteria, the VC that participated in the most funding rounds is designated as the lead VC. We report the detailed variable descriptions in Appendix Table **??**. In empirical analysis, we standardize all continuous independent variables in our main tables.

2.6 Summary Statistics

Panel A of Table 15 reports summary statistics on our IPO sample, which consists of 1,898 VC-backed IPOs from 1980 to 2015. Nearly half of our sample consists of technology firms, and more than one-third of IPO firms are associated with a top-tier underwriter. The average issuing firm goes public at the age of 13 years and raises 78 million dollars. Of the 1,898 IPOs, 202 are backed by at least one institution. Of those 202 IPOs, the average IPO firm raises 110 million dollars at the age of 15 years.

Panel B reports the exit channels of startups. Overall, we find that institutions are more likely to participate in startups with final IPO exits. Specifically, 26.78% of the startups with institutions' participation go to IPO, while only 12.65% of other startups go to IPO. This difference is highly statistically significant. The contrast is much less salient in the M&A channel. While 40.50% of the startups with institutions' participation exit via M&As, 40.22% of other startups exit via M&As. This difference is not statistically significant. We also compare the exit channel of institution-backed startups and the startups without institutions' backing in Figure 5. In the majority of years, a higher fraction of institution-backed startups exit through IPOs than non-institution-backed startups; the contrast is smaller when we look at the M&A exit. These conclusions are robust when we formally test this result using a multinomial logistic model (see Internet Appendix Table A3).

We examine the timing of pre-IPO institutions' investments in Panel C. We identify the first investment an institution makes in a given startup and report the stage of the startup at the time of that investment. We find that institutions are more likely than non-institutions to invest at late stages. Specifically, 17.35% (34.7%) of institutions enter at early (late) stages, while 35.56% (26.3%) of non-institutions' investments are in early (late) stages. These differences are statistically significant. These results suggest that startups in their late stages or with forthcoming IPOs may seek investment from institutions. The strong relationship between institutions' pre-IPO participation and the probability of going public prompts us to focus our analysis on whether institutions benefit startups in the IPO process.

3 Institutions and IPO Underpricing

We first conduct our baseline analysis on how institutions' participation affects startups' IPO underpricing using a set of panel regressions in Section 3.1. To address endogeneity concerns, we conduct our analyses using the propensity score matched sample in Section 3.2. We further use the 2003 mutual fund scandal to draw a further causal inference on whether institutions' participation leads to reduced IPO underpricing in Section 3.3.

3.1 OLS Specification

We first investigate how institutions' participation in pre-IPO investments is associated with IPO underpricing by estimating the following panel regression model:

$$\textit{Initial Return}_i = \alpha + \beta \textit{Institution Participation}_i + \gamma Z_i + \textit{IPO Year FE} + \textit{Industry FE} + \epsilon_i, \quad (1)$$

where *i* is the index for the startup. The dependent variable in Eq. (1) is the first-day return of the IPO of startup i. Our main variable of interest is *Institution Participation*. We use two proxies to capture institutions' participation: *Institution Shares* and *Institution Numbers*. *Institution Shares* is the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors. *Institution Numbers* is the proportion of investors in the startup that are institutions. Z_i is a vector of controls that includes *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, and *Ln(Proceeds)*. We also control for IPO year and industry fixed effects and cluster standard errors by IPO year.[19] In our analyses, we standardize all continuous independent variables to facilitate interpretation of our results.

Table 16 reports estimates of various specifications of Eq. (1). Columns (1) and (2) present the results without IPO year fixed effects but with industry fixed effects, using *Institution Shares* and *Insti-*

[19]In the IPO literature, the Fama-French 49 industry group is often used for industry classification (e.g., Liu and Ritter (2011)). We adopt a coarser industry classification (Fama-French 12 industry group), because our sample size is relatively small. For example, there are 12 industries with fewer than 10 observations when we use the Fama-French 49 industry groups.

tution Numbers as independent variables, respectively. For *Institution Shares* and *Institution Numbers*, the coefficient estimates are -0.023 (t-stat = -2.55) and -0.022 (t-stat = -2.75). Columns (3) and (4) exhibit results without industry fixed effects but with IPO year fixed effects. For *Institution Shares* and *Institution Numbers*, the coefficient estimates are -0.015 and -0.014 (t-stat = -2.14 and -2.33). In columns (5) and (6), we include both IPO year fixed effects and industry fixed effects. Including both fixed effects increases R^2 to 28.5%, from an R^2 of 17.0% in columns (1) and (2) and an R^2 of 27.4% in columns (3) and (4). The coefficient of *Institution Shares* is -0.016 (t-stat = -2.29). The economic magnitude is sizable: a one standard deviation increase in *Institution Shares* reduces IPO underpricing by 1.6%, which accounts for 6.4% of the mean IPO underpricing in our sample. The coefficient estimate on *Institution Numbers* is -0.015 (t-stat = -2.14). The economic magnitude is similar to the previous point estimate: a one standard deviation increase in *Institution Shares* reduces *Initial Return* by 1.5%, which accounts for 6.0% of the mean IPO underpricing in our sample. In untabulated results, we find that, on average, the IPO underpricing of startups with institutions' participation is 3.4% lower than that of startups without institutions' participation. This magnitude is comparable to our own estimate of underpricing associated with top-tier underwriters (3.4%). It also amounts to a substantial fraction of the underpricing associated with all-star analyst coverage (10.0% in our sample).[20] We also find that institutions' participation has a more important impact on IPO underpricing when industry uncertainty (measured by industry return volatility and industry-level forecast error) is greater (see Internet Appendix Table A6). Overall, the results are consistent with our hypothesis that institutions' pre-IPO participation in startup financing significantly reduces startups' IPO underpricing.[21]

3.2 Propensity Score Matching

Although we include a comprehensive set of control variables in our prior analyses, including a large number of fixed effects, we do not completely shield our analyses from the endogeneity concern. We further use a propensity score matching methodology to avoid spurious results from endogenous matching between institutions and startups driven by observable characteristics. Following the procedure in Lemmon and Roberts (2010), we match the sample based on the following characteristics: *Ln (Number of Rounds)*, *Ln (Number of VCs)*, *Ln (Total Amount Raised)*, *Early-stage Dummy*, *Lead VC*

[20]Our results are not driven by lower trading prices at the end of the first trading day. We do not find a significant relation between *Institution Participation* and these firms' long-term returns.

[21]We also find that institutions' participation helps reduce other direct costs in the IPO process, such as gross spreads. Institutions' participation also increases proceeds retention. These results are reported in Table A4 of the internet appendix.

Reputation, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, and *Ln (Proceeds)*. In particular, we run a logistic regression of the institution-backing dummy on the aforementioned variables with industry and IPO year fixed effects. We construct the control group using the nearest-neighbor method. For each institution-backed startup, we use the five non-institution-backed startups with the closest propensity score as the control group.

We report the pairwise comparison for sample characteristics for the pre-match and post-match samples in Panel A of Table 17. In pre-match samples, we find that institution-backed startups tend to be larger in size with more financing rounds and a higher number of investors. They tend to be older and raise more proceeds at the IPO periods. In contrast, there are no significant differences in these characteristics in the post-match samples. We also conduct a logistic regression analysis to examine the characteristics of pre-match and post-match samples. In the logistic regression setting (reported in Panel B), we find that there are significant differences between institution-backed and non-institution-backed startups largely in line with those found in the pairwise comparison in Panel A. These differences become statistically insignificant after the propensity score matching procedure. In Panel C, we report the characteristics for the treatment sample and the five control observations. We find that the difference in the propensity score between the treatment sample and the control observations is minimal.

We repeat the regression specified in Eq. (1) with the propensity score-matched sample. These results are reported in Table 18. The key coefficients are similar to those reported in our panel regressions. For example, in specifications where we control for both IPO year and industry fixed effects, the coefficients for *Institution Shares* and *Institution Numbers* are -0.016 and -0.015 respectively in the panel regressions. Using the matched sample, these two coefficients are -0.018 and -0.020. Both are statistically significant at the 5% level. Thus, our main results are unlikely to be driven by observable startups characteristics, including their size and age.

3.3 Evidence from the 2003 Mutual Fund Scandal

To further address the endogeneity concern, we utilize the 2003 mutual fund scandal as a shock to the probability of institutions' participation. Fund families involved in the scandal suffered large and long-lasting negative net flows (up to three years; see McCabe (2009)). This setting has been used in prior studies as an exogenous outflow (Anton and Polk, 2014; Koch, Ruenzi, and Starks, 2016). For example, Anton and Polk (2014) argue that the scandal-induced outflow is unrelated to firm fundamentals.

In our setting, we argue that a negative net flow is likely to decrease the likelihood of a given institution's participation in startup financing rounds, but is unlikely to have any association with startup's fundamentals (e.g., startup quality).

We first construct a sample of potential deals in the spirit of Bottazzi, Da Rin, and Hellmann (2016) and Gompers, Mukharlyamov, and Xuan (2016). For each IPO startup, we select a set of institutions that could potentially participate in the startup financing rounds. An institution is deemed to be a potential investor if (1) the institution has invested in the private market before the given startup exits, and (2) at least one of the previous investments is in the same Fama-French 12-industry group as the given startup.

We then identify institutions that are involved in the 2003 scandal by their names.[22] For each institution-startup investment pair, we consider an investment to be affected by the scandal if (1) the institution is involved in the 2003 scandal, (2) the first financing round is earlier than the 3 year anniversary of the scandal, and (3) the startup has not exited the private market at the time of the scandal.

We analyze the impact of this shock in a two-stage setting similar to Chaney, Sraer, and Thesmar (2012) and Hombert and Matray (2016). In our first stage, we follow Bennedsen, Nielsen, Pérez-González, and Wolfenzon (2007) and estimate the participation probability using the following OLS regression specification:[23]

$$\textit{Institution Dummy}_{i,j} = \beta \textit{Scandal}_{i,j} + \gamma Z_i + \textit{Institution}_i + \epsilon_{i,j}, \tag{2}$$

where *Institution Dummy*$_{i,j}$ is a dummy variable. It equals one if institution i invests in startup j, and otherwise it equals zero. *Scandal*$_{i,j}$ is a dummy variable that equals one only if the potential investment from institution i to startup j is affected by the scandal, as defined above. We include the same set of control variables as in Table 16, as well as institution fixed effects. Standard errors are clustered at the institution level.

In our second stage, we use the specification of Eq. (1). However, we use the *predicted* values of the institutions' participation variable. Specifically, we calculate *predicted* institution numbers as follows:

$$\widehat{\textit{Institution Numbers}}_j = \frac{\sum_i \widehat{\textit{Institution Dummy}}_{i,j}}{\sum_i \widehat{\textit{Institution Dummy}}_{i,j} + \textit{Number of Non-institution Investors}_j}. \tag{3}$$

The results are reported in Table 19. We first estimate a regression that includes fixed effects and the scandal dummy. The results are reported in column (1), where we show that the scandal indicator

[22]Our mutual fund scandal involvement data come from Anton and Polk (2014).

[23]We use OLS instead of Probit regression to maintain the consistency of the two-stage estimator, even though *Institution Dummy* is a binary variable. Angrist and Krueger (2001) argue that using a nonlinear first stage regression may harm the consistency of the estimator.

has a significant negative coefficient, indicating that institutions involved in the 2003 scandal have a significantly lower probability of investing in startups. The coefficient implies a 0.5% lower likelihood for an institution affected by the scandal to invest in the average startup, and this estimate is significant at the 1% level. This essentially means that these funds have close to zero probability in making a startup investment.

Next, we conduct second-stage analyses and include the aggregated fitted value from the first-stage regression as an independent variable, using Eq. (3).[24] The corresponding second-stage regressions are reported in Column (1) of Panel B, where we show that $\widehat{Institution\ Numbers}$ has a significant and negative effect on IPO underpricing. This coefficient estimate of -0.018 is also in line with our findings in Table 16.

In addition, we use an alternative specification that includes additional controls in the first-stage specification. As shown in Panel A, Column (2), the coefficient of the scandal variable remains statistically and economically significant, indicating the robustness of our first-stage estimation. We report the corresponding second-stage estimation in Column (2) of Panel B, and the coefficient estimate is similar to our first specification.

Since the scandal in 2003 specifically affects a subset of institutions, namely mutual funds, we repeat our analyses with only mutual fund families as potential investors when creating the institution-startup pairs. These analyses are reported in Columns (3) and (4) of Panels A and B. In Panel A, the coefficient of scandal involvement is similar to our initial specification (a 0.5% decrease in probability of participation). In the second stage, we again aggregate the $\widehat{Institution\ Dummy}$ and calculate the predicted *Institution Numbers* in the deal. The corresponding second-stage regressions are reported in Columns (3) and (4) of Panel B. In the second-stage regression, we also find negative and significant coefficients for $\widehat{Institution\ Numbers}$. These coefficients are slightly lower than the first two regressions, but they remain statistically significant.

In summary, our previous analyses demonstrate that the relationship between institutions' participation and IPO underpricing is unlikely to be driven by startup and deal characteristics.

[24] We rely on $\widehat{Institution\ Numbers}$, as opposed to $\widehat{Institution\ Amount}$, since it is easier to interpret the coefficient. Our results are consistent if we predict the *Institution Amount* in the first stage and use the aggregated *Institution Amount* in our second-stage regression.

4 Possible Mechanisms

Our results suggest that institutions' pre-IPO participation in startup financing leads to lower IPO underpricing. We explore a number of potential mechanisms of this finding in this section. The first mechanism is based on Liu and Ritter (2011), who argue that startups' lust for all-star analyst coverage leads to underpricing. We explore whether institutions' pre-IPO participation could reduce startups' lust for all-star coverage and thus reduce IPO underpricing in Section 4.1. The second mechanism is based on the information asymmetry in the IPO process. We explore whether institutions' participation reduces information asymmetry, and thus lead to reduced underpricing in Section 4.2. The third mechanism is that institutions' participation could affect IPO underpricing by providing additional funding. We explore this mechanism in Section 4.3. Finally, we discuss a number of additional mechanisms in Section 4.4. While providing definitive evidence for a specific mechanism on how institutions' participation reduces IPO underpricing is challenging, we aim to provide a coherent explanation for all the empirical results.

4.1 Institutional Investment as a Substitute for All-Star Analyst

We first explore the all-star analyst substitution mechanism. Our theoretical foundation builds on Liu and Ritter (2011). Liu and Ritter (2011) show that traditional VCs care about post-IPO share prices when they distribute their shares to limited partners (LPs). Since all-star analysts are able to attract institutions and boost short-term firm value, startups with VC investors are willing to accept underpricing in order to compensate underwriters who provide all-star analyst coverage. We hypothesize that pre-IPO investments by institutions themselves help build an institutional ownership base. Their presence reduces the importance of all-star analyst coverage. Thus institutions' participation may reduce IPO underpricing.

To test this hypothesis, we first examine whether startups invested in by institutions before their IPOs tend to have higher institutions' ownership in the secondary market for an extended period of time. We then directly test the effect of institutions' participation on startups' IPO underpricing.

The baseline assumption of the analyst-lust theory in Liu and Ritter (2011) is that all-star analyst coverage could attract institutional ownership in the post-IPO market. Meanwhile, the substitution effect between pre-IPO institutions' participation and all-star analysts also lies on the assumption that institutions that invest in pre-IPO startups provide a stable investors base in the post-IPO market and thus their participation reduces the importance of all-star analysts. Before formally testing the substitution effect, we provide some descriptive results to justify these assumptions. First, we compare the post-IPO institutional ownership for companies with and without all-star coverage in Figure A1. Figure A1 shows that all-star covered firms indeed have much higher institutional ownership than those without all-star analyst coverage. Furthermore, we compare the post-IPO institutional ownership for companies with all-star coverage and with pre-IPO institutional investment in Figure 6. Figure 6 clearly shows that both startups with pre-IPO institutional investment and with all-star analyst coverage are associated with higher levels of post-IPO institutional ownership than startup with neither all-star coverage nor pre-IPO investment. Meanwhile, the levels of post-IPO institutional ownership for startups with pre-IPO institutional investment and all-star analyst coverage are statistically indistinguishable. These results indicate that pre-IPO institutional investment may substitute all-star analyst coverage in boosting post-IPO institutional ownership.[25]

Next, we provide tests on the substitution effect between institutions' investment and all-star analyst coverage. Our first test is based on institutions' tendency to keep the shares they acquired. For this analysis, we group institutions into dedicated and non-dedicated institutions based on the classification of Bushee (1998).[26] As argued in Bushee (1998), dedicated investors are long-term investors, and thus we conjecture that dedicated institutions are less likely to liquidate their acquired shares shortly after a firm's public listing. Non-dedicated institutions consist of transient institutions and quasi-indexers. Transient institutions tend to have high portfolio turnover, while quasi-indexers mainly focus on tracking a broadly diversified index. Since newly listed firms are unlikely to be included in a major index, quasi-indexers are unlikely to maintain meaningful positions in these startups after they go public.[27]

[25] On average, institutions that invest in startups prior to their IPOs own a higher number of shares than an average institutional owner who invests during or after an IPO. We report this result in Table A7. Institutions that provided pre-IPO financing hold a significantly higher fraction of shares than those that purchased shares during or after the IPO for at least three years. This difference is 0.15% in quarter 0 (the IPO quarter), and the gap persists in the three years after the IPO. Institutions' relatively stable ownership after an IPO is also different from that of traditional VCs, which tend to distribute startup shares to LPs after the lockup period (usually in six months to one year after the IPO; see (Liu and Ritter, 2011)).

[26] To make sure that dedicated and non-dedicated institutions' participation are comparable to each other, we do not standardize each separately. Instead, we scale dedicated and non-dedicated institutions' participation measures by the standard deviation of *Institution Participation* for a given startup. As a result, these coefficients are not directly comparable with those reported in Table 16.

[27] Although we do not have detailed information on how do institutions liquidate their shares when firms go pubic, in untabulated results, we find some suggestive evidence that dedicated investors indeed have a relatively longer horizon than non-dedicated investors. Specifically, dedicated and non-dedicated institutions contribute similar amounts of investment in pre-IPO financing. The difference between the pre-IPO investment by dedicated and non-dedicated institutions is statistically insignificant. However, dedicated institutions hold a much higher proportion of shares in startups in the secondary market. The

Therefore, if the all-star analyst substitution mechanism explains our result, we should observe that dedicated investors are the main contributor to the reduction in startups' lust for all-star analyst coverage. Consequently, they are most effective in reducing IPO underpricing.

To test this hypothesis, we separately investigate the effect of institutions' participation from dedicated and non-dedicated institutions on IPO underpricing. We use the following specification for this test:

$$\begin{aligned} \textit{Initial Return}_i = \alpha &+ \beta_1 \textit{Dedicated Institution Participation}_i \\ &+ \beta_2 \textit{Non-Dedicated Institution Participation}_i + \gamma Z_i + \textit{IPO Year FE} + \textit{Industry FE} + \epsilon_i, \end{aligned} \tag{4}$$

We report this set of results in Panel A of Table 20. While both dedicated and non-dedicated institutions are associated with lower IPO underpricing, this relationship is statistically significant only for dedicated institutions. We also find that dedicated institutions' participation leads to a more significant reduction in IPO underpricing. This result is consistent with the hypothesis that institutions with a low tendency to liquidate startup shares are more effective in reducing underpricing.

Next, we carry out a direct test of substitution between all-star analyst coverage and institutions' pre-IPO investments. First, Liu and Ritter (2011) find that all-star analyst coverage is particularly important for startups invested in by VC firms since venture capitalists care about share values when they distribute the shares to LPs in the post-IPO markets (usually six months to one year after the IPO). Since all firms in our sample have investments from VC firms, we expect that all-star analyst coverage should play an important role in IPO underpricing. Furthermore, if dedicated institutions can substitute for all-star analyst coverage, we expect that dedicated institutions' participation should weaken the relationship between all-star analyst coverage and IPO underpricing.

We test our hypotheses using the following specification:

$$\begin{aligned} \textit{Initial Return}_i = \alpha &+ \beta_1 \textit{Dedicated Institution Participation}_i \\ &+ \beta_2 \textit{Dedicated Institution Participation}_i \times \textit{All-Star Dummy}_i \\ &+ \beta_3 \textit{Non-Dedicated Institution Participation}_i \\ &+ \beta_4 \textit{Non-Dedicated Institution Participation}_i \times \textit{All-Star Dummy}_i \\ &+ \beta_5 \textit{All-Star Dummy}_i + \gamma Z_i + \textit{IPO Year FE} + \textit{Industry FE} + \epsilon_i. \end{aligned} \tag{5}$$

where $\textit{All-Star Dummy}_i$ is an indicator variable equal to one if the startup i is covered by an institutional investor all-star analyst (top 3) from the bookrunner within one year of the IPO. We report these results

difference in the secondary market becomes statistically significant. This indicates that dedicated institutions are less likely to liquidate shares after an IPO or dedicated institutions acquire more share in the post-IPO markets.

in Panel B of Table 20.[28] In columns (1) and (2), we find results consistent with Liu and Ritter (2011) that there is a significant negative relationship between the all-star dummy and IPO underpricing. Next, we interact *Institution Shares* of dedicated and non-dedicated institutions with *All-Star Dummy*. This result is reported in column (3) in Panel B of Table 20. We find that the coefficient of the interaction term between *Dedicated Institution Shares* and *All-Star Dummy* is negative (t-stat = -2.40). A one standard deviation increase in *Dedicated Institution Shares* reduces the effect of *All-Star Dummy* by 0.023, or more than 20% of the all-star analyst coverage coefficient. We also use the *Dedicated Institutions Numbers* and *Non-Dedicated Institutions Numbers* as proxies for participation. This result is reported in column (4) of Panel B in Table 20. We find that the coefficient is -0.062 (t-stat = -2.38). This result is consistent with the analysis using *Dedicated Institution Shares* as a proxy for dedicated institutions' participation. In contrast, the interaction term between all-star analysts and non-dedicated institutions has a positive and statistically insignificant coefficient. This result supports our hypothesis that only dedicated institutions are able to effectively reduce startups' reliance on all-star analyst coverage.[29]

We also conduct a placebo test. A unique prediction by Liu and Ritter (2011) is that the association between all-star analyst coverage and startups' IPO underpricing is only present when these startups are at least partially funded by traditional VCs, since traditional VCs care about share prices when they distribute their shares to LPs. To test this hypothesis, we exclude VC-backed IPOs from the Global New Issues Databases. Specifically, we merge the IPO firms with CRSP Mutual Fund Holding data to identify mutual fund pre-IPO participation in non-VC-backed IPOs.[30] As reported in Table 21, we find that there is no significant relation between IPO underpricing and mutual fund investment and that the coefficient estimate is positive.

Taken together, our results are consistent with the hypothesis that institutions' participation substitutes for all-star analyst coverage and results in reduced IPO underpricing for startups.

[28] The reduction in sample size is due to the limited availability of the all-star analyst coverage data.

[29] We also analyze whether institutions' participation reduces the underpricing associated with top-tier underwriters. This result is reported in Internet Appendix Table A5. Our results are consistent with institutions' participation reducing underpricing associated with top-tier underwriters. This finding confirms that institutions' participation may substitute for non-price dimension services bundled by top-tier underwriters.

[30] In this analysis, we focus on a subset of institutions, mutual funds, since they report holdings of investments in private firms in regulatory filings. This disclosure is not available in 13-F filings.

4.2 Institutional Investment and Information Asymmetry

The second potential mechanism for our result is that institutional investment certify startup firms' quality. For example, Benveniste and Spindt (1989) argue that underwriters induce informed investors to reveal their information in the book-building process with underpricing. One may argue that pre-IPO investments by institutions reveal (often favorable) information about startups. Thus their investments are associated with lower underpricing to reflect the reduced compensation for other IPO investors to reveal information in the IPO process (Hanley, 1993; Sherman and Titman, 2002). We conduct two empirical tests to examine this mechanism.

The first test is based on institutional investors' past performances in the public market. If the reduced IPO underpricing is a result of institutions' participation revealing information about startups, an investment from an institution with better understanding of the startup's industry should be a stronger signal to the market. Thus, their investment should lead to a greater reduction in IPO underpricing. We measure institutions' industry expertise using the performance of their stock holdings in a given startup's Fama-French 12 industry classification over the 24-month period prior to a startup's IPO.[31] Our empirical specification is as follow:

$$\begin{aligned} \textit{Initial Return}_i = \alpha &+ \beta_1 \textit{Institution Participation}_i + \beta_2 \textit{Institution Performance}_i \\ &+ \beta_3 \textit{Institution Participation} \times \textit{Institution Performance}_i \\ &+ \gamma Z_i + \textit{IPO Year FE} + \textit{Industry FE} + \epsilon_i . \end{aligned} \tag{6}$$

We use excessive returns, DGTW-adjusted returns, and industry-adjusted returns as proxies. This set of results is reported in Table 22. The interaction term between institutions' participation and return proxies is statistically insignificant. We further adjust our performance measure using Fama-French 12 industry portfolio returns. This adjustment ensures that we are measuring institutions' stock selection ability as opposed to exposure to industry returns. We also find that the interaction term of industry-adjusted performance and institutions' participation to be statistically insignificant. Overall, the reduction in IPO underpricing is not significantly affected by institutions' industry expertise.

The second test is to examine the offer price adjustment following Hanley (1993) and Hanley and Hoberg (2010). If our IPO underpricing result is driven by institutions' certification effect, institutions' participation in a startup could mitigate uncertainties before the startup's IPO. The lower uncertainties

[31] Alternative measures of performance based on 12-month or 36-month returns yield similar results.

due to institutions' participation should improve the accuracy of the proposed offering prices, and thus we should expect less adjustment in the offering prices during the IPO process.

Empirically, we examine this hypothesis using the following specification:

$$\Delta P_i = \alpha + \beta \textit{Institution Participation}_i + \gamma Z_i + \textit{IPO Year FE} + \textit{Industry FE} + \epsilon_i, \tag{7}$$

$$|\Delta P_i| = \alpha + \beta \textit{Institution Participation}_i + \gamma Z_i + \textit{IPO Year FE} + \textit{Industry FE} + \epsilon_i \tag{8}$$

where ΔP is the price adjustment from the filing date midpoint to the IPO price, scaled by the filing date midpoint. We report the regression results in Table 23. We find that both *Institution Shares* and *Institution Numbers* are not significantly related to either the level or the absolute value of the price adjustment. Thus our results are unlikely to be driven by institutions revealing proprietary information about the startup.

Additionally, if institutions have the ability to reduce IPO underpricing by certifying startups, we should observe a negative relationship between IPO underpricing and institutions' pre-IPO participation for both VC-backed and non-VC-backed deals but, as indicated in Table 21, there is little evidence for this. Taken together, the results do not lend sufficient support for the hypothesis that institutions' participation reveals startup information and leads to lower IPO underpricing.

4.3 Institutions as Pure Financing Providers

The third mechanism is that institutions' participation provides funding for startups in the private market. Although there is no direct connection between providing financing and underpricing, it is possible that institutions' participation in startup financing leads to startups to delay their IPOs, since they are able to gain sufficient financing in the private market. Thus these startups are more mature when they decide to go public, which is naturally associated with lower IPO underpricing.

We argue that this is also unlikely to be the main mechanism. For example, in the propensity score matching procedure, we have already matched firm age and amount of capital raised. In the post-match sample, we do not observe a significant difference in startups' age and financing amount between startups with and without pre-IPO institutional investments. Yet we are still able find that institutions' participation leads to lower IPO underpricing. Additionally, we carry out another placebo test based on institutional investment as LPs. If the only role institutions play is to provide capital, we should expect little difference between startups invested in by institutions as GPs and startups indirectly financed by institutions as LPs. The role of institutional LPs is limited to the provision of capital. Empirically, we use the regression specification of Eq. (1) and measure *LP Institution Participation* using investors with at least one institutional LP to test this hypothesis. Table 24 reports the placebo test results. Similar to the previous analysis, the dependent variable in our regression is *Initial Return*. We capture institutions' participation in VC deals as LPs by *LP Institution Shares* and *LP Institution Numbers*. The coefficient estimates are not significantly different from zero, indicating that institutions' *indirect* participation in VC deals as LPs does not reduce IPO underpricing. Additionally, Ewens and Farre-Mensa (2018) argue that there is a tremendous increase in private equity funds in startup financing. The scale of the startup investments from PEs seems to dwarf the investments from institutions. Given that institutions are usually not the main contributor of capital, our results are unlikely to be driven by institutions acting purely as providers of financing.

4.4 Additional Mechanisms

We discuss two additional mechanisms. First, startups may be better governed by institutional investors and thus have better transparency (e.g., with low information asymmetry). Second, institutions' participation and IPO underpricing may both be related to startups' preference for concentrated ownership (Booth and Chua, 1996).

4.4.1 Institutions and Startup Governance

One potential mechanism is that institutions may improve startups' governance. In the public market, institutions are generally regarded as actively involved in corporate governance (e.g., Gillan and Starks (2000)). If institutional investors are actively involved in startups' nurturing or governance, it is possible that their participation may lead to lower underpricing. However, there is a lack of support to this argument. First, institutional investors tend to hold many firms and their holdings in startups are only a small fraction of their portfolios. Thus institutions are unlikely to pour their resources into the governance of these startups (Fich, Harford, and Tran, 2015). Second, Chernenko, Lerner, and Zeng (2017) directly examine the governance mechanism and find that institutions generally have weaker cash flow rights, are less involved in corporate governance, and are under-represented on boards of directors in startups. Taken together, institutions do not seem to play an active role in startup governance.

4.4.2 Institutions' Participation and Ownership Concentration

Another potential mechanism for the IPO underpricing reducing effect of institutions is related to the ownership preferences of startups backed by institutions. This argument is based on Booth and Chua (1996). Specifically, Booth and Chua (1996) argue that IPO underpricing is a way for issuers to induce broader ownership structure in the secondary market. It is possible that startups with a preference for more concentrated ownership (i.e., closely held by a few institutions) may choose to be financed by institutions before IPOs. Since they do not have a preference for a broad ownership, they would not allow significant IPO underpricing. To examine the validity of this mechanism, we tabulate the ownership concentration in the secondary market for four quarters after the IPO from the 13-F in Internet Appendix Table A8. We find no statistically significant difference in ownership concentration (measured using Herfindahl index) between startups with and without institutional financing prior to IPOs. Thus, it is very unlikely that our results are driven by startups' differential preference ownership concentration.

5 Conclusion

Our study provides insight into the economic consequences of a new phenomenon that has attracted significant attention in academia and media recently: Public market institutions (e.g., mutual funds or hedge funds) that traditionally focus on the public market are increasingly investing in startups. Specifically, we study the economic consequence of institutions' pre-IPO participation on IPO underpricing. We have one novel empirical finding: Institutions' pre-IPO participation leads to lower IPO underpricing. Our further analysis shows that the reduction in IPO underpricing does not appear to be driven by the endogenous matching between startups and institutions. We focus on three potential economic mechanisms: 1. Institutions' substitution for the role of the analysts in the secondary market; 2. Institutions' certification effect for the startups; 3. Institutions as pure finance providers. Our results are more consistent with a substitution effect between institutions and all-star analysts. Specifically, our result suggests that institutions' participation improves the bargaining power of startups over the underwriters with all-star analysts. As a result, those underwriters' ability to underprice startup shares during the IPO process gets weakened.

Our study complements the nascent literature on institutional investment in startups by providing a novel demand-side explanation. We also contribute to the IPO underpricing literature by building on Liu and Ritter (2011) and arguing that institutions could substitute for all-star analysts in providing secondary market services. Finally, we add to the literature on the role of institutional investors by studying their benefits to private rather than public firms.

CEO vs. Consumer Confidence: Investment, Financing, and Firm Performance

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Abstract

We examine to what degree corporate managers take cues for investors. Using similarly constructed measures of CEO optimism and consumer optimism, our analysis provides evidence that, holding CEO optimism constant, CEOs substantially increase their capital expenditures and net financing when investors are more optimistic. CEOs, however, trade against investor optimism in their own personal trading accounts. And, while CEO optimism positively predicts firm performance, investor optimism negatively predicts firm performance and subsequent earnings surprises. Taken together, our findings suggest that investor beliefs strongly affect corporate investment; in particular, it appears that better-informed managers sometimes succumb to investor pressure or use times of high investor optimism to empire build.

Keywords: Investor Optimism, CEO Optimism, Corporate Investment.
JEL Classification: *G02, G10, G31, G32, G34*

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1. Introduction

The question to what degree corporate managers take cues from the stock market has occupied much of the finance literature of the last fifty years. On one hand, corporate managers may disregard investor opinions as corporate managers are the ones with the most complete information set to decide what is best in the firm's best interest. At the same time, it is plausible that shareholders sometimes possess pieces of insight that corporate managers, initially, are not aware of. Moreover, management has tendencies to squander corporate wealth when its interests are not well aligned with those of shareholders. Forcing managers to take cues from the stock market can help alleviate these agency problems.

The above question has become even more difficult with the advent of behavioral finance. One area of behavioral corporate finance literature examines what effects biased beliefs among investors have on firms' decision making (e.g., Baker, Stein, and Wurgler, 2003; Bergman and Jenter, 2007; Polk and Sapienza, 2013). A separate line of work examines the effects of biased beliefs among corporate managers themselves (e.g., Malmendier and Tate, 2005, 2008; Hirshleifer, Low, and Teoh, 2012; Ben-David, Graham, and Harvey, 2013). If biases manifest themselves primarily on the investor side, more rational/better informed managers should be given as much discretion and insulation from market pressures as possible (while ensuring that managers' primary objective is that of value maximization) so that managers can freely make decisions that are value-enhancing, yet unpopular with investors. An irrational manager view, on the other hand, prescribes that managers strongly respond to mostly rational market-price signals.

Our paper contributes to the above literature by estimating to what degree corporate managers take cues from investors and what their consideration are for paying attention to investor beliefs. While basic and fundamental, this question is challenging tackle empirically. The perhaps most straightforward strategy to quantify the degree to which corporate investment increases with investor optimism would be to estimate a regression equation of corporate investment on investor optimism. The difficulty with this approach is that investor optimism and CEO optimism likely are positively correlated with each other. To what degree any observed association comes from CEO optimism versus investor optimism is therefore unclear. To circumvent this problem, we thus require a measure of CEO optimism that is similarly constructed and, as such, comparable to the measure of investor optimism.

In this paper, we point to such a measure. Measures of consumer optimism, the most popular of which are those compiled by *The Conference Board* and the *University of Michigan*, are widely discussed in the financial press and closely followed by investors. They have also been used by numerous academic studies as measures of investor optimism (XXX). What is less known is that *The Conference Board* also publishes a measure of CEO optimism. This measure of CEO optimism is similarly constructed to that of consumer optimism and allows us to directly compare CEO optimism with consumer optimism and to disentangle their relative effects on corporate finance activities.

Empirically, we find that CEOs and consumers often disagree. The late 1990s, for instance, mark an episode when consumers were substantially more optimistic about economic conditions than CEOs. The mid-2000s, on the other hand, represent years during which CEOs were less pessimistic than consumers. Generally, we find that both optimism measures respond similarly to certain macroeconomic variables, such as inflation and GDP growth. However, consumer optimism is more closely tied to labor market conditions, whereas CEOs respond more strongly to corporate performance.

When including both CEO and consumer optimism in a dynamic panel regression equation, we find that the level of investment and the level of capital raised both increase with CEO optimism. In particular, our estimates imply that a one-standard deviation increase in CEO optimism comes with a 0.32% increase in future capital expenditure (scaled by total assets) and a 0.29% increase in future aggregate net dollar amount raised via debt and equity (scaled by total assets).

Perhaps more surprisingly, our results suggest that capital expenditure and capital raised also increase with investor optimism. The effects are similar in economic magnitude. In particular, holding CEO optimism constant, our regression analysis indicates that a one-standard-deviation increase in investor optimism comes with a 0.14% increase in future capital expenditure (scaled by total assets) and a 0.45% increase in future aggregate net dollar amount raised via debt and equity (scaled by total assets).

What are the channels through which the above patterns emerge? One possibility is that investor optimism reveals information about the marginal product of capital that CEOs initially did not consider. As CEOs subsequently condition on this piece of information, capital expenditures and capital raised increase. An alternative perspective is that well-informed CEOs have a tendency to empire build, which is easier to do when investors tend to be more optimistic (e.g., Fisher and Statman, 2003; Lemmon and Portniaguina, 2006). Even without empire-building tendencies, well-informed CEOs may feel pressure from investors to invest more when investors are highly optimistic. To differentiate between these alternative views, we examine how investor optimism relates to subsequent firm performance and trading decisions of CEOs in their own personal trading accounts.

We find that while CEO optimism positively predicts firm performance, consumer optimism negatively predicts firm performance. In particular, a one-standard-deviation increase comes with 0.45% lower ROA and 2.30% lower sales growth. Consumer optimism also strongly negatively predicts subsequent earnings surprises and stock returns, which suggests that the lower performance following periods of high investor optimism is not simply a reflection of the business cycle, but instead represents an unexpected negative shock to financial market participants. Our results are robust to research design choices and easily survive the inclusion of controls including variable capturing macroeconomic conditions.

Consumer optimism also strongly negatively associates with net share purchases by CEOs. That is, while CEOs expand their firms' operations during times of high consumer optimism, in their own personal trading account, CEOs aggressively trade against consumer optimism. When extending our analysis to other investor groups, we find that mutual fund flows positively associate with consumer confidence, suggesting that retail investors do not trade against consumer optimism. Instead, our finding supports prior studies that use consumer optimism as a proxy for retail investor optimism (Baker and Wurgler 2006). Mutual funds themselves also do not trade against consumer confidence, which should not surprise as we document that mutual funds receive more inflows during times of high consumer optimism. Versions of aggregate measures of order imbalance (buyer-initiated trades minus seller-initiated trades) using Trade and Quote (TAQ) data also fail to produce negative associations with consumer optimism. In the end, CEOs are the only group of market participants for which we find strong trading against consumer optimism. The most parsimonious explanation for these patterns is that well-informed CEOs recognize that periods of high investor optimism (relative to CEOs' own level of optimism) precede poor subsequent performances. At the same time, CEOs feel pressured to expand corporate operations when investor sentiment is high or use times of high investor sentiment as an opportunity to empire-build. These results favor a governance model in which managers are given as much discretion as possible (while ensuring that their primary objective is that of value-maximization) and corporate managers are insulated from sometimes irrational market pressures.

The rest of the paper is organized as follows. We describe the CEO and consumer optimism measures, as well as our methodology in Section 2. Section 3 examines the relation between CEO and consumer optimism and subsequent firm performance. Section 4 examines the relation between the optimism measures and corporate investment and financing activities. Section 5 examines the relation between optimism levels and insider trading. Section 6 concludes.

2. Data

In this section, we describe our data (Sub-Sections 2.1 – 2.4). Sub-Section 2.5 describes our research design.

2.1 CEO Optimism

We gauge CEO optimism using *The Conference Board* measure of CEO Optimism. The data series begins in 1976 and our sample ends in 2014. The CEO confidence survey is conducted every quarter with questionnaires mailed out in the second month of the quarter and responses flowing in throughout the remainder of the survey quarter. According to *The Conference Board*, questionnaires are mailed to 800 CEOs, of which between 80 and 100 CEOs respond. The companies surveyed are all members of *The Conference Board* and operate in one of the following ten industries: food/textiles/apparel, paper/printing/publishing, chemicals/petroleum/rubber, metal, machinery, utilities, wholesale/retail trade, banking/financing, insurance, and business services.

CEOs are asked to assess (1) current economic conditions vs. six months ago, (2) expectations for the economy, six months ahead, and (3) expectations for their own industry, six months ahead. For each of these three questions, CEOs are given five reply options: "substantially better" [Score=100], "moderately better" [Score=75], "same" [Score=50], "moderately worse" [Score=25], and "substantially worse" [Score=0]. *The Conference Board* computes the average score across all CEOs for each question, and then averages across the three questions to form the *CEO Confidence Index, CEO Optimism*. Theoretically, the measure can range from 0 to 100; in our sample period, the index ranges from 24 to 76.

To validate our measure, we compare the CEO confidence index with two other similar measures of executive expectations. We first consider the Global CFO Outlook Survey which begins in 2001 and is analyzed in Graham and Harvey (2007). The survey questionnaire is delivered quarterly to senior financial executives and subscribers of *CFO Magazine*, and respondents are asked whether they are more/less optimistic about the economy relative to the previous quarter. Analogous to our CEO confidence index, we calculate the CFO confidence index as the difference between the percentage of optimistic responses and the percentage of pessimistic responses. We find that the correlation between our CEO confidence index and the CFO confidence index is 0.87 (statistically significant at the 1% level).

A second alternative measure of management optimism is the *Philadelphia Fed's Business Outlook Survey*, in which manufacturing firms in the third Federal Reserve District (Pennsylvania, New Jersey, and Delaware) are asked to rate business conditions on a monthly basis. We find that the correlation between our CEO confidence index and the Fed's "future general business activity index" is 0.66 (statistically significant at the 1% level). The benefit of using the Conference Board data is that it provides a longer and (geographically and industry-wise) broader measure of CEO optimism; it is also constructed very similarly to our measure of consumer optimism. The CEO confidence index's high correlation with alternative measures of manager optimism helps build confidence in the data.

2.2 Consumer Optimism

We measure investor optimism using *The Conference Board* consumer confidence index. The survey is conducted by TNS, a market information group. The data have been used by Ludvigson (2004) and Lemmon and Portniaguina (2006), among others. On the first business day of the month, TNS mails questionnaires to a sample of 5,000 households; responses flow in throughout the survey month. The sample is representative based on key demographics and geographics as defined by the US Census Bureau and it is drawn from on original sample in which respondents agree to do the interviews. *The Conference Board* reports that of the 5,000 households, between 3,250 and 3,500 respond.[2]

Data are available every other month from 1967 to mid-1977, and every month thereafter. Our sample period begins in 1976 (based on the availability of CEO optimism) and ends in 2014. As the CEO confidence survey is conducted once every quarter with questionnaires being mailed out in the second month of the quarter (and responses flowing in throughout the remainder of the second month and the full third month of the survey quarter), we use consumer confidence data as of the third month of the quarter (i.e., March, June, September, and December). Using data from the second month of the quarter has very little impact on our results.[3]

From January 1981 to December 2014, *The Conference Board* also provides consumer confidence indices by geographic regions. Consumers are assigned to one of the following regions: New England, Middle Atlantic, South Atlantic, East North Central, West North Central, East South Central, West South Central, Mountain, and Pacific. Coval and Moskowitz (1999) find that investors exhibit a preference for locally headquartered firms. In our regression analysis, we take advantage of the region-level data to build a panel, which goes from January 1981 through December 2014. Specifically, we match each firm's

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[2] Questionnaires are mailed to a sample of 5,000 households that previously agreed to do the interviews, and as a result the overall response rate is unknown.

[3] From Feb 1976 to Feb 1977, when consumer surveys were still conducted bi-monthly, we use data as of February (2nd month of the quarter), June (3rd month), August (2nd month), December (3rd month).

headquarter location to the aforementioned seven regions and use the regional consumer optimism measures.[4]

Consumers are asked to assess (1) current economic conditions vs. six months ago, (2) expectations for the economy, six months ahead, (3) current employment conditions vs. six months ago, (4) expectations for employment conditions, six months ahead, and (5) expectations for total family income, six months ahead. For each of these five questions, consumers are given three reply options: “positive”, “negative”, or “neutral”. For each question, the positive figure is divided by the sum of the positive and the negative figure to yield a proportion. For each question, the average proportion for the calendar year 1985 is used as a benchmark to yield the index value for that question. The *Consumer Confidence Index*, *Consumer Optimism*, is the average of the index value for questions 1, 2, 3, 4, and 5. The optimism measure is set to equal 100 in calendar year 1985. Throughout our sample, the optimism measure ranges from 26.9 to 142.5.

A comparison of the questions used to construct CEO optimism with the questions posed in the consumer confidence survey reveals similarities: Both surveys ask participants to evaluate current as well as future economic conditions with a six-month time frame. The two major differences are that while CEOs are asked to evaluate industry conditions in their third question, consumers are surveyed about their employment condition and family income. Moreover, while CEOs are given five reply options (“substantially better”, “moderately better”, “same”, “moderately worse”, and “substantially worse”), consumers are given only three reply options (“better”, “same”, “worse”).[5]

Similar to our exercise with the CEO confidence index, we compare the consumer confidence index with an alternate measure of consumer optimism. In particular, we consider *The Survey of Consumer Sentiment* carried out by the Survey Research Center at the University of Michigan. We find that the correlation between these two consumer confidence indices is 0.80 (statistically significant at the 1% level).

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[4] Our findings are similar but slightly less robust when using national (not region-matched) consumer optimism.

[5] Another difference is that, unlike the values in *The Conference Board* measure of CEO Optimism, the response proportions used in the Consumer Optimism Index are seasonally adjusted. However, *The Conference Board* notes that the consumer optimism “series are typically not highly seasonal” (*The Conference Board*, 2011, p. 3). Correspondingly, we observe little seasonality in *The Conference Board* measure of CEO Optimism.

2.3 Macroeconomic Data

We use the following macroeconomic variables in our study: dividend yield, which is the total ordinary cash dividend of the CRSP value-weighted index over the last four quarters, divided by the index value at the end of the current quarter (Fama and French, 1988); default spread, which is the difference between the yields-to-maturity on Moody's Baa-rated and Aaa-rated bonds; the yield on three-month Treasury bills; GDP growth measured as 100 times the quarterly change in the natural logarithm of GDP; consumption growth measured as 100 times the quarterly change in the natural logarithm of personal consumption expenditures; labor income growth measured as 100 times the quarterly change in the natural logarithm of labor income (= total personal income minus dividend income, per capita), deflated by the PCE deflator; unemployment rate; inflation rate from CRSP, cumulated over the most recent three months; the consumption-to-wealth ratio from Lettau and Ludvigson (2001); as well as corporate profitability measured as corporate profits after tax (without Inventory Valuation Adjustment and without Capital Consumption Adjustment). The data sources for the macroeconomic variables are the Board of Governors of the Federal Reserve System, the US Department of Commerce, and the US Department of Labor.

2.4 CEO versus Consumer Optimism

Table 25 reports descriptive statistics for our optimism measures and the macroeconomic variables. Figure 7 plots national aggregate CEO optimism against national aggregate consumer optimism. By construction, the scales of CEO and consumer optimism are different. To facilitate comparisons, we standardize both measures. In particular, we compute the average and the standard deviation of each series over the 1976 to 2014 sample period. We then subtract from each quarterly observation its average and we scale the difference by its standard deviation.

The plot reveals that CEOs and consumers periodically agree and disagree. For example, during the late 1970s and the late 2000s both CEOs and consumers were pessimistic. On the other hand, there was striking disagreement during the late 1990s when consumers were substantially more optimistic about economic conditions than CEOs. Following the recent financial crisis, CEOs show more optimism than consumers.

We characterize CEOs' and consumers' reactions to macroeconomic conditions in Table 26 by estimating regression equations of CEO (consumer) optimism on lagged realizations of macroeconomic variables. We observe that high GDP growth precedes both high CEO and high consumer optimism, whereas high inflation precedes low CEO and low consumer optimism. While CEOs and consumers respond similarly to past GDP growth and inflation, consumers respond much more positively to consumption and labor market conditions (e.g. low unemployment and high labor income growth), whereas CEOs' optimism relates more positively to past realizations of corporate profitability. As strong corporate performance does not necessarily imply good labor market conditions, this provides a partial explanation for why sometimes CEOs and consumers differ in their optimism levels. In our analysis, we explore how these differences relate to corporate finance activities and firm performance.

2.5 Sample and Methodology

We study a large unbalanced panel of 12,716 firms from 1981 through 2014. Following prior literature (e.g., Fama and French, 2005), we exclude financial firms (i.e., firms with a one-digit SIC of 6) as well as utility firms (i.e., firms with a two-digit SIC of 49). Otherwise, our sample includes all CRSP/Compustat firms with data on performance.

Our dynamic panel regression has the following setup:

$$y_{i,t} = \rho y_{i,t-1} + \beta_1 CEO\_Conf_{t-1} + \beta_2 Cons\_Conf_{r,t-1} + \delta X_{i,t-1} + \Gamma Z_{t-1} + \alpha_i + \epsilon_{i,t},$$

where $y_{i,t} \epsilon \{ROA, Sales\ growth, investment, issuance\}$. CEO_Conf_{t-1} and $Cons\_Conf_{r,t-1}$ are the lagged CEO and regional consumers' confidence. $X_{i,t-1}$ is a matrix of firm-specific control variables, including *Firm Size*$_{i,t-1}$, which is the market value of equity prior to the beginning of firm *i*'s fiscal year, $Q_{i,t-1}$, which is total assets plus the market value of equity (at fiscal year-end) minus the book value of equity (Compustat SEQ), scaled by total assets, and *Cash_Flow*$_{i,t-1}$ (measured as Income Before Extraordinary Items plus

Depreciation and Amortization, scaled by previous total asset). Z_{t-1} is a matrix of macroeconomic variables described in section 2.3. Lastly, α_i is the firm fixed effect.

The presence of $y_{i,t-1}$ and α_i causes bias in the coefficients (Nickell (1981). To address this problem, we employ Helmert's transformation or orthogonal deviation suggested by Arellano and Bover (1995), to remove fixed effects, α_i[6]. Specifically, consider a random variable x_t, orthogonal deviation subtracts the mean of all future observations from x_t. Let $\tilde{x}_t$ denote the transformed x_t. Then $\tilde{x}_t = c_t\left[x_t - \frac{x_{t+1}+x_{t+2}+\cdots+x_T}{T-t}\right]$, where $c_t = \sqrt{(T-t)/(T-t+1)}$.

To estimate our regression we rely on System Generalized Method of Moments (GMM) developed by Blundell and Bond (1998) and Arellano and Bover (1995).[7] System GMM stack transformed equation and untransformed equation together. This can be achieved by pre-multiplying our panel regression equation by $\begin{bmatrix} M \\ I \end{bmatrix}$, where M is the orthogonalization matrix. In the transformed equation, we use second and third lagged levels, $y_{i,t-2}$ and $y_{i,t-3}$ as an instrument for $\tilde{y}_{i,t-1}$. In the untransformed equation, we use second and third lagged difference, $\Delta y_{i,t-2}$ and $\Delta y_{i,t-3}$ as instrument for $y_{i,t-1}$.[8]

There are two key exogeneity assumption need to be fulfilled for our instruments to be valid. First, lagged level, $y_{i,t-2}$ needs to be orthogonal to the current transformed shocks. That is,

$$E\left[y_{i,t-2}\tilde{\epsilon}_{i,t}\right] = E\left[y_{i,t-3}\tilde{\epsilon}_{i,t}\right] = 0.$$

Second, lagged difference, $\Delta y_{i,t-2}$ needs to be orthogonal to the untransformed shocks. That is,

$$E\left[\Delta y_{i,t-2}\left(\alpha_i + \epsilon_{i,t}\right)\right] = E\left[\Delta y_{i,t-3}\left(\alpha_i + \epsilon_{i,t}\right)\right] = 0.$$

We do not suspect any violations in either exogeneity assumptions. First of all, lagged dependent variables are unlikely to be correlated with future shocks. Second of all, the correlations between our dependent variables (performance, investment, and issuance) are likely to be constant over time. Due to the

[6] The orthogonal deviations is also used by Love (2003), Gilchrist and Himmelberg (1999), and Bond and Meghir (1994) among others.
[7] An alternative method is the Di_erence Generalized Method of Moments (GMM) developed by Arellano and Bond (1991). We rely on the System GMM to gain e_ciency. See Beck, Levine, Loayza (2002) for a more detail discussion
[8] See Roodman (2006) for a detailed discussion.

nature of orthogonal deviation, our transformed firm-specific control variables can be correlated with transformed errors. Similar to dependent variables, we also lagged levels as instruments for firm-specific control variables.

We test the validity of the instruments with two tests suggested by Arellano and Bond (1991). First, we reply on the Hansen test of over-identification. Hansen test yields a *J*-statistic following χ^2 distribution under the null hypothesis of valid instruments.[9] All results from dynamic panel regression yield J-statistics that fail to reject the null hypothesis of valid instruments. Second, we test for autocorrelation in the error terms. Specifically, we test for second-order autocorrelation with first-difference errors. Again, all results fail to reject the null hypothesis of no autocorrelation in the error terms.

3. Optimism and Firm Performance: CEOs and Consumers

We examine the relation between CEO and consumer optimism and firm performance using two accounting-based measures. Our first measure is return on assets (Compustat EBIT / (AT+lagged(AT)/2), and the second measure is annual growth in sales (Compustat SALE / lagged(SALE)). To quantify the effect of readability on firm value, we follow the system GMM outlined above. To distinguish the channel through which CEO and consumer optimism are associated with firm performance, we include various macroeconomic variables. We consider the following nine macroeconomic variables as in Lemmon and Portniaguina (2006): Dividend yield, default spread, the yield on three-month Treasury bills, GDP growth, consumption growth, labor income growth, unemployment rate, the inflation rate and the consumption-to-wealth ratio from Lettau and Ludvigson (2001).

Optimism can be tied to macroeconomic variables for both rational and irrational reasons. CEOs and Consumers may correctly assess economic conditions and report their “true” level of optimism. Alternatively, consumers and executives may over-extrapolate economic conditions and report optimism levels that are either too pessimistic or too exuberant. In either scenario, reported levels of optimism are

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[9] We prefer Hansen test over Sargan test due to the potential heteroskedasticity, which cause Sargan statistic to be inconsistent (Roodman, 2009).

correlated with economic conditions. By partialling out the component of optimism and of firm performance that is tied to specific macroeconomic variables, we are able to shed light on which aspects of economic conditions CEOs use in their decision making process.

Table 27 reports the estimates for ROA. We find a positive (negative) relation between CEO (consumer) optimism and subsequent ROA. When we do not include the macroeconomic variables in the regression equation, the coefficient estimate implies that a one-standard-deviation increase in *CEO Optimism* comes with 0.6% (*t*-statistic = 5.85) higher future ROA. In contrast, a one-standard-deviation increase in *Consumer Optimism* comes with -0.45% (*t*-statistic = -6.19) lower future ROA. To put these numbers in perspective, the median ROA in our sample is 6.95%.

The positive (negative) relation between CEO (consumer) optimism and ROA remains when including the macroeconomic variables. The magnitude of the coefficient estimates, however, decreases. A one standard deviation increase in *CEO Optimism* is now associated with a 0.49% (*t*-statistic = 4.66) increase in future ROA, whereas a one standard deviation increase in *Consumer Optimism* is now associated with a -0.24% (*t*-statistic = -2.90) decrease in future ROA.

The decrease in the magnitude of the coefficient estimates implies that part of the relation between optimism and future ROA is due to macroeconomic conditions. For example, good labor market conditions boosts consumer optimism, parts of which may be rational and parts of which may be irrational. At the same time, good labor market conditions are negatively associated with future ROA. Consequently, once "controlling" for labor income growth, the relation between consumer optimism and future ROA weakens.

We make similar observations for CEO and consumer optimism and future sales growth (Table 28). Sales growth marginally increases with CEO optimism, yet strongly decreases with consumer optimism. When we do not include our macroeconomic variables, the estimate for *CEO Optimism* is 0.57 (*t*-statistic = 1.06) and the estimate for *Consumer Optimism* is -2.30 (*t*-statistic = -3.71). These numbers imply that a one-standard-deviation increase in CEO (consumer) optimism is associated with a 0.57% increase (2.30% decrease) in future sales growth. The median sales growth in our sample is 7.92%. As shown in Table 28, the sales growth results are similar when we include our macroeconomic variables as additional independent variables.

3.1 Optimism and Subsequent Earnings Surprises

A potential interpretation of the performance results is that CEOs and consumers are confident at different points in the business cycle, which naturally reflect different performance levels. If high consumer optimism leads not only to lower sales growth and ROA, but also to *disappointing* cash flows, then the negative relation between consumer optimism and firm performance is unlikely to be generated by business cycle effects alone.

We test this channel by analyzing the relation between CEO and consumer optimism and subsequent earnings surprises. We obtain information regarding sell-side analyst earnings forecasts from the *Institutional Brokers Estimate System* (IBES) unadjusted U.S. detail history file, which tracks all historical (i.e., not-split-adjusted) actual EPS and all historical EPS forecasts made by each analyst.

We first assess how CEO and consumer optimism relate to analyst earnings forecasts. We compare, for each analyst/firm pair, the most recent forecast for the upcoming annual earnings announcement as of calendar quarter t with that as of calendar quarter t-1. We label this difference between the forecast as of quarter t and the forecast as of quarter t-1 the analyst's "forecast revision". We compute, for each firm, the average forecast revision across all analysts covering the firm in question and we divide the average by the stock price as of the end of quarter t-1; we then average this variable across all firms to obtain an aggregate forecast revision variable.

When regressing aggregate forecast revisions from quarter t-1 to quarter t on changes in optimism from quarter t-1 to quarter t, we obtain coefficient estimates for *CEO Optimism* and *Consumer Optimism* of 0.07 (t-statistic = 3.13) and 0.08 (t-statistic = 1.67), respectively. That is, changes in earnings forecasts positively associate with changes in CEO and consumer optimism.

Are these changes warranted by fundamentals? We first compute for all annual earnings announcement the difference between the actual EPS and the EPS consensus forecast, scaled by the stock price as of the forecast period end date ("earnings surprise"). We then estimate the following regression equation:

$$ES_t = \alpha + \beta_1 CEO\_Conf_{t-1} + \beta_2 Cons\_Conf_{t-1} + X\delta + e_t. \quad (2)$$

The dependent variable is either the value-weighted average earnings surprise across all firms over the following *one* year or the value-weighted average earnings surprise across all firms over the ensuing *three* years, starting from calendar quarter *t*. The independent variables include *CEO Optimism*, *Consumer Optimism*, past earnings surprises and past value-weighted stock market returns, as well as our set of macroeconomic variables. Standard errors are Newey-West adjusted with four or twelve lags.[10]

Table 29 reports the results. In Panel A, we study one-year earnings surprises. We find that consumer optimism negatively predicts future earnings surprises. In particular, a one-standard-deviation increase in *Consumer Optimism* comes with -0.19% (t-statistic = -2.71) or -0.33% (*t*-statistic = -4.08) more negative earnings surprises, depending on whether we account for macroeconomics variables or not. In contrast, *CEO Optimism* is not statistically significantly related to future earnings surprises. In Panel B, we find similar, but slightly weaker results for three-year earnings surprises.

Our evidence suggests that analyst forecasts tied to CEO optimism are warranted by fundamentals and eventually met by actual earnings (hence, the lack of predictability). On the other hand, forecasts tied to consumer optimism are not fully warranted by fundamentals. As a result, episodes of high consumer optimism come with earnings forecasts that are too high and that are subsequently missed by actual earnings.

3.2 Optimism and Subsequent Stock Market Performance

As a natural extension, we also analyze how optimism relates to future stock market performance. Following Greenwood and Shleifer (2014), we use value-weighted one-year and three-year stock market returns in excess of the risk-free rate. The independent variables include *CEO Optimism*, *Consumer*

[10] The results are robust when using equal-weighted earnings surprises and forecast revisions as well as constructing earnings surprises and forecast revisions using mean forecasts.

Optimism, as well as lagged non-overlapping value-weighted stock market returns and macroeconomics variables. Standard errors are computed using Newey-West with four and twelve lags, respectively.

Consistent with our earnings-surprise results, the results in Table 30 reveal that consumer optimism negatively predicts future stock returns. For the three-year horizon, for instance, the coefficient estimate for *Consumer Optimism* is -14.25 (*t*-statistic = -3.90) and -8.19 (*t*-statistic = -1.66) with and without the macroeconomic variables, respectively. As before, the results for CEO optimism are weaker.

Taken together, our evidence suggest managers accurately assess future economic conditions as their views are in line with subsequent firm performance. High consumer optimism, on the other hand, tends to be "contradicted" by low future firm performance. Our earnings surprise and stock return results suggest that the negative relation between consumer optimism and future firm performance more likely reflects negative cash flow shocks than business cycle effects. Any positive sentiment tied to high consumer optimism, thus, appears more susceptible to over-optimism.

4. Optimism and Corporate Finance: CEOs and Consumers

In this section, we investigate how CEO and consumer optimism relate to measures of investment and financing activity.

4.1 Investment

We examine the relation between CEO and consumer optimism and future corporate investment using two measures of investment. Our investment measure is the aggregate dollar amount of capital expenditure for firm *i* during fiscal year *t* (Compustat CAPX), scaled by the lagged book value of total assets (Compustat AT).

Our baseline regression equation is motivated by Baker, Stein and Wurgler (2003):

$$Inv_{i,t} = \alpha_i + \beta_1 CEO\_Conf_{t-1} + \beta_2 Cons\_Conf_{r,t-1} + X\delta + e_{i,t}, \quad (3)$$

where *i* indexes firms, *t* denotes the fiscal year and *r* denotes the region of firm *i*. $Inv_{i,t}$ is capital expenditure of firm *i* in fiscal year *t*. α_i represents firm-fixed effects. As in the previous analysis, *CEO* $Optimism_{t-1}$ and

Consumer Optimism$_{r,t-1}$ reflect the standardized, most recent levels of optimism prior to the beginning of firm *i*'s fiscal year. We include the same set of controls as in equation (1). We implement the Arellano-and-Bover-system GMM and we construct our standard errors to be robust to heteroskedasticity and arbitrary forms of serial correlations as well as clustering by fiscal year.

As reported in Table 31, the regression produces strong positive and significant slopes on the optimism measures with or without including macroeconomic variables. When we do not include macroeconomic variables, the estimate on *CEO Optimism* equals 0.32 (*t*-statistic = 4.44), implying that a one standard deviation increase in CEO optimism leads to a 0.32% increase in scaled capital expenditure. The estimate on *Consumer Optimism* equals 0.14 (*t*-statistic = 3.34), implying that a one standard deviation increase in consumer optimism translates to a 0.14% increase in scaled capital expenditure. These effects are economically meaningful relative to the median level of scaled investment of 4.48%.

When including our set of macroeconomic variables, our results imply that a one standard deviation increase in *CEO Optimism* is associated with a 0.14% (*t*-statistic = 3.60) increase in future scaled capital expenditure, whereas a one standard deviation increase in *CEO Optimism* is associated with a 0.11% (*t*-statistic = 2.16) increase in future scaled capital expenditure.

4.2 Financing

Given the rise in investment activity associated with high levels of CEO and consumer optimism, it is natural to assess whether increases in optimism also lead to greater external financing activity. We explore the connection between optimism and issuance by computing the aggregate dollar amount raised through both debt issuance and external equity issuance by firm *i* during fiscal year *t*, scaled by the lagged book value of total assets.

We define *Debt Issuance* as the difference between (a) "Debt – Due in 1st Year" (Compustat DD1) plus "Long-Term Debt – Total" (Compustat DLTT) in fiscal year *t* and (b) the sum of DD1 and DLTT in fiscal year *t*-1. *External Equity Issuance* is computed as in McKeon (2013): The backbone is the "Sale of Common and Preferred Stock-"variable (Compustat SSTKY) from the Compustat Fundamentals Quarterly file. SSTKY is a year-to-date figure. To compute quarterly common share issuances for quarters 2, 3, and 4, we thus subtract the SSTKY from the previous quarter. We also subtract proceeds from preferred shares (increases in PSTKQ, or PSTKRQ when missing). Negative values or missing values are set equal to zero.

Quarterly common share issuance captures both internal and external equity issuances. Based on hand-collected data, McKeon (2013) makes the observation that in virtually all cases for which quarterly common share issuances scaled by market capitalization exceeds three percent, equity issuances represent external equity issuances. We therefore assume that anytime scaled quarterly common share issuances exceed three percent, shares issuances in that quarter were external equity issuance. *External Equity Issuance* is the sum of the quarterly external equity issuances.[11]

Our analysis is organized around the following regression equation:

$$Fin_{i,t} = \alpha_i + \beta_1 CEO\_Conf_{t-1} + \beta_2 Cons\_Conf_{r,t-1} + X\delta + e_{i,t}, \tag{4}$$

where *i* indexes firms, *t* denotes the fiscal year and *r* denotes the region of firm *i*. $Fin_{i,t}$ is the sum of *Debt Issuance* and *External Equity Issuance* of firm *i* in year *t*, scaled by its lagged total assets. *CEO Optimism*$_{t-1}$ and *Consumer Optimism*$_{r,t-1}$ reflect the most recent levels of optimism prior to the beginning of firm *i*'s fiscal year, and they continue to be standardized with mean zero and unit standard deviation to facilitate interpretation. We use the same set of independent variables as in regression equation (1) and use the system GMM approach of Arellano and Bover (1995), where standard errors are clustered by fiscal year and are robust to heteroskedasticity and arbitrary forms of serial correlation.

The results in Table 32 show that external financing significantly increases with both CEO and consumer optimism. Based on our estimated coefficients, a one standard deviation increase in *CEO Optimism* leads to 0.29% more net issuance (*t*-statistic = 1.97), which compares to an average scaled net issuance of 6.52% in our overall sample. Consumer optimism is also positively related to issuance, with a one standard deviation increase in *Consumer Optimism* corresponding to a 0.45% more net issuance (*t*-

[11] Please see the Appendix in McKeon (2013) for a more detailed description of the variable construction.

statistic = 3.20). The positive relation between optimism and net issuance weakens when accounting for macroeconomic variables.

In untabulated analyses, we also explore whether CEO and consumer optimism associate with firms' decisions to issue equity relative to debt by constructing *Equity-Ratio*, which is the ratio of external equity-issuance over total issuance, and using it as the dependent variable in regression equation (4). We find no significant relation between *Equity-Ratio* and either measure of optimism. Our findings suggest CEO and consumer optimism positively predict the level of financing, but there is no consumer or CEO optimism-based market timing.

Taken together, we find that the level of investment and financing increases not only with the level of CEO optimism, but also with the level of consumer optimism. There are two possible interpretations. First, CEOs like to empire build, which is easier to do when consumer optimism is high and investors tend to be optimistic. Second, CEOs are misled by high consumer optimism and expand the firm without realizing that times of high consumer optimism are not necessarily an opportune time to grow the firm. To differentiate between these two views of the data, we examine the relation between optimism and insider trading in the next section.

5. Optimism and Insider Trading: CEOs and Consumers

The previous sections document a positive relation between CEO and consumer optimism and corporate expansion yet contrasting relations with firm performance. In particular, the negative relation between consumer optimism and earnings surprises/stock market returns suggests that market participants are too optimistic during time of high consumer optimism. If CEOs recognize consumer over-optimism, they may benefit from the reduced resistance to empire building and increase firm investment accordingly. In addition, more rational CEOs may also benefit at the personal level by trading against investor over-optimism. For example, Jenter (2005) finds that managers' private portfolio decisions are consistent with market timing behavior, with insider purchases being more likely among low valuation firms and less likely among high valuation firms.

In line with our earnings-surprise and stock-market-return regression equation (2), we estimate the following time-series regression equation to assess the relation between CEO and consumer optimism and insider trading:

$$Ins_t = \alpha + \beta_1 CEO\_Conf_{t-1} + \beta_2 Cons\_Conf_{t-1} + X\delta + e_t \tag{5}$$

The dependent variable is the aggregate value of shares purchased minus the aggregate value of shares sold across all CEOs, value-weighted by firms' lagged market capitalization. *CEO Optimism*$_{t-1}$ and *Consumer Optimism*$_{t-1}$ denote the standardized, one-quarter lagged CEO and consumer optimism prior to the insider transactions. As before, we control for the past dependent variable and past value-weighted market returns. Standard errors are Newey-West adjusted with four lags.

The results are presented in Table 33. The estimate on *Consumer Optimism* equals -1.42 (*t*-statistic = -2.12) or -2.31 (*t*-statistic = -2.08), depending on whether we account for macroeconomics variables or not. This suggests that CEOs tend to sell after periods of high consumer optimism. The estimate on *CEO Optimism* is 0.40 (*t*-statistic = 0.70) or 1.28 (*t*-statistic = 1.17), providing suggestive evidence that CEOs not only trade against investor optimism, but also tend to trade based on their own outlooks about the economy.

6. Conclusion

Consumer optimism is widely discussed in the financial press and generally treated as an important piece of economic information. Many brokerage firms forecast its movements, and it has been the subject of many academic studies (e.g. Ludvigson, 2004; Lemmon and Portniaguina, 2006). Our study is the first to examine a similarly constructed measure of CEO optimism, and we examine the relation between aggregate measures of CEO and consumer optimism and corporate finance activities, firm performance, and insider trading.

We find that CEO optimism positively predicts future ROA. In contrast, consumer optimism is negatively related to future ROA, future sales growth, subsequent earnings surprises and subsequent stock market performance. Despite their opposite predictions regarding future performance, we find that capital expenditure and financing all increase with CEO and consumer optimism. Further, we find that CEOs sell shares in their personal accounts during periods of high consumer optimism.

Our study makes an important incremental step in the literature on behavioral corporate finance by considering the possibility of irrational behavior by both CEOs and investors. Baker and Wurgler (2012) argue that research that emphasizes behavioral biases among investors has different corporate governance implications than research that focuses on manager biases. An irrational investor view prescribes that managers' incentives be aligned, but also suggests that more rational/better informed managers be given the flexibility to make decisions that are unpopular with investors. An irrational manager view, on the other hand, prescribes that managers strongly respond to rational market-price signals with limited managerial discretion. These differing normative implications necessitate research on their relative importance, and our findings provide initial evidence on this matter.

Our results imply that corporate expansions supported by shareholders during times of high sentiment subsequently lead to disappointing results. Corporate expansions tied to CEO beliefs, on the other hand, appear to be mostly warranted by fundamentals. In our particular setting, managers thus appear to be the more informed albeit their incentives are not fully aligned with those of shareholders

Table 1: Summary Statistics

This table presents the characteristics of the viewing and filing institutional investors. Panel A reports institution-level characteristics, including *Portfolio Size (Millions)*, *Number of Stocks Held*, *Portfolio Turnover*, *Portfolio Net Flow*, *Excess Return*, *FF3 Alpha*, *Carhart Alpha*, *FF5 Alpha*. Panel B reports holding-based characteristics, including *Size (Millions)*, *BM*, *Momentum*, *Turnover*, *Idiosyncratic Volatility*, *Age*, *Institutional Ownership*. Holding-based characteristics are calculated using all stock reported in the 13-F filings and averaged across stocks. Column (1) includes all institutional investors in the Thomson Reuters Institutional (13f) Holdings Database. Column (2) and (3) includes all viewing and filing institutional investors, respectively. Column (4) compares the viewing and filing institutional investors.

Panel A: Institutional Characteristics

	(1) Thomson Reuters	(2) Viewing	(3) Filing	(4) Viewing -Filing
Portfolio Size (Millions)	2402	9617	4063	5554
Number of Stocks Held	178	415	193	222
Portfolio Turnover	0.12	0.17	0.15	0.02
Portfolio Net Flow	0.02	0.02	0.02	0.00
Excess Return	0.82%	0.81%	0.85%	-0.04%
FF3 Alpha	-0.06%	-0.16%	-0.12%	-0.03%
Carhart Alpha	0.10%	0.07%	0.11%	-0.04%
FF5 Alpha	0.04%	-0.03%	0.07%	-0.09%

Panel B: Stock Characteristics

	(1) Thomson Reuters	(2) Viewing	(3) Filing	(4) Viewing -Filing
Size (Millions)	46200	28292	33810	-5518
BM	0.51	0.50	0.52	-0.02
Momentum	0.10	0.12	0.11	0.01
Turnover	0.22	0.24	0.24	0.00
Idiosyncratic Volatility	0.02	0.02	0.02	-0.00
Age	124.42	119.09	119.84	-0.75
Institutional Ownership	0.74	0.77	0.75	0.02

Table 2: Determinants of Information Source

This table reports the results of how viewing institutional investors determine whose filings to access. I report OLS regression results at viewer-filer-quarter level. The dependent variable is *13-F Access*, which is a dummy variable that equals one if a viewing institutional investor accessed a 13-F filing from a filing institutional investor. The independent variables are lagged *13-F Access*, institution-level characteristics, including *Portfolio Size*, *Portfolio Turnover*, *Carhart Alpha*, and holding-based characteristics, including *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, *Idiosyncratic Volatility*, *Ln(Age)*, *Institutional Ownership*. Holding-based characteristics are calculated using all stock reported in the 13-F filings and averaged across stocks. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer fixed effects. The standard errors are double clustered by quarter and viewing institutional investor. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)
Lagged 13-F Access	24.626***			24.571***
	(4.675)			(4.667)
Portfolio Size		0.034***		0.015***
		(6.786)		(5.952)
Portfolio Turnover		0.213***		0.045***
		(2.905)		(6.419)
Net Flow		-0.010		0.010
		(-1.142)		(0.885)
Carhart Alpha		-0.007		0.212***
		(-0.187)		(4.626)
Ln(Size)			0.001	1.667***
			(0.239)	(4.934)
Ln(BM)			0.040***	-0.139***
			(4.588)	(-9.599)
Momentum			0.031**	-0.024
			(2.097)	(-0.837)
Turnover			0.265***	0.027***
			(2.876)	(8.163)
Idiosyncratic Volatility			0.838*	0.079**
			(1.814)	(2.404)
Ln(Age)			-0.168***	-0.015**
			(-7.684)	(-2.336)
Institutional Onwership			0.246***	0.022
			(6.448)	(1.066)
Fixed Effects	YES	YES	YES	YES
Observations	33,971,123	33,971,123	33,971,123	33,971,123
Adjusted R-Square	0.0897	0.0318	0.0315	0.0900

Table 3: Viewing Trade and Filing Trade

This table reports the results of how the trading behavior of filings institutional investors affect the trading behavior of viewing institutional investors. I report OLS regression results at stock-viewer-quarter level. The dependent variable is *Viewing Trade*, which measures the change in the number of shares held of a viewing institutional investor, scaled by the total number of shares outstanding. The key independent variables *Filing Trade*, which measures the change in number of shares held of the filing institutional investors, scaled by total number of shares outstanding. For each viewing institutional investor, *Filing Trade* is averaged across all viewed filing institutional investors, weighted equally or by total asset under management. I also include the following stock characteristic as control variables: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*, Δ*Institutional Ownership*$_{t-1}$ and Δ*Institutional Ownership*$_t$. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
VW Filing Trade	-0.006***		-0.007***		-0.007***	
	(-2.786)		(-2.956)		(-3.096)	
EW Filing Trade		-0.005**		-0.005**		-0.005**
		(-2.032)		(-2.101)		(-2.281)
Ln(Size)			-0.008	-0.008	-0.009	-0.009
			(-1.021)	(-1.021)	(-1.103)	(-1.104)
Ln(BM)			0.002	0.002	0.003	0.003
			(0.513)	(0.510)	(0.865)	(0.863)
Momentum			0.007*	0.007*	0.005	0.005
			(1.789)	(1.753)	(1.272)	(1.239)
Turnover			-0.001	-0.001	0.001	0.001
			(-0.328)	(-0.346)	(0.232)	(0.214)
Idiosyncratic Volatility			-0.009*	-0.009*	-0.010*	-0.010*
			(-1.821)	(-1.810)	(-1.914)	(-1.904)
Excess Return			0.032***	0.032***	0.030***	0.030***
			(5.716)	(5.705)	(5.393)	(5.384)
Ln(Age)			-0.008***	-0.008***	-0.007**	-0.007**
			(-3.078)	(-3.047)	(-2.549)	(-2.519)
ΔIO_{t-1}					0.002	0.002
					(0.580)	(0.563)
ΔIO_t					0.041***	0.041***
					(4.580)	(4.581)
Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	617,047	617,047	617,047	617,047	617,047	617,047
Adjusted R-Square	0.0300	0.0300	0.0307	0.0307	0.0321	0.0321

Table 4: Viewing Trade Direction and Filing Trade Direction

This table reports the results of how the trading direction of filings institutional investors affect the trading direction of viewing institutional investors. I report OLS regression results at stock-viewer-quarter level. The dependent variable is *Viewing Buy*, which is a dummy variable that equals one if a viewing institutional investor increased its position for a stock. The key independent variables *Filing Buy*, which is a dummy variable that equals one if viewed filing institutional investors increased their position. I also include the following stock characteristic as control variables: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*, Δ*Institutional Ownership*$_{t-1}$ and Δ*Institutional Ownership*$_{t}$. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
VW Filing Buy	-0.005**		-0.005**		-0.005**	
	(-2.265)		(-2.418)		(-2.440)	
EW Filing Buy		-0.006***		-0.006***		-0.006***
		(-3.005)		(-3.027)		(-3.064)
Ln(Size)			-0.004	-0.004	-0.005	-0.005
			(-1.003)	(-1.000)	(-1.020)	(-1.016)
Ln(BM)			0.002	0.002	0.002	0.002
			(1.347)	(1.349)	(1.547)	(1.549)
Momentum			0.006***	0.006***	0.005***	0.005***
			(3.451)	(3.442)	(3.221)	(3.211)
Turnover			0.005**	0.005**	0.005**	0.005**
			(2.435)	(2.429)	(2.597)	(2.591)
Idiosyncratic Volatility			-0.004**	-0.004**	-0.004**	-0.004**
			(-2.314)	(-2.305)	(-2.386)	(-2.377)
Excess Return			0.019***	0.019***	0.019***	0.019***
			(8.853)	(8.850)	(8.776)	(8.772)
Ln(Age)			-0.004**	-0.004**	-0.003**	-0.003**
			(-2.229)	(-2.221)	(-2.038)	(-2.028)
ΔIO_{t-1}					-0.000	-0.000
					(-0.108)	(-0.089)
ΔIO_{t}					0.008**	0.008**
					(2.484)	(2.486)
Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	564,412	564,412	564,412	564,412	564,412	564,412
Adjusted R-Square	0.0878	0.0878	0.0888	0.0888	0.0890	0.0890

Table 5: Change in Viewing Portfolio Weight and Change in Filing Portfolio Weight

This table reports the results of how changes in portfolio weight of filings institutional investors affect changes in portfolio weight of viewing institutional investors. I report OLS regression results at stock-viewer-quarter level. The dependent variable is Δ*Viewing Weight*, which measures changes in portfolio weight of a viewing institutional investor. The key independent variables Δ*Filing Weight*, which measures changes in portfolio weight of filing institutional investors. I also include the following stock characteristic as control variables: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*, Δ*Institutional Ownership*$_{t-1}$ and Δ*Institutional Ownership*$_{t}$. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
VW Δ Filing Weight	-0.035***		-0.026***		-0.026***	
	(-6.693)		(-5.016)		(-5.002)	
EW Δ Filing Weight		-0.029***		-0.019***		-0.019***
		(-7.034)		(-4.757)		(-4.759)
Ln(Size)			0.037**	0.036**	0.037**	0.036**
			(2.117)	(2.070)	(2.111)	(2.064)
Ln(BM)			0.013***	0.013***	0.012***	0.012***
			(3.297)	(3.299)	(3.100)	(3.101)
Momentum			0.003	0.003	0.004	0.004
			(0.909)	(0.886)	(1.359)	(1.339)
Turnover			-0.003	-0.003	-0.004	-0.004
			(-0.671)	(-0.724)	(-0.958)	(-1.009)
Idiosyncratic Volatility			0.003	0.003	0.004	0.003
			(1.055)	(0.960)	(1.150)	(1.053)
Excess Return			0.093***	0.093***	0.094***	0.095***
			(14.142)	(14.206)	(13.744)	(13.796)
Ln(Age)			0.007***	0.007***	0.006***	0.006***
			(3.203)	(3.203)	(2.822)	(2.826)
ΔIO_{t-1}					-0.004	-0.004
					(-1.588)	(-1.534)
ΔIO_{t}					-0.024***	-0.024***
					(-7.488)	(-7.509)
Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	617,047	617,047	617,047	617,047	617,047	617,047
Adjusted R-Square	0.136	0.136	0.143	0.143	0.144	0.144

Table 6: Subsample Analysis: Viewing Trade and Filing Trade

This table reports the subsample results of how the trading behavior of filings institutional investors affect the trading direction of viewing institutional investors. Panel A (Panel B) contains stocks that experienced filing institutional buy (sell). I report OLS regression results at stock-viewer-quarter level. The dependent variable is *Viewing Trade*, which measures the change in the number of shares held of a viewing institutional investor, scaled by the total number of shares outstanding. The key independent variables *Filing Trade*, which measures the change in number of shares held of the filing institutional investors, scaled by total number of shares outstanding. For each viewing institutional investor, *Filing Trade* is averaged across all viewed filing institutional investors, weighted equally or by total asset under management. I also include the following stock characteristic as control variables: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*, Δ*Institutional Ownership*$_{t-1}$ and Δ*Institutional Ownership*$_t$. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	Panel A: Filing Buy		Panel B: Filing Sell	
	(1)	(2)	(3)	(4)
VW Filing Trade	-0.004		-0.009**	
	(-1.225)		(-2.375)	
EW Filing Trade		-0.003		-0.008**
		(-1.004)		(-2.158)
Ln(Size)	-0.013	-0.011	-0.002	-0.006
	(-1.398)	(-1.225)	(-0.245)	(-0.706)
Ln(BM)	0.004	0.004	0.003	0.003
	(0.895)	(0.898)	(0.522)	(0.624)
Momentum	0.004	0.004	0.005	0.004
	(0.947)	(0.830)	(1.070)	(1.027)
Turnover	0.001	0.002	0.000	-0.001
	(0.183)	(0.578)	(0.039)	(-0.307)
Idiosyncratic Volatility	-0.011*	-0.012**	-0.006	-0.006
	(-1.746)	(-2.131)	(-0.876)	(-0.779)
Excess Return	0.034***	0.032***	0.026***	0.026***
	(5.314)	(4.857)	(4.003)	(4.244)
Ln(Age)	-0.007*	-0.003	-0.006	-0.006**
	(-1.940)	(-0.947)	(-1.617)	(-2.310)
ΔIO_{t-1}	-0.002	0.001	0.004	0.000
	(-0.423)	(0.136)	(1.074)	(0.020)
ΔIO_t	0.037***	0.039***	0.046***	0.045***
	(4.440)	(4.669)	(4.754)	(4.897)
Fixed Effects	YES	YES	YES	YES
Observations	296,367	287,674	246,554	254,884
Adjusted R-Square	0.0363	0.0375	0.0341	0.0329

Table 7: Alternative Explanation: Propensity Score Matching

This table reports the results of how the trading behavior of filings institutional investors affect the trading direction of viewing institutional investors using the propensity-score-matched sample. I report OLS regression results at quarter-viewer-stock level. The dependent variable is *Viewing Trade*, which measures the change in the number of shares held of a viewing institutional investor, scaled by the total number of shares outstanding. The key independent variables *Filing Trade*, which measures the change in number of shares held of the filing institutional investors, scaled by total number of shares outstanding. For each viewing institutional investor, *Filing Trade* is averaged across all viewed filing institutional investors, weighted equally or by total asset under management. I also include the following stock characteristic as control variables: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*, Δ*Institutional Ownership*$_{t-1}$ and Δ*Institutional Ownership*$_t$. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	Full Sample		Filing Buy		Filing Sell	
	(1)	(2)	(3)	(4)	(5)	(6)
VW Filing Trade	0.003		-0.001		0.006	
	(0.992)		(-0.255)		(1.029)	
EW Filing Trade		0.001		0.002		0.006
		(0.243)		(0.570)		(1.178)
Ln(Size)	-0.007	-0.007*	-0.008	-0.006	-0.011	-0.014*
	(-1.085)	(-1.914)	(-0.905)	(-0.699)	(-1.425)	(-1.992)
Ln(BM)	0.005	0.002	0.005	0.006	0.004	0.003
	(1.363)	(1.580)	(1.182)	(1.535)	(0.843)	(0.528)
Momentum	0.007*	0.006***	0.011*	0.011*	0.003	0.004
	(2.008)	(4.617)	(1.866)	(2.017)	(0.965)	(1.028)
Turnover	-0.001	0.004**	-0.000	-0.000	-0.001	-0.001
	(-0.295)	(2.425)	(-0.002)	(-0.059)	(-0.394)	(-0.237)
Idiosyncratic Volatility	-0.003	-0.004*	0.000	-0.001	-0.004	-0.004
	(-0.508)	(-1.861)	(0.058)	(-0.219)	(-0.632)	(-0.645)
Excess Return	0.023***	0.015***	0.023***	0.024***	0.024***	0.023***
	(4.580)	(7.738)	(3.162)	(3.231)	(5.101)	(4.918)
Ln(Age)	-0.005**	-0.003**	-0.003	-0.004	-0.005	-0.003
	(-2.125)	(-2.371)	(-0.927)	(-1.132)	(-1.510)	(-1.141)
ΔIO_{t-1}	0.011**	0.003**	0.015**	0.014**	0.011**	0.011**
	(2.443)	(2.633)	(2.673)	(2.498)	(2.230)	(2.194)
ΔIO_t	0.046***	0.010***	0.048***	0.048***	0.047***	0.046***
	(5.757)	(3.393)	(5.595)	(5.612)	(5.843)	(5.550)
Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	658,490	595,265	302,998	301,046	275,070	276,703
Adjusted R-Square	0.0353	0.0886	0.0383	0.0386	0.0336	0.0339

Table 8: Alternative Explanation: Common Information

This table reports the results of how the trading behavior of filings institutional investors affect the trading direction of viewing institutional investors. I report OLS regression results at quarter-viewer-stock level. The dependent variable is *Viewing Trade*, which measures the change in the number of shares held of a viewing institutional investor, scaled by the total number of shares outstanding. The key independent variables *Filing Trade*, which measures the change in number of shares held of the filing institutional investors, scaled by total number of shares outstanding. For each viewing institutional investor, *Filing Trade* is averaged across all viewed filing institutional investors, weighted equally or by total asset under management. I include the contemporaneous *Filing Trade* and the following stock characteristic: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*, Δ*Institutional Ownership*$_{t-1}$ and Δ*Institutional Ownership*$_t$. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	Full Sample		Filing Buy		Filing Sell	
	(1)	(2)	(3)	(4)	(5)	(6)
VW Filing Trade	-0.010***		-0.004		-0.015***	
	(-4.172)		(-1.223)		(-3.996)	
EW Filing Trade		-0.008***		-0.002		-0.013***
		(-3.197)		(-0.651)		(-3.991)
VW Filing Trade$_t$	0.038***		0.040***		0.038***	
	(7.584)		(7.087)		(7.023)	
EW Filing Trade$_t$		0.042***		0.044***		0.041***
		(7.838)		(8.006)		(6.521)
Controls	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	593,650	593,650	296,133	287,443	225,460	233,768
Adjusted R-Square	0.0330	0.0333	0.0377	0.0393	0.0348	0.0338

Table 9: Profitability

This table reports the results of whether the contrarian trading strategy is profitable. In the end of each quarter, for all stocks that experienced average filings sell, I measures $\hat{\beta}$ as the coefficient estimate of regressing viewing institutional trades on filings institutional trades. Based on its $\hat{\beta}$, all stocks that experienced average filing sell are put into $\hat{\beta}$-weighted contrarian ($\hat{\beta} < 0$) and confirmation portfolios ($\hat{\beta} > 0$). The contrarian and confirmation portfolios are also divided in to $\hat{\beta}$-sorted terciles. Portfolio returns are computed over the next month. Panel A reports the returns of contrarian stocks. Panel B reports the returns of confirmations stocks. I report the quarterly average return measures, including excess return over the risk-free rate, excess return over the market return, Fama-French 3-factor alpha, Carhar 4-factor alpha, and Fama-French 5-factor alpha. Portfolios are $\hat{\beta}$ weighted. $\hat{\beta}$ is the coefficient estimate of regressing viewing institutional trades on filings institutional trades at stock-quarter level.The definitions of the all variables can be found in Section 2. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

Panel A: Contrarian Portfolio

	$\hat{\beta}$	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
Overall	-0.418	2.98%*	1.04%**	0.62%**	0.91%***	0.66%**
		(1.98)	(2.13)	(2.18)	(3.11)	(2.17)
Small	-0.041	2.73%*	0.85%*	0.10%	0.22%	0.24%
		(1.85)	(1.80)	(0.29)	(0.69)	(0.67)
Median	-0.194	2.78%*	0.84%	0.05%	0.12%	0.21%
		(1.77)	(1.54)	(0.12)	(0.33)	(0.64)
Large	-1.108	3.05%**	1.11%**	0.75%**	1.10%***	0.77%**
		(2.02)	(2.09)	(2.29)	(3.22)	(2.11)

Panel B: Confirmation Portfolio

	$\hat{\beta}$	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
Overall	0.398	2.75%*	0.78%	0.08%	0.11%	0.17%
		(1.82)	(1.45)	(0.17)	(0.26)	(0.35)
Small	0.039	2.72%*	0.80%	0.11%	0.36%	0.30%
		(1.82)	(1.58)	(0.29)	(0.94)	(0.72)
Median	0.181	3.10%**	1.24%**	0.29%	0.47%	0.49%
		(2.10)	(2.62)	(1.00)	(1.57)	(1.65)
Large	0.971	2.65%*	0.66%	0.00%	-0.00%	0.06%
		(1.71)	(1.06)	(0.01)	(-0.01)	(0.11)

Table 10: Profitability Sensitivity Analysis: Stock Characteristics

This table reports the results of how the profitability of contrarian trading strategy varies with stock characteristics. In the end of each quarter, for all stocks that experienced average filings sell, I measures $\hat{\beta}$ as the coefficient estimate of regressing viewing institutional trades on filings institutional trades. Contrarian stocks ($\hat{\beta} < 0$) are sorted into $\hat{\beta}$-weighted terciles, based on its market capitalization, dollar volume and illiquidity (Panel A, B, and C). I report the quarterly average return measures, including excess return over the risk-free rate, excess return over the market return, Fama-French 3-factor alpha, Carhar 4-factor alpha, and Fama-French 5-factor alpha. Portfolios are $\hat{\beta}$ weighted. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

Panel A: Market Capitalization					
	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
Small Cap	3.46%**	1.57%**	1.01%**	1.48%***	1.20%**
	(2.15)	(2.20)	(2.14)	(2.85)	(2.38)
Median Cap	3.49%**	1.51%**	1.19%**	1.44%***	1.11%**
	(2.21)	(2.31)	(2.46)	(3.24)	(2.33)
Large Cap	1.85%	-0.13%	-0.66%	-0.55%	-0.73%
	(1.34)	(-0.36)	(-1.46)	(-1.18)	(-1.50)

Panel B: Volume					
	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
Low Volume	3.52%**	1.59%**	1.38%***	1.67%***	1.59%***
	(2.34)	(2.37)	(2.89)	(3.47)	(3.18)
Median Volume	3.44%**	1.55%**	0.91%**	1.24%***	0.77%*
	(2.11)	(2.49)	(2.36)	(3.06)	(1.97)
High Volume	1.70%	-0.30%	-0.89%*	-0.62%	-0.89%*
	(1.15)	(-0.68)	(-1.99)	(-1.31)	(-1.87)

Panel C: Illiquidity					
	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
Liquid	1.51%	-0.10%	-0.65%	-0.57%	-0.78%
	(0.84)	(-0.24)	(-1.38)	(-1.08)	(-1.56)
Median Liquid	3.11%	1.49%*	0.92%*	0.97%*	0.76%
	(1.50)	(2.00)	(1.72)	(1.71)	(1.39)
Illiquid	3.26%	1.75%**	1.22%*	1.49%**	1.53%**
	(1.63)	(2.11)	(2.04)	(2.50)	(2.43)

Table 11: Profitability Mechanism: Buying Price Pressure

This table reports the results of whether the profitability of the contrarian trading strategy is due to buying price pressure. In the end of each quarter, for all stocks that experienced average filings sell, I measures $\hat{\beta}$ as the coefficient estimate of regressing viewing institutional trades on filings institutional trades. Contrarian stocks ($\hat{\beta} < 0$) are sorted into $\hat{\beta}$-weighted terciles, based on its market capitalization, volume and illiquidity. Portfolio returns for the small, low volume and illiquid are measured three quarters after profitability is measures ($t + 1$). shown in Panel A, B, and C, respectively. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

Panel A: Small Cap

Quarter	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
t+1	**3.46%****	**1.57%****	**1.01%****	**1.48%*****	**1.20%****
	(2.15)	**(2.20)**	**(2.14)**	**(2.85)**	**(2.38)**
$t+2$	2.42%	0.72%	-0.20%	0.09%	-0.05%
	(1.29)	(0.73)	(-0.38)	(0.18)	(-0.09)
$t+3$	2.17%	0.41%	-0.20%	-0.15%	-0.01%
	(1.25)	(0.49)	(-0.39)	(-0.32)	(-0.03)
$t+4$	2.63%	0.92%	0.10%	0.29%	0.58%
	(1.60)	(1.10)	(0.17)	(0.55)	(0.99)

Panel B: Low Volume

Quarter	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
t+1	**3.52%****	**1.59%****	**1.38%*****	**1.67%*****	**1.59%*****
	(2.34)	**(2.37)**	**(2.89)**	**(3.47)**	**(3.18)**
$t+2$	2.60%	0.93%	0.00%	0.26%	0.22%
	(1.48)	(1.08)	(0.00)	(0.52)	(0.39)
$t+3$	2.28%	0.49%	-0.20%	-0.09%	-0.12%
	(1.43)	(0.66)	(-0.44)	(-0.20)	(-0.26)
$t+4$	2.51%	0.74%	0.10%	0.31%	0.41%
	(1.55)	(0.88)	(0.17)	(0.56)	(0.70)

Panel C: Illiquid

Quarter	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
t+1	**3.26%**	**1.75%****	**1.22%***	**1.49%****	**1.53%****
	(1.63)	**(2.11)**	**(2.04)**	**(2.50)**	**(2.43)**
$t+2$	2.32%	0.72%	-0.77%	-0.45%	-0.76%
	(1.03)	(0.63)	(-1.15)	(-0.78)	(-1.09)
$t+3$	2.84%	1.08%	0.29%	0.25%	0.36%
	(1.31)	(1.07)	(0.50)	(0.44)	(0.59)
$t+4$	3.34%	1.14%	0.32%	0.33%	0.67%
	(1.53)	(1.15)	(0.49)	(0.56)	(0.98)

Table 12: Profitability Mechanism: Return Reversal

This table reports the results of whether the profitability of the contrarian trading strategy is due to selling price pressure. In the end of each quarter, for all stocks that experienced average filings sell, I measures $\hat{\beta}$ as the coefficient estimate of regressing viewing institutional trades on filings institutional trades. Contrarian stocks ($\hat{\beta} < 0$) are sorted into $\hat{\beta}$-weighted terciles, based on its market capitalization, volume and illiquidity. Portfolio returns for the small, low volume and illiquid are measured two quarters after prior to the profitability is measured ($t + 1$), shown in Panel A, B, and C, respectively. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

Panel A: Small Stocks

Quarter	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
$t-1$	0.44%	-1.35%	-1.56%***	-1.18%**	-1.41%***
	(0.24)	(-1.45)	(-3.08)	(-2.32)	(-2.82)
t	0.14%	-1.67%*	-2.22%***	-1.79%***	-2.32%***
	(0.08)	(-1.69)	(-3.62)	(-3.07)	(-3.94)
$\boldsymbol{t+1}$	**3.46%****	**1.57%****	**1.01%****	**1.48%*****	**1.20%****
	(2.15)	**(2.20)**	**(2.14)**	**(2.85)**	**(2.38)**

Panel B: Low Volume Stocks

Quarter	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
$t-1$	0.91%	-0.96%	-1.05%***	-0.72%*	-0.85%**
	(0.54)	(-1.27)	(-2.73)	(-1.77)	(-2.35)
t	1.40%	-0.42%	-0.82%	-0.59%	-0.94%*
	(0.80)	(-0.46)	(-1.47)	(-1.07)	(-1.74)
$\boldsymbol{t+1}$	**3.52%****	**1.59%****	**1.38%*****	**1.67%*****	**1.59%*****
	(2.34)	**(2.37)**	**(2.89)**	**(3.47)**	**(3.18)**

Panel C: Illiquid Stocks

Quarter	Rf Excess Return	Mkt Excess Return	FF3 Alpha	Carhart Alpha	FF5 Alpha
$t-1$	1.17%	-0.38%	-0.49%	-0.23%	-0.51%
	(0.54)	(-0.37)	(-0.99)	(-0.54)	(-1.02)
t	0.78%	0.11%	-0.70%	-0.52%	-0.81%
	(0.36)	(0.10)	(-1.08)	(-0.87)	(-1.37)
$\boldsymbol{t+1}$	**3.26%**	**1.75%****	**1.22%***	**1.49%****	**1.53%****
	(1.63)	**(2.11)**	**(2.04)**	**(2.50)**	**(2.43)**

Table 13: Additional Information Acquisition

This table reports the results of whether viewing institutional investors acquire stock-related information after 13-F access. I report OLS regression results at stock-viewer-week level. The dependent variable is *Direct Access*, which is a dummy variables that equals one if an institutional investor accesses filings from a stock. I consider proxy statements, 8-K, insider-trading, and 10-K/Q filings. The key independent variables *13-F Access*, which is a dummy variable that equals one if a stock appeared in a 13-F filing accessed by a institutional investor. Panel A (B) reports results where *Direct Access* is measured in the same (next) week as *13-F Access*. I control for the lagged *Direct Access* and the following lagged stock characteristic: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*. The definitions of the all variables are in Section 2. I include week-institution-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	Panel A: Current Week				
	(1)	(2)	(3)	(4)	(5)
	Any	Proxy	8-K	Insider	10-K/Q
13-F Access	0.307***	0.017	0.079**	0.012	0.209***
	(6.075)	(1.488)	(2.373)	(1.551)	(4.369)
Direct Access$_{t-1}$	12.970***	8.162***	9.350***	10.794***	12.766***
	(22.686)	(7.656)	(10.353)	(6.322)	(20.833)
Direct Access$_{t-2}$	8.735***	5.846***	6.293***	7.875***	8.393***
	(15.920)	(5.741)	(7.864)	(5.610)	(15.043)
Direct Access$_{t-3}$	7.557***	5.231***	5.443***	6.754***	7.236***
	(14.008)	(5.491)	(6.900)	(4.799)	(13.478)
Direct Access$_{t-4}$	7.406***	4.950***	5.296***	6.652***	7.135***
	(13.356)	(5.186)	(6.442)	(4.545)	(12.974)
Ln(Size)	0.298***	0.035***	0.093***	0.012***	0.248***
	(12.330)	(9.822)	(10.729)	(7.834)	(12.417)
Ln(BM)	0.120***	0.017***	0.052***	0.004***	0.093***
	(6.070)	(5.919)	(6.543)	(2.992)	(5.780)
Momentum	-0.027*	-0.003*	-0.011*	0.001	-0.023*
	(-1.850)	(-1.759)	(-1.768)	(0.536)	(-1.866)
Turnover	0.512***	0.051***	0.276***	0.020***	0.355***
	(5.103)	(3.308)	(5.558)	(3.366)	(4.803)
Idiosyncratic Volatility	5.981***	1.058***	3.757***	0.368***	3.476***
	(5.659)	(5.655)	(5.772)	(4.930)	(4.706)
Excess Return	-0.129***	-0.011***	-0.057***	-0.004**	-0.100***
	(-5.468)	(-3.304)	(-4.788)	(-2.318)	(-4.929)
Ln(Age)	0.001***	0.000**	0.000	0.000	0.001***
	(2.984)	(2.035)	(1.196)	(0.684)	(3.010)
IO	-0.061	-0.038***	-0.026	-0.009*	-0.039
	(-1.216)	(-3.817)	(-1.483)	(-1.957)	(-0.857)
Fixed Effects	YES	YES	YES	YES	YES
Observations	51,550,668	51,550,668	51,550,668	51,550,668	51,550,668
Adjusted R-Square	0.162	0.0449	0.0594	0.0334	0.147

	Panel B: Next Week				
	(1) Any	(2) Proxy	(3) 8-K	(4) Insider	(5) 10-K/Q
13-F Access	0.333***	0.023*	0.098***	0.023**	0.282***
	(7.971)	(1.736)	(4.938)	(2.158)	(6.954)
Direct Access$_{t-1}$	9.876***	6.282***	6.904***	8.606***	9.524***
	(16.197)	(5.664)	(7.880)	(5.347)	(15.084)
Direct Access$_{t-2}$	8.079***	5.457***	5.724***	7.160***	7.748***
	(14.110)	(5.445)	(6.879)	(4.714)	(13.451)
Direct Access$_{t-3}$	7.507***	5.051***	5.358***	6.730***	7.228***
	(13.406)	(5.205)	(6.446)	(4.486)	(13.014)
Direct Access$_{t-4}$	7.461***	4.734***	5.173***	6.446***	7.153***
	(13.037)	(5.121)	(6.165)	(4.784)	(12.597)
Ln(Size)	0.316***	0.036***	0.097***	0.012***	0.263***
	(12.322)	(9.731)	(10.596)	(7.743)	(12.423)
Ln(BM)	0.126***	0.017***	0.054***	0.005***	0.098***
	(6.059)	(5.841)	(6.470)	(2.893)	(5.758)
Momentum	-0.029*	-0.004**	-0.011*	0.001	-0.023*
	(-1.847)	(-2.030)	(-1.806)	(0.549)	(-1.782)
Turnover	0.536***	0.051***	0.286***	0.021***	0.374***
	(5.079)	(3.245)	(5.515)	(3.316)	(4.800)
Idiosyncratic Volatility	6.193***	1.117***	3.840***	0.390***	3.578***
	(5.524)	(5.705)	(5.767)	(5.284)	(4.498)
Excess Return	-0.133***	-0.012***	-0.062***	-0.005**	-0.103***
	(-5.590)	(-3.375)	(-5.090)	(-2.392)	(-5.036)
Ln(Age)	0.001***	0.000**	0.000	0.000	0.001***
	(2.975)	(2.010)	(1.182)	(0.645)	(3.015)
IO	-0.068	-0.038***	-0.028	-0.009*	-0.045
	(-1.268)	(-3.643)	(-1.571)	(-1.909)	(-0.933)
Fixed Effects	YES	YES	YES	YES	YES
Observations	51,191,204	51,191,204	51,191,204	51,191,204	51,191,204
Adjusted R-Square	0.147	0.0405	0.0536	0.0264	0.138

Table 14: Direct Access and Institutional Trades

This table reports the results of how acquisition of stock-related information affect the trading direction of viewing institutional investors. The dependent variable is *Viewing Trade*, which measures the change in the number of shares held of a viewing institutional investor, scaled by the total number of shares outstanding. The key independent variables *Filing Trade*, which measures the average change in number of shares held of the filing institutional investors, scaled by total number of shares outstanding. The change in number of shares by averaging across all its viewed filing institutional investors, weighted equally or total asset under management. *Direct Access*, which is a dummy variables that equals one if an institutional investor accesses filings from a stock after viewing the stock in 13-F filings. I also include the following stock characteristic as control variables: *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*, and *Idiosyncratic Volatility*, *Excess Return*, *Ln(Age)*, Δ*Institutional Ownership*$_{t-1}$ and Δ*Institutional Ownership*$_{t}$. All dependent variables are lagged, unless indicated otherwise. The definitions of the all variables can be found in Section 2. I include quarter-viewer-industry fixed effects. The standard errors are double clustered by quarter and industry. t-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, **$p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)
	Any	Proxy	8-K	Insider	10-K/Q
VW Filing Trade×Direct Access	-0.016**	-0.003	-0.020*	-0.037	-0.026**
	(-2.098)	(-0.170)	(-1.942)	(-0.778)	(-2.507)
VW Filing Trade	-0.008*	-0.009**	-0.008*	-0.009**	-0.007*
	(-1.807)	(-2.183)	(-1.990)	(-2.177)	(-1.781)
Direct Access	0.012	-0.002	0.003	-0.052	0.024*
	(1.022)	(-0.061)	(0.135)	(-1.627)	(1.809)
Controls	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES
Observations	246,554	246,554	246,554	246,554	246,554
Adjusted/Pseudo R-Square	0.0348	0.0348	0.0348	0.0348	0.0348

Table A1: Propensity Score Matching Diagnostics

This table presents the statistics from a propensity score matching procedure. A treated observation is a viewing-filing institutional investor pair, where the viewing institutional investor accessed a 13-F filing from the filing institutional investor. For each viewer-quarter , I construct propensity score using the following characteristics of : *Ln(Size)*, *Ln(BM)*, *Momentum*, *Turnover*,*Idiosyncratic Volatility*, *Ln(Age)*, *Institutional Ownership*, and *Institutional Trade*, *Portfolio Size*, *Portfolio Turnover*, *Portfolio Net Flow*, *Excess Return*. Panel A reports the pairwise comparisons between the treatment and control observations for both pre-match and post-match results. Panel B reports the Probit regression with *13-F Access* as the dependent variable for the pre-match and post-match samples. Panel C reports the estimated propensity score distributions for the treatment group and the nearest-neighboring control groups. The definitions of the all variables can be found in Section 2. The *Percentage Significant* are reported in parentheses, indicating the percentage of viewer-quarter in which the variables is significant at 5 percent level.

Panel A: Pairwise comparisons

	Pre-match				Post-match			
	Treatment	Control	Treatment -Control	Percent Significant	Treatment	Control	Treatment -Control	Percent Significant
Ln(Size)	8.658	8.610	-0.048	2.35%	8.663	9.108	0.445	46.79%
Ln(BM)	-0.871	-0.884	-0.012	1.83%	-0.870	-0.918	-0.048	31.36%
Momentum	0.129	0.130	0.001	0.26%	0.129	0.117	-0.011	20.05%
Turnover	0.240	0.243	0.002	2.61%	0.241	0.211	-0.031	50.39%
Idiosyncratic Volatility	0.018	0.019	0.000	1.31%	0.019	0.016	-0.002	44.73%
Ln(Age)	4.524	4.526	0.002	1.83%	4.523	4.668	0.145	52.96%
Institutional Ownership	0.767	0.772	0.004	2.35%	0.767	0.744	-0.022	44.22%
Portfolio Size	7640.800	7300.700	-340.100	4.44%	7595.400	2316.900	-5278.500	38.56%
Portfolio Net Flow	0.048	0.053	0.005	1.04%	0.043	0.020	-0.023	11.83%
Portfolio Turnover	0.160	0.171	0.011	4.70%	0.159	0.116	-0.043	41.65%
Portfolio Carhart Alpha	0.004	0.004	0.000	0.26%	0.004	0.001	-0.003	8.23%

Panel B: Probit Regression Results				
	Pre-Match		Post-Match	
	Coefficient Estimate	Percent Significant	Coefficient Estimate	Percent Significant
Ln(Size)	0.048	24.42%	2.432	3.916%
Ln(BM)	0.117	26.74%	1.918	3.394%
Momentum	-0.061	18.51%	0.960	1.305%
Turnover	0.141	25.96%	0.406	2.872%
Idiosyncratic Volatility	-0.348	14.65%	0.479	1.567%
Ln(Age)	-0.339	42.42%	-0.635	3.133%
Institutional Ownership	0.361	35.48%	0.479	1.567%
Portfolio Size	0.202	55.01%	4.634	2.872%
Portfolio Net Flow	-0.008	10.03%	0.479	1.567%
Portfolio Turnover	0.102	26.22%	0.406	2.872%
Portfolio Carhart Alpha	0.082	7.46%	0.312	0.783%

Panel C: Estimated Propensity Score Distributions						
	No. of Obs.	Mean	SD	P5	Median	P95
Difference		0.000	-0.002	0.000	0.000	0.000
Control	37944	0.032	0.045	0.001	0.013	0.125
Treatment	37944	0.033	0.047	0.001	0.013	0.125

Table 15: Summary Statistics

This table presents the summary statistics of variables in our analyses. Panel A reports summary statistics for main variables. For *Institution Shares* and *Institution Numbers*, we consider startups backed by at least one institution. The definitions of the variables are reported in the Appendix Table **??**. Additional summary statistics can be found in Appendix Table A2. Panel B compares the fraction of startups with and without institutions' participation that have exited through IPO or M&A channels. Panel C compares the fraction of startups in early/late stages at the time of first investment between institution and non-institution investors. Early stages include "Early Stage" and "Startup/Seed". Late stages include "Later Stage" and "Buyout/Acquisition". T-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Summary Statistics

	Obs	Mean	Std Dev	Quartile 1	Median	Quartile 3
Initial Return	1898	0.25	0.44	0.00	0.10	0.29
Institution Shares	202	0.23	0.27	0.04	0.11	0.30
Institution Numbers	202	0.23	0.17	0.08	0.17	0.33
Lead VC Reputation	1898	0.27	0.51	0.00	0.07	0.29
Tech Dummy	1898	0.48	0.50	0.00	0.00	1.00
Top-tier Dummy	1898	0.37	0.48	0.00	0.00	1.00
Prior Market Return	1898	0.00	0.01	-0.01	0.00	0.01
Secondary Fraction	1898	0.29	0.37	0.00	0.00	0.68
Age	1898	12.56	17.83	4.00	7.00	12.00
Proceeds (millions)	1898	77.63	157.02	23.80	45.00	82.50

Panel B: Startup Exit Rate

Exit Channel	With Institutions	Without Institutions	Difference
IPO or M&A	67.28%	52.87%	14.42%***
			(9.77)
IPO	26.78%	12.65%	14.13%***
			(10.31)
M&A	40.50%	40.22%	0.29%
			(0.19)

Panel C: Startup Development Stage at Investment

Stage	Institution Investment	Non-institution Investment	Difference
Early Stages	17.43%	35.63%	-18.20%***
			(-6.94)
Late Stages	34.70%	26.30%	8.10%***
			(2.49)

Table 16: Institutions' Participation and IPO Underpricing

This table reports the results of our investigation into how institutions' participation affects IPO underpricing. We report OLS regression results. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We include industry fixed effects in Columns (1) and (2), IPO year fixed effects in Columns (3) and (4), and both fixed effects in Columns (5) and (6). We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
Institution Shares	-0.023**		-0.015**		-0.016**	
	(0.009)		(0.007)		(0.007)	
Institution Numbers		-0.022**		-0.014**		-0.015**
		(0.008)		(0.006)		(0.007)
Lead VC Reputation	-0.016	-0.016	-0.017	-0.018*	-0.015	-0.016
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)
Tech Dummy	0.093**	0.094**	0.131***	0.131***	0.075**	0.075**
	(0.036)	(0.036)	(0.043)	(0.043)	(0.035)	(0.035)
Top-tier Dummy	0.075*	0.076*	0.031	0.032	0.034	0.034
	(0.039)	(0.039)	(0.025)	(0.025)	(0.026)	(0.026)
Prior Market Return	0.022	0.022	0.023*	0.023*	0.024*	0.024*
	(0.015)	(0.015)	(0.013)	(0.012)	(0.013)	(0.012)
Secondary Fraction	-0.050*	-0.050*	-0.011	-0.011	-0.019	-0.019
	(0.028)	(0.027)	(0.020)	(0.020)	(0.020)	(0.020)
Ln (Age)	-0.080***	-0.080***	-0.041***	-0.041***	-0.048***	-0.048***
	(0.023)	(0.023)	(0.011)	(0.011)	(0.012)	(0.012)
Ln (Proceeds)	0.105***	0.105***	0.082**	0.082**	0.088***	0.088***
	(0.030)	(0.030)	(0.031)	(0.031)	(0.032)	(0.032)
Observations	1,898	1,898	1,898	1,898	1,898	1,898
Fixed Effects	YES	YES	YES	YES	YES	YES
Adjusted R-Square	0.170	0.170	0.274	0.274	0.285	0.285

Table 17: Propensity Score Matching Diagnostics

This table presents the statistics from a propensity score matching analysis. A treated firm is an issuing startup that has at least one institutional investor. We construct propensity score using the following characteristics: *Ln (Number of Rounds)*, *Ln (Number of VCs)*, *Ln (Total Amount Raised)*, *Early-stage Dummy*, *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. Panel A reports the pairwise comparisons of average characteristics between the treatment and control sample for both pre-match and post-match results. Panel B reports the Logit regression with Institution Backing Dummy as the dependent variable for the pre-match and post-match samples. Panel C reports the estimated propensity score distributions for the treatment group and 4 nearest-neighboring control groups. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Pairwise comparisons

	Pre-match				Post-match			
Variables	Control	Treatment	t-stat	P-value	Control	Treatment	t-stat	P-value
Ln (Number of Rounds)	1.521	1.708	-4.428	0.000	1.719	1.708	0.258	0.797
Ln (Number of VCs)	1.788	2.133	-6.678	0.000	2.144	2.133	0.208	0.836
Ln (Total Amount Raised)	9.961	10.417	-4.163	0.000	10.421	10.417	0.034	0.973
Early-stage Dummy	0.319	0.307	0.343	0.732	0.287	0.307	-0.558	0.578
Lead VC Reputation	0.011	-0.043	0.843	0.400	-0.042	-0.043	0.018	0.986
Tech Dummy	0.490	0.475	0.399	0.690	0.451	0.475	-0.616	0.538
Top-tier Dummy	0.368	0.396	-0.772	0.441	0.378	0.396	-0.472	0.637
Prior Market Return	0.006	-0.037	0.626	0.532	-0.041	-0.037	-0.048	0.962
Secondary Fraction	-0.003	0.021	-0.314	0.753	0.003	0.021	-0.233	0.816
Ln (Age)	-0.022	0.211	-2.990	0.003	0.195	0.211	-0.197	0.844
Ln (Proceeds)	-0.023	0.178	-2.394	0.017	0.138	0.178	-0.469	0.639

Panel B: Logit Regression Results		
Variables	Pre-Match	Post-Match
Ln (Number of Rounds)	0.060	-0.069
	(0.274)	(-0.306)
Ln (Number of VCs)	1.065***	0.055
	(5.496)	(0.284)
Ln (Total Amount Raised)	0.018	-0.021
	(0.220)	(-0.258)
Early-stage Dummy	-0.104	0.137
	(-0.566)	(0.723)
Lead VC Reputation	0.011	0.000
	(0.123)	(0.003)
Tech Dummy	0.151	0.220
	(0.542)	(0.793)
Top-tier Dummy	-0.055	-0.006
	(-0.278)	(-0.028)
Prior Market Return	-0.048	-0.006
	(-0.577)	(-0.073)
Secondary Fraction	-0.094	0.007
	(-1.036)	(0.076)
Ln (Age)	0.312***	-0.005
	(3.282)	(-0.055)
Ln (Proceeds)	0.539***	0.111
	(4.087)	(0.827)
Observations	1,871	1,212
Exit Year Fixed Effects	YES	YES
Industry Fixed Effects	YES	YES
Pseudo R-Square	0.116	0.009

Panel C: Estimated Propensity Score Distributions						
	No. of Obs.	Mean	SD	P5	Median	P95
Match Number 1						
Difference	202	0.001	0.007	0.000	0.000	0.005
Control	202	0.193	0.130	0.031	0.167	0.440
Treatment	202	0.192	0.128	0.031	0.167	0.446
Match Number 2						
Difference	202	0.003	0.013	0.000	0.000	0.011
Control	202	0.193	0.130	0.031	0.167	0.440
Treatment	202	0.191	0.124	0.030	0.167	0.451
Match Number 3						
Difference	202	0.004	0.013	0.000	0.000	0.013
Control	202	0.193	0.130	0.031	0.167	0.440
Treatment	202	0.190	0.124	0.030	0.167	0.427
Match Number 4						
Difference	202	0.004	0.013	0.000	0.000	0.017
Control	202	0.193	0.130	0.031	0.167	0.440
Treatment	202	0.191	0.124	0.030	0.167	0.446
Match Number 5						
Difference	202	0.005	0.015	0.000	0.001	0.022
Control	202	0.193	0.130	0.031	0.167	0.440
Treatment	202	0.190	0.122	0.031	0.166	0.422

Table 18: Propensity Score Matching Results

This table reports the results of our investigation into how institutions' participation affects IPO underpricing using the propensity-score-matched-sample. We report OLS regression results. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We include industry fixed effects in Columns (1) and (2), IPO year fixed effects in Columns (3) and (4), and both fixed effects in Columns (5) and (6). We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
Institution Shares	-0.021**		-0.018**		-0.018**	
	(0.009)		(0.009)		(0.009)	
Institution Numbers		-0.019**		-0.020**		-0.020**
		(0.008)		(0.009)		(0.009)
Lead VC Reputation	-0.018	-0.019	-0.009	-0.010	-0.010	-0.010
	(0.015)	(0.015)	(0.013)	(0.013)	(0.011)	(0.011)
Tech Dummy	0.095	0.095	0.130***	0.130***	0.088	0.088
	(0.058)	(0.058)	(0.047)	(0.047)	(0.061)	(0.061)
Top-tier Dummy	0.035	0.037	0.009	0.010	0.008	0.009
	(0.048)	(0.048)	(0.044)	(0.044)	(0.041)	(0.042)
Prior Market Return	0.013	0.013	0.011	0.011	0.011	0.011
	(0.019)	(0.019)	(0.016)	(0.016)	(0.016)	(0.015)
Secondary Fraction	-0.047	-0.046	-0.005	-0.004	-0.010	-0.009
	(0.028)	(0.028)	(0.012)	(0.012)	(0.013)	(0.013)
Ln (Age)	-0.054**	-0.054**	-0.023**	-0.023**	-0.026***	-0.026***
	(0.020)	(0.020)	(0.009)	(0.009)	(0.009)	(0.009)
Ln (Proceeds)	0.108***	0.107***	0.085**	0.084**	0.082**	0.082**
	(0.030)	(0.030)	(0.032)	(0.032)	(0.031)	(0.031)
Observations	1,212	1,212	1,212	1,212	1,212	1,212
IPO Year Fixed Effects	NO	NO	YES	YES	YES	YES
Industry Fixed Effects	YES	YES	NO	NO	YES	YES
Adjusted R-Square	0.160	0.159	0.365	0.366	0.366	0.366

Table 19: Evidence from Mutual Fund Scandal

This table reports the results of the two-stage regressions. Panel A reports the results from first-stage OLS regressions. The key independent variable is *Scandal*, which is a dummy variable that equals one if the potential investment for an institution-startup pair is affected by the scandal. The key dependent variable is Institution Dummy, which is a dummy variable that equals one if an institution-startup pair investment took place. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by institution. Panel B reports the results from second-stage regressions. The key independent variable is $\widehat{Institution\ Numbers}$, which is the predicted *Institution Numbers* calculated from the corresponding first stage regressions using Eq. (3). The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The standard errors are clustered and bootstrapped with 1,000 repetitions. Columns (1) and (2) consider all institutions. Columns (3) and (4) consider only mutual funds. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: First Stage: Predicting Institutions' Investment

	(1)	(2)	(3)	(4)
Scandal	-0.005***	-0.006***	-0.005***	-0.005***
	(0.001)	(0.002)	(0.001)	(0.002)
Lead VC Reputation		-0.000		-0.001***
		(0.000)		(0.000)
Tech Dummy		0.000		0.002
		(0.001)		(0.001)
Top-tier Dummy		-0.000		-0.002**
		(0.001)		(0.001)
Prior Market Return		-0.000		-0.000
		(0.000)		(0.000)
Secondary Fraction		0.000		0.001*
		(0.000)		(0.000)
Ln (Age)		0.001		0.000
		(0.001)		(0.001)
Ln (Proceeds)		0.000		0.000
		(0.001)		(0.001)
Observations	42,579	42,485	20,525	20,476
Fixed Effects	YES	YES	YES	YES
Adjusted R-Square	0.005	0.005	0.006	0.007

Panel B: Second Stage: Predicting IPO Underpricing				
	(1)	(2)	(3)	(4)
$\widehat{\text{Institution Numbers}}$	-0.018***	-0.018***	-0.014***	-0.012**
	(0.006)	(0.006)	(0.005)	(0.005)
Lead VC Reputation	-0.011	-0.011	-0.011	-0.012
	(0.012)	(0.012)	(0.012)	(0.012)
Tech Dummy	0.058	0.058	0.058	0.058
	(0.036)	(0.036)	(0.037)	(0.037)
Top-tier Dummy	0.046*	0.046*	0.044	0.043*
	(0.025)	(0.025)	(0.027)	(0.026)
Prior Market Return	0.024*	0.024*	0.023	0.023
	(0.014)	(0.013)	(0.014)	(0.014)
Secondary Fraction	-0.022**	-0.022***	-0.023***	-0.023**
	(0.009)	(0.008)	(0.009)	(0.009)
Ln (Age)	-0.050***	-0.049***	-0.052***	-0.052***
	(0.010)	(0.010)	(0.010)	(0.010)
Ln (Proceeds)	0.085***	0.085***	0.088***	0.088***
	(0.014)	(0.015)	(0.015)	(0.015)
Observations	1,895	1,895	1,862	1,862
Fixed Effects	YES	YES	YES	YES
Adjusted R-Square	0.287	0.287	0.287	0.287

Table 20: Institution Classification and All-Star Coverage Substitution

This table presents evidence of the substitution effect between dedicated institutions and all-star analyst coverage. We report OLS regression results. Panel A shows how the effects of institutions' participation on IPO underpricing vary with institution classification. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We calculate both *Institution Shares* and *Institution Numbers* separately by institution classification. Panel B presents whether institutions' participation alleviate all-star analyst lust effect of IPO underpricing. *All-star Dummy* is a dummy variable that equals one if the IPO is covered by an Institutional Investor all-star analyst (top 3) associated with the bookrunner within one year of the IPO. The dependent variable and key independent variables are the same as in Panel A. We control for *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Institution Classification and IPO Underpricing		
	(1)	(2)
Dedicated Institution Shares	-0.048**	
	(0.021)	
Non-dedicated Institution Shares	-0.027	
	(0.018)	
Dedicated Institution Numbers		-0.028**
		(0.012)
Non-dedicated Institution Numbers		-0.017
		(0.010)
Observations	1,864	1,864
Control Variables	YES	YES
Fixed Effects	YES	YES
Adjusted R-Square	0.288	0.288

Panel B: Institutions' Participation and All-star Analysts				
	(1)	(2)	(3)	(4)
All-star Dummy	0.158***	0.101***	0.103***	0.100***
	(0.037)	(0.027)	(0.026)	(0.024)
Dedicated Institution Shares			-0.113**	
× All-star Dummy			(0.047)	
Non-dedicated Institution Shares			0.092	
× All-star Dummy			(0.077)	
Dedicated Institution Numbers				-0.062**
× All-star Dummy				(0.026)
Non-dedicated Institution Numbers				0.068
× All-star Dummy				(0.057)
Dedicated Institution Shares			-0.032	
			(0.042)	
Non-dedicated Institution Shares			-0.075**	
			(0.030)	
Dedicated Institution Numbers				-0.025
				(0.018)
Non-dedicated Institution Numbers				-0.033*
				(0.017)
Observations	1,155	1,155	1,155	1,155
Control Variables	No	YES	YES	YES
Fixed Effects	YES	YES	YES	YES
Adjusted R-Square	0.259	0.299	0.300	0.299

Table 21: Institutions' Participation and IPO Underpricing in Non-VC-Backed Sample

This table reports the results of our investigation into how institutions' participation affects IPO underpricing in the absence of traditional VC investors. We report OLS regression results. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *MF Participation*, which is a dummy variable that equals one if an mutual fund invested in a non-VC backed pre-IPO startups. We also include the following control variables: *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in the Appendix Table A1. We include industry fixed effects in Column (1), IPO year fixed effects in Column (2), and both fixed effects in Column (3). We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)
MF Participation	0.055	0.062	0.063
	(0.065)	(0.059)	(0.058)
Tech Dummy	0.060**	0.082***	0.044**
	(0.023)	(0.021)	(0.021)
Top-tier Dummy	0.059*	0.047**	0.048**
	(0.030)	(0.023)	(0.023)
Prior Market Return	0.012***	0.012***	0.013***
	(0.004)	(0.004)	(0.004)
Secondary Fraction	-0.009	0.005	0.003
	(0.008)	(0.005)	(0.005)
Ln (Age)	-0.038***	-0.025***	-0.027***
	(0.012)	(0.007)	(0.007)
Ln (Proceeds)	0.024***	-0.003	0.001
	(0.008)	(0.005)	(0.005)
Observations	4,973	4,973	4,973
Fixed Effects	YES	YES	YES
Adjusted R-Square	0.109	0.193	0.202

Table 22: Institutional Performance and IPO Underpricing

This table reports the results of our investigation into how institutions' participation affects IPO underpricing conditioning on the performance of institutional investors. We report OLS regression results. Institutional investor performance is measured using stock holdings in the industry of the startup (based on Fama-French 12 Industries). We calculate the 24-month average excess return over the market return (*Excess Return*), DGTW-adjusted return (*DGTW Return*), and industry-adjusted return (*Industry-adj. Return*). For a startup, we average three return measures across all participating institutions, weighted by their investment amount. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
Institution Shares	-0.015					
× Excess Return	(0.029)					
Institution Numbers		-0.034				
× Excess Return		(0.026)				
Institution Shares			-0.053			
× DGTW Return			(0.044)			
Institution Numbers				-0.063		
× DGTW Return				(0.039)		
Institution Shares					-0.026	
× Industry-adj. Return					(0.027)	
Institution Numbers						-0.030
× Industry-adj. Return						(0.021)
Excess Return	0.090	0.151				
	(0.098)	(0.089)				
DGTW Return			0.247	0.303*		
			(0.182)	(0.177)		
Industry-adj. Return					0.094	0.117
					(0.084)	(0.073)
Institution Shares	-0.021**		-0.020**		-0.020**	
	(0.009)		(0.009)		(0.009)	
Institution Numbers		-0.020***		-0.021***		-0.018***
		(0.006)		(0.007)		(0.005)
Observations	1,820	1,820	1,820	1,820	1,820	1,820
Controls	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES
Adjusted/Pseudo R-Square	0.289	0.289	0.289	0.289	0.289	0.289

Table 23: Institutions' Participation and Change in Offer Price

This table presents the results of our investigation into how institutions' participation affects the change in the offer price. We report OLS regression results. Panel A presents how institutions' participation affects the change in the offer price. Panel B presents how institutions' participation affects the absolute change in the offer price. The change in the offer price is measured as the change from the filing date midpoint price to the offer price, scaled by midpoint price. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Panel A: Change in Offer Price		Panel B: |Change in Offer Price|	
	(1)	(2)	(3)	(4)
Institution Shares	-0.004		0.001	
	(0.005)		(0.003)	
Institution Numbers		-0.007		0.001
		(0.005)		(0.003)
Lead VC Reputation	-0.007**	-0.008**	-0.000	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)
Tech Dummy	0.033**	0.033**	0.002	0.002
	(0.014)	(0.014)	(0.011)	(0.011)
Top-tier Dummy	-0.036***	-0.036***	-0.007	-0.007
	(0.011)	(0.011)	(0.009)	(0.009)
Prior Market Return	0.013**	0.013**	0.010	0.010
	(0.005)	(0.005)	(0.006)	(0.006)
Secondary Fraction	0.018***	0.017***	0.005	0.005
	(0.005)	(0.005)	(0.004)	(0.004)
Ln (Age)	-0.022***	-0.021***	-0.005	-0.005
	(0.006)	(0.006)	(0.005)	(0.005)
Ln (Proceeds)	0.084***	0.084***	-0.008	-0.008
	(0.012)	(0.012)	(0.009)	(0.009)
Observations	1,846	1,846	1,846	1,846
Controls	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES
Adjusted R-Square	0.166	0.167	0.024	0.024

Table 24: Institutions' LP Participation and IPO Underpricing

This table presents the results of our investigation into how institutions' participation as a limited partner affects IPO underpricing. We report OLS regression results. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *LP Institution Shares*, which measures the total dollar amount invested by all investors with at least one institutional LP, scaled by the total dollar amount invested by all investors, and *LP Institution Numbers*, which measures the total number of investors with at least one institutional LP, scaled by the total number of investors. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)
LP Institution Shares	0.005	
	(0.012)	
LP Institution Numbers		0.002
		(0.006)
Lead VC Reputation	-0.016	-0.016
	(0.011)	(0.010)
Tech Dummy	0.074**	0.074**
	(0.035)	(0.035)
Top-tier Dummy	0.034	0.034
	(0.025)	(0.026)
Prior Market Return	0.024*	0.024*
	(0.013)	(0.012)
Secondary Fraction	-0.019	-0.019
	(0.020)	(0.020)
Ln (Age)	-0.049***	-0.049***
	(0.012)	(0.012)
Ln (Proceeds)	0.085**	0.086**
	(0.031)	(0.032)
Observations	1,898	1,898
Fixed Effects	YES	YES
Adjusted R-Square	0.284	0.284

Table A2: Additional Summary Statistics

This table presents additional summary statistic of variables in our analyses. We multiply *Forecast Error* and *Industry Volatility* by 100 to facilitate interpretation. *Total Amount Raised* is measured in thousands of dollars. The definitions of the variables are reported in Appendix Table A1.

	Obs	Mean	Std Dev	Quartile 1	Median	Quartile 3
Forecast Error ($\times$ 100)	1,752	0.04	0.47	-0.02	0.03	0.05
Industry Volatility ($\times$ 100)	1,889	0.33	0.28	0.15	0.25	0.39
All-Star Dummy	1,155	0.21	0.41	0.00	0.00	0.00
Successful Exit Dummy	19,495	0.54	0.50	0.00	1.00	1.00
IPO Dummy	19,495	0.13	0.34	0.00	0.00	0.00
M&A Dummy	19,495	0.40	0.49	0.00	0.00	1.00
Startup Age at First Round	19,495	5.74	13.52	0.00	1.00	5.00
Number of Rounds	19,495	4.17	3.18	2.00	3.00	6.00
Number of VCs	19,495	5.36	4.40	2.00	4.00	7.00
Total Amount Raised (Thousands)	19,495	40,696	77,242	4,901	16,054	43,562
Early-stage Dummy	19,495	0.42	0.49	0.00	0.00	1.00
VC Reputation	19,495	0.17	0.42	0.00	0.03	0.16
Industry MB	19,495	0.43	0.90	0.03	0.10	0.36
Number of IPOs at Exit	19,495	19.24	17.42	9.00	13.00	22.00
Number of MAs at Exit	19,495	1772.40	422.91	1,565	1,746	2,051

Table A3: Institutions' Participation and Startup Exit Channel

This table presents the results of our investigation into how institutions' participation affects the channel of exit. We report multinomial logit results. The dependent variable *Exit Category* equals one if a company goes public, two if a company is acquired, and three if a company is liquidated. The key independent variables are *Institution Share*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We also include the following control variables: *Ln(Startup Age at First Round)*, *Ln(Number of Rounds)*, *Ln(Number of VCs)*, *Ln(Total Amount Raised)*, *Early-stage Dummy*, *Lead VC Reputation*, *Industry MB*, *Ln(Lagged number of IPO at exit)* and *Ln(Lagged number of MA at exit)*. The definitions of the control variables are reported in Appendix Table A1. We also include industry and IPO year fixed effects. We standardize all continuous independent variables to facilitate interpretation. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	IPO	M&A	IPO	M&A
	(1)	(2)	(3)	(4)
Institution Shares	0.094***	0.028		
	(0.026)	(0.021)		
Institution Numbers			0.090***	0.023
			(0.027)	(0.022)
Ln (Startup Age at First Round)	0.119***	0.014	0.117***	0.014
	(0.027)	(0.019)	(0.027)	(0.019)
Ln (Number of Rounds)	-0.431***	-0.362***	-0.432***	-0.362***
	(0.038)	(0.025)	(0.038)	(0.025)
Ln (Number of VCs)	0.216***	0.238***	0.216***	0.238***
	(0.039)	(0.026)	(0.039)	(0.026)
Ln (Total Amount Raised)	1.266***	0.319***	1.265***	0.319***
	(0.046)	(0.026)	(0.046)	(0.026)
Early-stage Dummy	-0.170***	-0.081***	-0.170***	-0.081***
	(0.029)	(0.018)	(0.029)	(0.018)
VC Reputation	0.082***	0.098***	0.083***	0.098***
	(0.024)	(0.019)	(0.024)	(0.019)
Industry MB	-0.106**	-0.086***	-0.106**	-0.086***
	(0.042)	(0.020)	(0.042)	(0.020)
Ln (Lagged Number of IPOs at Exit)	0.122***	0.067**	0.122***	0.067**
	(0.046)	(0.027)	(0.046)	(0.027)
Ln (Lagged Number of MAs at Exit)	-0.055	0.011	-0.060	0.011
	(0.112)	(0.077)	(0.112)	(0.077)
Observations	19,495		19,495	
Fixed Effects	YES		YES	
Pseudo R-Square	0.149		0.149	

Table A4: Institutions' Participation and IPO Costs

This table reports the result of our investigation into how institutions' participation affects IPO costs. We report OLS regression results. The dependent variables are *Gross Spread,* which measures the gross underwriting spread, scaled by gross proceeds dollar amount of issuance, and *Proceeds Retention*, which measures the ratio of the net proceeds to the gross proceeds. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Panel A: Gross Spread		Panel B: Proceeds Retention	
	(1)	(2)	(3)	(4)
Institution Shares	-0.044**		0.240***	
	(0.020)		(0.067)	
Institution Numbers		-0.052**		0.233***
		(0.021)		(0.053)
VC Reputation	-0.034***	-0.035***	0.021	0.026
	(0.011)	(0.012)	(0.097)	(0.097)
Tech Dummy	0.096	0.097	-0.280	-0.279
	(0.068)	(0.068)	(0.416)	(0.415)
Top-tier Dummy	-0.227***	-0.225***	1.162***	1.154***
	(0.034)	(0.033)	(0.395)	(0.395)
Prior Market Return	0.012	0.013	0.368**	0.367**
	(0.015)	(0.015)	(0.160)	(0.159)
Secondary Fraction	-0.091***	-0.091***	0.200	0.201
	(0.026)	(0.026)	(0.159)	(0.160)
Ln (Age)	-0.078***	-0.077***	0.590***	0.586***
	(0.024)	(0.024)	(0.210)	(0.209)
Observations	1,895	1,895	1,452	1,452
Fixed Effects	YES	YES	YES	YES
Adjusted R-Square	0.158	0.161	0.011	0.011

Table A5: Institution Classification and Top-tier Underwriter

This table presents the evidence of the substitution effect between dedicated institutions and a top-tier underwriter. We report OLS regression results. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We calculate both *Institution Shares* and *Institution Numbers* separately by institution classification. *Top-tier Dummy* is a dummy variable that equals one if there is at least one underwriter with an updated ranking of nine, defined as in Loughran and Ritter (2004). We control for *Lead VC Reputation*, *Tech Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)
Top-tier Dummy	0.040	0.041
	(0.026)	(0.025)
Dedicated Institution Shares	-0.072	
× Top-tier Dummy	(0.044)	
Non-dedicated Institution Shares	-0.049	
× Top-tier Dummy	(0.036)	
Dedicated Institution Numbers		-0.047**
× Top-tier Dummy		(0.022)
Non-dedicated Institution Numbers		-0.027
× Top-tier Dummy		(0.025)
Dedicated Institution Shares	-0.007	
	(0.031)	
Non-dedicated Institution Shares	-0.016	
	(0.021)	
Dedicated Institution Numbers		0.001
		(0.019)
Non-dedicated Institution Numbers		-0.009
		(0.015)
Observations	1,864	1,864
Control Variables	YES	YES
Fixed Effects	YES	YES
Adjusted R-Square	0.288	0.288

Table A6: Uncertainty and IPO Underpricing

This table presents the results of our investigation into how the effect of institutions' participation on IPO underpricing varies with the uncertainty associated with startups. We report OLS regression results. Columns (1) and (2) present how the institutions' participation effect varies with industry-level analyst |*Forecast error*|, measured as the industry value-weighted average analyst forecast error of quarterly earnings. Columns (3) and (4) present how the institutions' participation effect varies with *Industry Volatility*, measured as the 24-month industry return volatility. The dependent variable is *Initial Return*, which measures the return from the offer price to the first-trading-day closing price. The key independent variables are *Institution Shares*, which measures the total dollar amount invested by all institutions, scaled by the total dollar amount invested by all investors, and *Institution Numbers*, which measures the total number of institutions, scaled by the total number of investors. We also include the following control variables: *Lead VC Reputation*, *Tech Dummy*, *Top-tier Dummy*, *Prior Market Return*, *Secondary Fraction*, *Ln (Age)*, *Ln (Proceeds)*. The definitions of the control variables are reported in Appendix Table A1. We also include IPO year fixed effects and industry fixed effects. We standardize all continuous independent variables to facilitate interpretation. The standard errors are clustered by IPO year. Standard errors are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	|Forecast Error|		Industry Volatility	
	(1)	(2)	(3)	(4)
Institution Shares × |Forecast Error|	-0.002**			
	(0.001)			
Institution Numbers × |Forecast Error|		-0.003**		
		(0.001)		
Institution Shares × Industry Volatility			-0.008*	
			(0.004)	
Institution Numbers × Industry Volatility				-0.007**
				(0.003)
|Forecast Error|	-0.003	-0.004		
	(0.004)	(0.004)		
Industry Volatility			0.036*	0.035*
			(0.019)	(0.019)
Institution Shares	-0.016*		-0.016**	
	(0.008)		(0.007)	
Institution Numbers		-0.014*		-0.016**
		(0.007)		(0.007)
Observations	1,752	1,752	1,889	1,889
Controls	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES
Adjusted R-Square	0.287	0.286	0.287	0.287

Table A7: Post-IPO Institution Holding

This table presents the average fraction of shares held by institutions over 12 quarters after the IPO. The fraction of shares held is defined as shares held by an institution scaled by total shares outstanding. We consider institutions that hold at least one share at the end of the IPO quarter end. Furthermore, the pre-IPO group contains institutions that invest in a startup during its financing rounds. The post-IPO group contains institutions that *do not* invest during financing rounds. T-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)
Quarters after IPO Date	Pre-IPO Investors	Post-IPO Investors	Pre -Post
1	0.15%	0.01%	0.15%***
			(5.462)
2	0.14%	0.01%	0.14%***
			(5.259)
3	0.14%	0.01%	0.14%***
			(5.430)
4	0.13%	0.01%	0.12%***
			(5.320)
5	0.13%	0.01%	0.13%***
			(5.172)
6	0.14%	0.01%	0.13%***
			(4.516)
7	0.14%	0.01%	0.13%***
			(4.425)
8	0.14%	0.01%	0.14%***
			(4.657)
9	0.13%	0.01%	0.12%***
			(3.800)
10	0.12%	0.01%	0.11%***
			(3.455)
11	0.14%	0.01%	0.14%***
			(3.918)
12	0.13%	0.01%	0.12%***
			(2.975)

Table A8: Post-IPO Institution Ownership Concentration

This table presents the difference in institutional ownership concentration between startups with and without institutions' participation. We track institutional ownership concentration over 4 quarters after IPO. We measure institutional ownership concentration using the Herfindahl-Hirschman Index of all institutional holdings. T-statistics are reported in parentheses. Significance Level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1)	(2)	(3)
Quarters after IPO Date	With Institutions	Without Institutions	With -Without
1	0.18	0.20	-0.02
			(-1.343)
2	0.20	0.22	-0.02
			(-1.183)
3	0.20	0.22	-0.02
			(-1.428)
4	0.20	0.22	-0.02
			(-1.440)

Table 25: Descriptive Statistics

This table reports descriptive statistics. Panel A reports descriptive statistics of the CEO and Consumer Optimism Indices and various macroeconomic variables. The optimism indices are aggregated from quarterly surveys conducted by *The Conference Board*. The sample begins from the second quarter in 1976 and ends in 2014. Macroeconomic variables are observed quarterly and seasonally adjusted (where appropriate). They include: dividend yield, default spread, the yield on three-month Treasury bills; GDP growth, consumption growth, labor income growth, unemployment rate, inflation rate consumption-to-wealth ratio, corporate profits, and value-weighted stock market returns. All macroeconomic variables are described in Section 2.3. Panel B reports descriptive statistics of independent variable used in our analysis. They include: return-on-asset, sales growth, one-year earnings surprise, three-years earnings surprise, one-year market return, three-years market return, capital expenditure, net financing activity, and one-year insider trading. All macroeconomic variables are described in Section 3.

	N	Mean	Standard Deviation	Quartile 1	Median	Quartile 3
Panel A: Optimism and Macroeconomics Variables						
CEO Optimism	155	53.97	10.10	48.00	55.00	61.00
Consumer Optimism	155	91.76	24.09	77.54	94.81	106.27
Dividend Yield	155	2.80	1.16	1.86	2.48	3.79
Default Spread	155	1.10	0.47	0.78	0.96	1.27
Treasury Yield (3 month)	155	1.33	0.97	0.57	1.32	1.90
GDP Growth	155	1.46	0.93	1.02	1.38	1.85
Consumption Growth	155	1.54	0.92	1.01	1.48	1.96
Labor Income Growth	155	0.43	0.91	0.05	0.44	0.91
Unemployment Rate	155	6.47	1.57	5.33	6.13	7.40
Inflation Rate	155	0.93	0.85	0.52	0.78	1.22
Consumption-to-Wealth	155	0.48	1.65	-0.86	0.59	1.98
Corporate Profits	155	1.33	8.78	-0.61	2.16	4.78
Panel B: Dependent Variables						
Return-on-Assets (firm level)	96531	3.78	18.93	-0.00	7.22	13.01
Sales Growth (firm level)	71066	11.55	35.97	-2.09	7.53	19.84
One-Year Earnings Surprise	123	-0.00	0.01	-0.01	-0.00	-0.00
Three-Years Earnings	115	-0.00	0.01	-0.01	-0.00	-0.00
One-Year Market Return	154	0.08	0.16	-0.02	0.10	0.18
Three-Years Market Return	146	0.24	0.29	0.07	0.23	0.45
Capital Expenditure (firm	96531	6.82	7.92	2.07	4.37	8.51
Net Financing Activity (firm	89682	5.70	21.90	-1.54	0.00	5.73
One-Year Insider Trading	113	-3.03	5.26	-2.87	-1.30	-0.00

Table 26: Determinants of CEO and Consumer Optimism

This table reports coefficient estimates from time-series regressions of standardized CEO and Consumer Optimism Indices on lagged macroeconomic variables. Macroeconomic variables are observed quarterly and seasonally adjusted (where appropriate). The optimism indices are aggregated from quarterly surveys conducted by *The Conference Board*. The sample begins from the second quarter in 1976 and ends in 2014. We do not report the intercept. *T*-statistics are reported in parentheses and are based on Newey-West standard errors with four lags. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	CEO Optimism				Consumer Optimism			
	Single Regression		Multiple Regression		Single Regression		Multiple Regression	
	Coefficient	Adj. R^2	Coefficient	Adj. R^2	Coefficient	Adj. R^2	Coefficient	Adj. R^2
Dividend Yield	0.01	-0.01	-0.22	0.46	-0.19	0.04	-0.23*	0.67
	(0.08)		(-1.60)		(-1.53)		(-1.95)	
Default Spread	0.05	-0.01	-0.18		-1.01***	0.24	-0.34	
	(0.18)		(-0.83)		(-5.32)		(-1.50)	
Treasury Yield (3 month)	-0.17	0.02	0.14		0.16	0.02	0.40***	
	(-1.28)		(0.83)		(0.88)		(3.15)	
GDP Growth	0.22	0.03	0.27**		0.35***	0.11	0.23**	
	(1.44)		(2.54)		(2.66)		(2.27)	
Consumption Growth	0.23	0.04	0.19		0.32**	-0.09	0.24***	
	(1.52)		(1.61)		(2.59)		(3.28)	
Labor Income Growth	0.19*	0.02	0.09		0.32***	0.08	0.10*	
	(1.92)		(1.24)		(3.39)		(1.96)	
Unemployment Rate	0.29***	0.21	0.38***		-0.40***	0.43	-0.29***	
	(4.68)		(4.28)		(-5.32)		(-5.27)	
Inflation Rate	-0.22	0.03	-0.48***		0.02	-0.01	-0.35***	
	(-1.11)		(-4.13)		(0.14)		(-3.97)	
Consumption-to-Wealth	-0.02	-0.01	-0.01		0.13**	0.04	0.04	
	(-0.34)		(-0.28)		(2.02)		(1.15)	
Corporate Profits	0.04***	0.08	0.03***		0.01	-0.00	0.01	
	(4.87)		(4.55)		(0.65)		(0.89)	

Table 27: CEO and Consumer Optimism and Future Return-on-Assets [%]

This table reports coefficient estimates from system GMM regressions of future ROA on lagged measures of CEO and consumer optimism. The dependent variable is the return-on-assets of firm *i* during fiscal year *t+1* [%].The independent variables include the most recent standardized CEO and Consumer Optimism Indices prior to the beginning of firm *i*'s fiscal year, as well as the lagged dependent variable, Firm Size (market value of equity) measured at the beginning of the fiscal year, Tobin's $Q_{i,t-1}$, and $Cash\_Flow_{i,t-1}$ (measured as Income Before Extraordinary Items plus Depreciation and Amortization, scaled by previous total asset). The sample begins in 1981 and ends in 2014. We do not report the intercept. *T*-statistics are reported in parentheses and are based on standard errors clustered by both firm and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
CEO Optimism	0.60***		0.60***	0.48***		0.49***
	(5.63)		(5.85)	(4.55)		(4.66)
Consumer Optimism		-0.48***	-0.45***		-0.22**	-0.24***
		(-5.54)	(-6.19)		(-2.73)	(-2.90)
Lagged ROA	0.64***	0.63***	0.63***	0.63***	0.63***	0.63***
	(23.03)	(21.83)	(22.97)	(22.49)	(22.69)	(22.55)
Firm Size	-87.10	-134.41*	-80.06	-2.10	-19.68	-0.85
	(-1.38)	(-1.83)	(-1.13)	(-0.03)	(-0.25)	(-0.01)
Tobin's Q	1.66***	1.79***	1.73***	1.66***	1.68***	1.66***
	(8.44)	(9.57)	(9.22)	(10.21)	(9.98)	(10.16)
Cash Flow	-4.34***	-3.72**	-4.04***	-4.14***	-4.15***	-4.14***
	(-3.42)	(-2.65)	(-3.21)	(-3.25)	(-3.25)	(-3.25)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Firm Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	33	33	33	33	33	33
Observations	96,531	96,531	96,531	96,531	96,531	96,531

Table 28: CEO and Consumer Optimism and Future Sales Growth [%]

This table reports coefficient estimates from system GMM regressions of future one-year sales growth on lagged measures of CEO and consumer optimism. The dependent variable is the percentage change in sales growth of firm *i* from the end of fiscal year *t* to the end of fiscal year *t*+1. The independent variables include the most recent standardized CEO and Consumer Optimism Indices prior to the beginning of firm *i*'s fiscal year, as well as the lagged dependent variable, Firm Size (market value of equity) measured at the beginning of the fiscal year, Tobin's $Q_{i,t-1}$, and $Cash\_Flow_{i,t-1}$ (measured as Income Before Extraordinary Items plus Depreciation and Amortization, scaled by previous total asset). The sample begins in 1981 and ends in 2014. We do not report the intercept. *T*-statistics are reported in parentheses and are based on standard errors clustered by both firm and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
CEO Optimism	0.57		0.57	0.94		1.12
	(0.97)		(1.06)	(1.12)		(1.39)
Consumer Optimism		-2.35***	-2.30***		-1.34***	-1.45***
		(-4.16)	(-3.71)		(-3.64)	(-3.93)
Lagged Sales Growth	0.09***	0.09***	0.09***	0.09***	0.09***	0.09***
	(7.43)	(8.63)	(8.42)	(9.55)	(9.69)	(9.66)
Firm Size	-747.70***	-689.71***	-650.51***	-631.20***	-656.31***	-645.45***
	(-3.26)	(-3.06)	(-3.10)	(-3.58)	(-3.66)	(-3.58)
Tobin's Q	3.68***	4.12***	4.04***	4.17***	4.22***	4.17***
	(3.92)	(4.81)	(4.79)	(5.12)	(5.19)	(5.13)
Cash Flow	-11.92**	-9.51**	-9.70**	-7.88*	-7.61*	-7.73*
	(-2.61)	(-2.31)	(-2.34)	(-1.77)	(-1.72)	(-1.74)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Firm Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	33	33	33	33	33	33
Observations	71,066	71,066	71,066	71,066	71,066	71,066

Table 29: CEO and Consumer Optimism and Future Earnings Surprises

This table reports coefficient estimates from time-series regressions of aggregate earnings surprises on lagged measures of CEO and consumer optimism. In Panel A, the dependent variable is the one-year value-weighted average earnings surprise across all firms. In Panel B, the dependent variable is the three-year value-weighted average earnings surprise across all firms. Earnings surprise is the difference between the actual earnings and the analyst consensus forecast, scaled by stock price. The independent variables include the most recent standardized CEO and Consumer Optimism Indices, as well as the lagged dependent variable and lagged value-weighted stock market performance. The sample begins in 1982 and ends 2012. We do not report the intercept. *T*-statistics are reported in parentheses and are based on Newey-West standard errors with four or twelve lags. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: One-Year Earnings Surprises						
CEO Optimism	-0.07		-0.08	0.01		0.05
	(-0.67)		(-0.86)	(0.33)		(1.26)
Consumer Optimism		-0.19***	-0.19***		-0.32***	-0.33***
		(-2.64)	(-2.71)		(-4.14)	(-4.08)
Lagged Earnings Surprise	0.03	0.03	0.03	-0.03***	-0.02***	-0.02***
	(1.22)	(1.49)	(1.63)	(-3.58)	(-4.73)	(-4.83)
Lagged Stock Returns	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(-0.28)	(-0.52)	(-0.02)	(-0.72)	(-0.62)	(-0.75)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Adj R-squared	0.01	0.15	0.16	0.61	0.69	0.69
Observations	123	123	123	123	123	123
Panel B: Three-Year Earnings Surprises						
CEO Optimism	-0.07		-0.08	-0.02		0.03
	(-0.85)		(-1.08)	(-0.51)		(0.69)
Consumer Optimism		-0.11*	-0.12*		-0.28***	-0.29***
		(-1.69)	(-1.86)		(-4.20)	(-3.85)
Lagged Earnings Surprise	0.04***	0.03***	0.04***	-0.01*	-0.00	-0.00
	(3.03)	(3.34)	(3.85)	(-1.93)	(-1.18)	(-1.17)
Lagged Stock Returns	-0.00	-0.00	-0.00	-0.00	-0.00*	-0.00*
	(-1.07)	(-1.33)	(-0.63)	(-1.31)	(-1.72)	(-1.73)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Adj R-squared	0.07	0.12	0.15	0.69	0.78	0.78
Observations	115	115	115	115	115	115

Table 30: CEO and Consumer Optimism and Future Stock Market Returns [%]

This table reports coefficient estimates from time-series regressions of one-year and three-year value-weighted stock market returns on lagged quarterly measures of CEO and consumer optimism. In Panel A, the dependent variable is the one-year excess value-weighted CRSP market return. In Panel B, the dependent variable is the three-year excess value-weighted CRSP market return. The independent variables include the most recent standardized lagged CEO and Consumer Optimism Indices, as well as past value-weighted CRSP market return. The sample begins in the third quarter of 1976 and ends in 2014. We do not report the intercept. *T*-statistics are reported in parentheses and are based on Newey-West standard errors with four or twelve lags. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: One-Year Stock Market Returns						
CEO Optimism	0.23		0.25	-0.13		-0.24
	(0.12)		(0.13)	(-0.07)		(-0.12)
Consumer Optimism		-3.74*	-3.74*		0.72	0.80
		(-1.87)	(-1.86)		(0.19)	(0.21)
Lagged Stock Returns	-0.12	-0.10	-0.11	0.05	0.05	0.05
	(-0.74)	(-0.62)	(-0.74)	(0.36)	(0.34)	(0.34)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Adj R-squared	-0.01	0.04	0.03	0.17	0.17	0.16
Observations	154	154	154	154	154	154

	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Three-Year Stock Market Returns						
CEO Optimism	5.85**		5.94***	0.92		2.08
	(2.20)		(3.02)	(0.37)		(0.83)
Consumer Optimism		-	-		-7.52*	-8.19*
		14.21***	14.25***		(-1.68)	(-1.66)
Lagged Stock Returns	-0.49**	-0.29	-0.44**	-0.29	-0.26	-0.26
	(-1.99)	(-1.57)	(-2.01)	(-1.20)	(-1.19)	(-1.18)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Adj R-squared	0.04	0.23	0.28	0.58	0.60	0.60
Observations	146	146	146	146	146	146

Table 31: CEO and Consumer Optimism and Future Capital Expenditure [%]

This table reports coefficient estimates from system GMM regressions of annual scaled capital expenditure on lagged measures of CEO and consumer optimism. The dependent variable is the aggregate dollar amount of capital expenditures (by firm *i* during fiscal year *t*) divided by the book value of total assets$_{i,t-1}$ [%]. The independent variables include the most recent standardized CEO and Consumer Optimism Indices prior to the beginning of firm *i*'s fiscal year, as well as the lagged dependent variable, Firm Size (market value of equity) measured at the beginning of the fiscal year, Tobin's $Q_{i,t-1}$, and Cash_Flow$_{i,t-1}$ (measured as Income Before Extraordinary Items plus Depreciation and Amortization, scaled by previous total asset).. The sample begins in 1981 and ends in 2014. We do not report the intercept. *T*-statistics are reported in parentheses and are based on standard errors clustered by both firm and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
CEO Optimism	0.32***		0.32***	0.15***		0.14***
	(4.29)		(4.44)	(3.62)		(3.60)
Consumer Optimism		0.12*	0.14***		0.11**	0.11**
		(2.02)	(3.34)		(2.25)	(2.16)
Lagged Capital Expenditure	0.43***	0.42***	0.42***	0.41***	0.41***	0.41***
	(18.90)	(18.26)	(19.22)	(17.57)	(17.57)	(17.58)
Firm Size	-164.01***	-206.86***	-168.73***	-97.20***	-105.26***	-98.05***
	(-3.46)	(-4.53)	(-3.75)	(-3.21)	(-3.47)	(-3.28)
Tobin's Q	0.99***	0.99***	0.97***	0.88***	0.89***	0.88***
	(13.96)	(13.87)	(13.18)	(15.75)	(16.01)	(15.95)
Cash Flow	5.43***	5.44***	5.35***	4.77***	4.75***	4.76***
	(8.59)	(8.73)	(8.31)	(7.77)	(7.82)	(7.76)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Firm Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	33	33	33	33	33	33
Observations	96,531	96,531	96,531	96,531	96,531	96,531

Table 32: CEO and Consumer optimism and Future Net Financing Activity [%]

This table reports coefficient estimates from system GMM regressions of an annual net financing activity measure on lagged measures of CEO and consumer optimism. The dependent variable is the aggregate dollar amount raised through both external equity and debt issuance (by firm *i* during fiscal year *t*) divided by the book value of total assets$_{i,t-1}$ [%].The independent variables include the most recent standardized CEO and Consumer Optimism Indices prior to the beginning of firm *i*'s fiscal year, as well as the lagged dependent variable, Firm Size (market value of equity) measured at the beginning of the fiscal year, Tobin's $Q_{i,t-1}$, and Cash_Flow$_{i,t-1}$ (measured as Income Before Extraordinary Items plus Depreciation and Amortization, scaled by previous total asset).. The sample begins in 1981 and ends in 2014. We do not report the intercept. *T*-statistics are reported in parentheses and are based on standard errors clustered by both firm and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
CEO Optimism	0.30*		0.29*	-0.06		-0.07
	(1.95)		(1.97)	(-0.27)		(-0.31)
Consumer Optimism		0.44***	0.45***		0.22	0.22
		(3.00)	(3.20)		(1.64)	(1.63)
Lagged Net Financing Activity	0.05***	0.05***	0.05***	0.05***	0.05***	0.05***
	(5.94)	(5.59)	(5.70)	(5.65)	(5.65)	(5.63)
Firm Size	-256.98	-307.83**	-281.14*	-243.32	-249.96	-242.56
	(-1.68)	(-2.06)	(-1.85)	(-1.51)	(-1.53)	(-1.51)
Tobin's Q	4.95***	4.88***	4.85***	4.82***	4.80***	4.82***
	(10.04)	(10.07)	(9.85)	(11.34)	(11.37)	(11.30)
Cash Flow	-9.17*	-9.64*	-9.67*	-10.13*	-10.14*	-10.14*
	(-1.81)	(-1.86)	(-1.89)	(-1.88)	(-1.89)	(-1.88)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Firm Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	33	33	33	33	33	33
Observations	89,682	89,682	89,682	89,682	89,682	89,682

Table 33: CEO and Consumer Optimism and Future Insider Net Purchases [Mill.]

This table reports coefficient estimates from time-series regressions of one-year value-weighted net insider purchase on lagged quarterly measures of CEO and consumer optimism. The dependent variable is the aggregate value of shares purchased minus the aggregate value of shares sold across all CEOs, value-weighted by firms' lagged market capitalization. The independent variables include the most recent standardized lagged CEO and Consumer Optimism Indices, as well as previous insider trading, and lagged value-weighted stock market performance. The sample begins in the first quarter of 1986 and ends in the first quarter of 2014. We do not report the intercept. *T*-statistics are reported in parentheses and are based on Newey-West standard errors with four lags. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
CEO Optimism	0.44		0.40	1.01		1.28
	(0.68)		(0.70)	(0.98)		(1.17)
Consumer Optimism		-1.43**	-1.42**		-2.00**	-2.31**
		(-2.11)	(-2.12)		(-2.24)	(-2.08)
Lagged Insider Trading	0.44**	0.36**	0.35**	0.26	0.25	(-2.08)
	(2.41)	(2.28)	(2.32)	(1.57)	(1.58)	0.24
Lagged Stock Returns	1.42	3.26	1.90	2.71	4.85	2.95
	(0.21)	(0.61)	(0.31)	(0.42)	(0.83)	(0.50)
Macroeconomic Controls?	No	No	No	Yes	Yes	Yes
Adj R-Squared	0.29	0.36	0.36	0.39	0.4	0.42
Observations	113	113	113	113	113	113

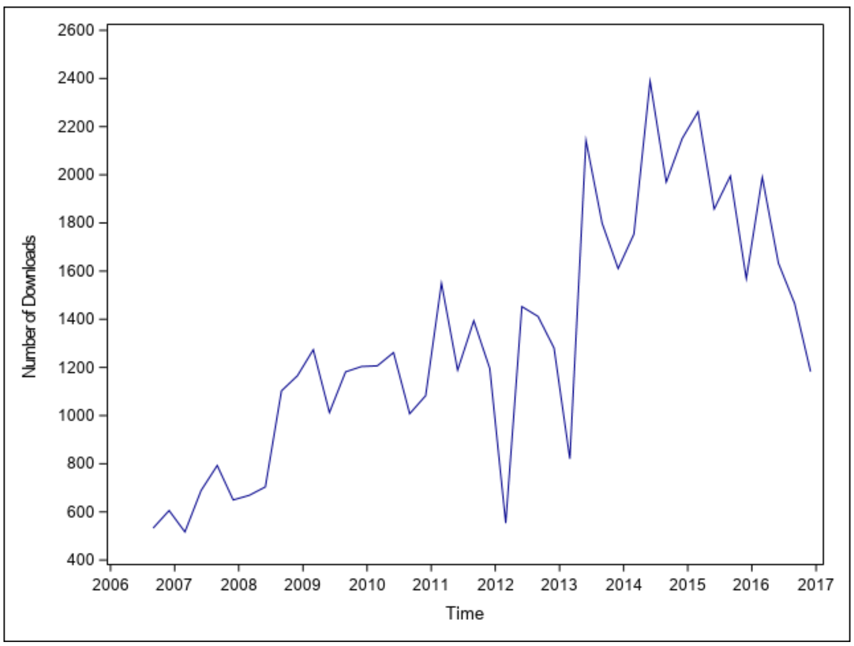
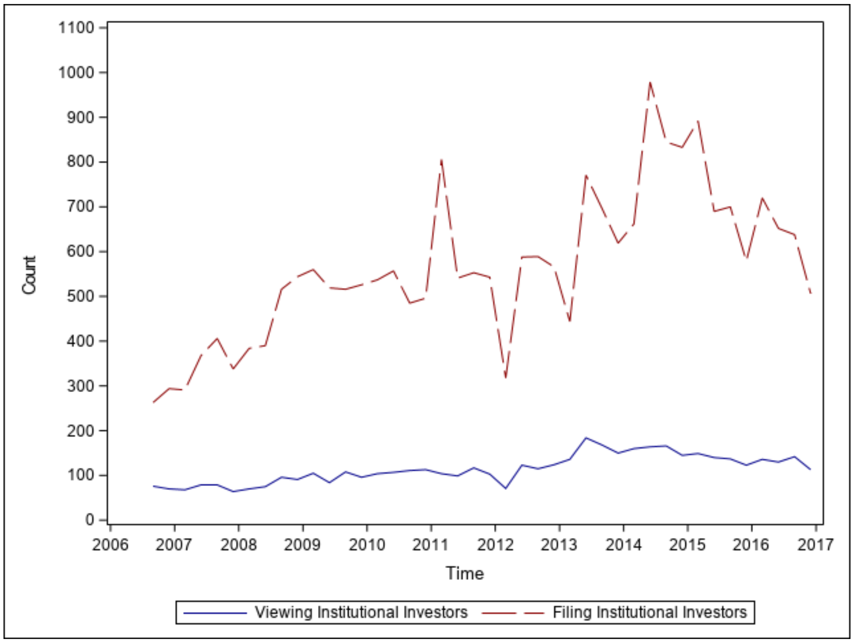


Figure 1: Download Activity of 13-F Filings

This figure presents the time-series of 13-F downloads activities from the third quarter of 2006 to the last quarter of 2016.



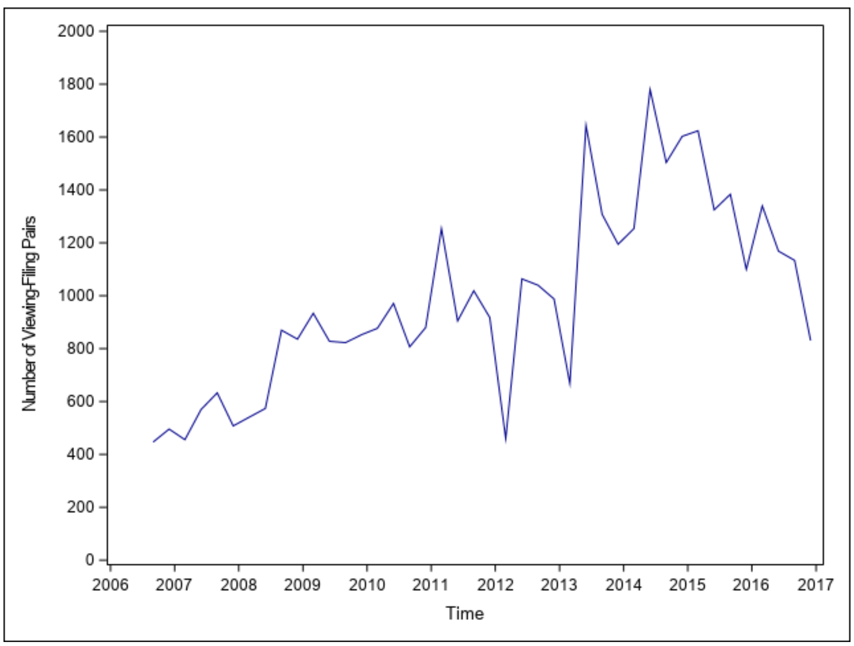


Figure 2: Number of Institutional Investors

These figures present the time-series of number of institutional investors involved in the information acquisition process from the third quarter of 2006 to the last quarter of 2016.. The top figure plots the number of unique viewing and filing institutional investors over time. The bottom figure plots the number of unique viewing-filing institutional investor pairs over time.

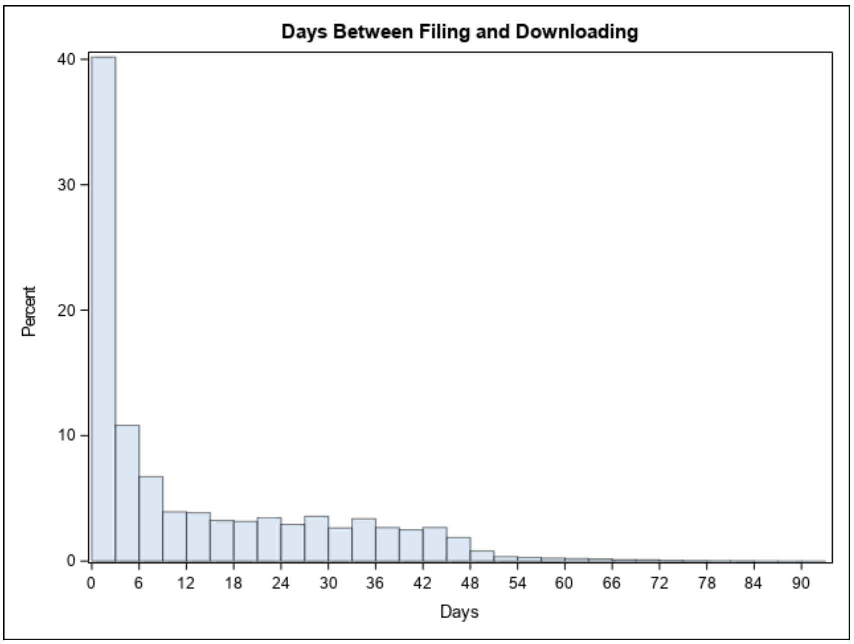


Figure 3: Download Activity of 13-F Filings

This figure presents the histograms of duration between the filing and the viewing of 13-F filings from the third quarter of 2006 to the last quarter of 2016.

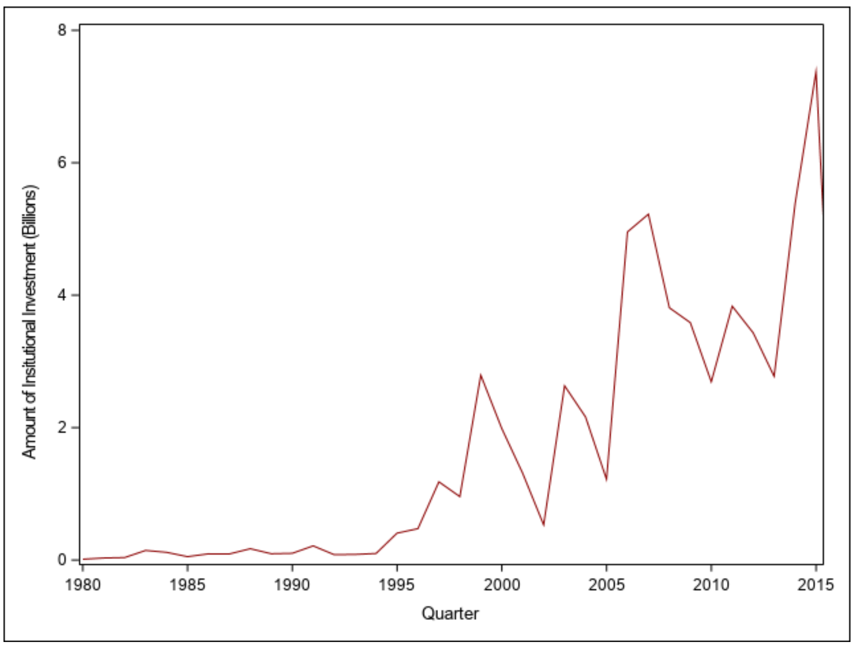


Figure 4: Institutions' Participation in Startups

This figure presents the time-series variation of investments made by institutions. We plot the annual total dollar amount of investment made by institutions in startups.

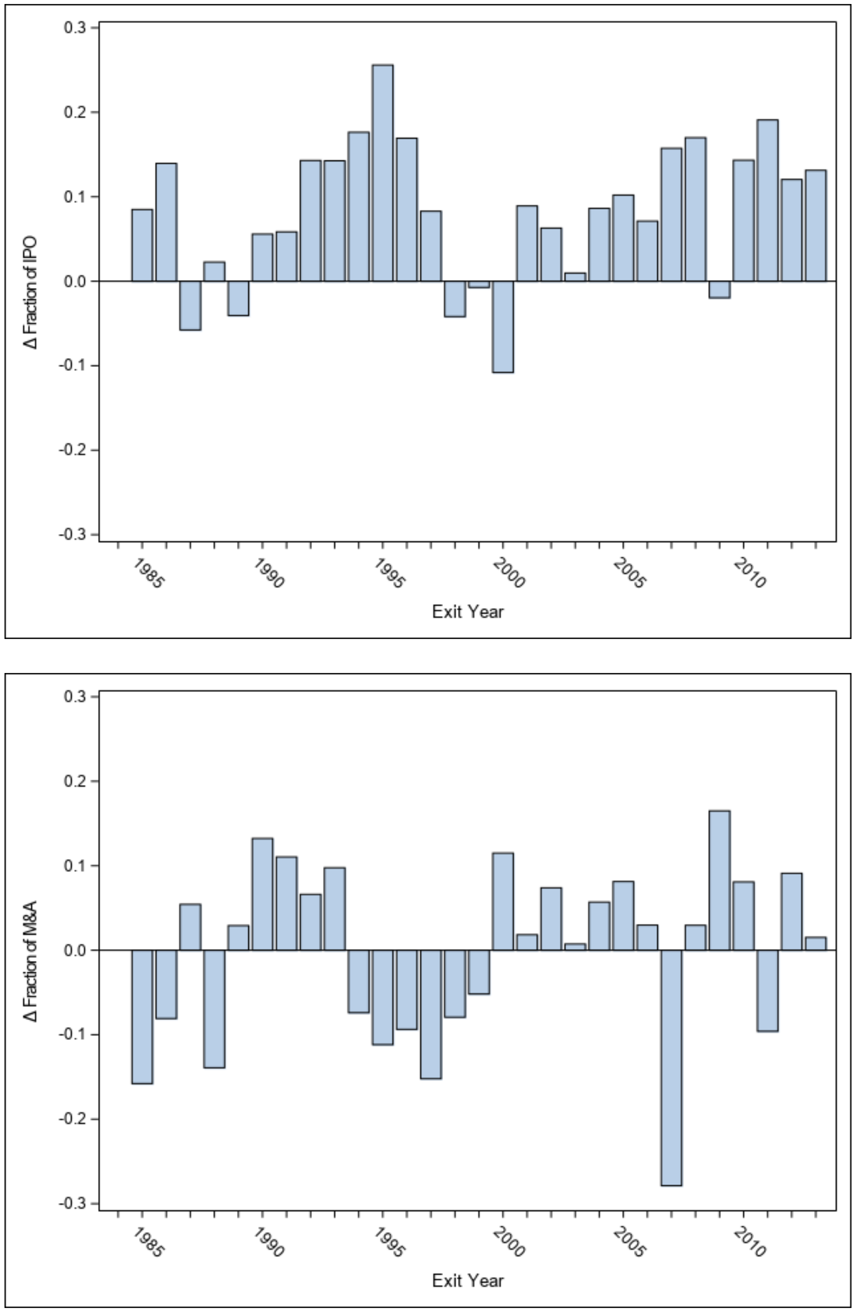


Figure 5: Difference in Exit Status

This figure presents the difference in exit status between startups with and without institutions' participation. For a given exit status, a positive number indicates a higher fraction of startups with institutions' participation. In the top figure, we plot the fraction of startups going public by startup exit year. In the bottom figure, we plot the fraction of startups being acquired by startup exit year.

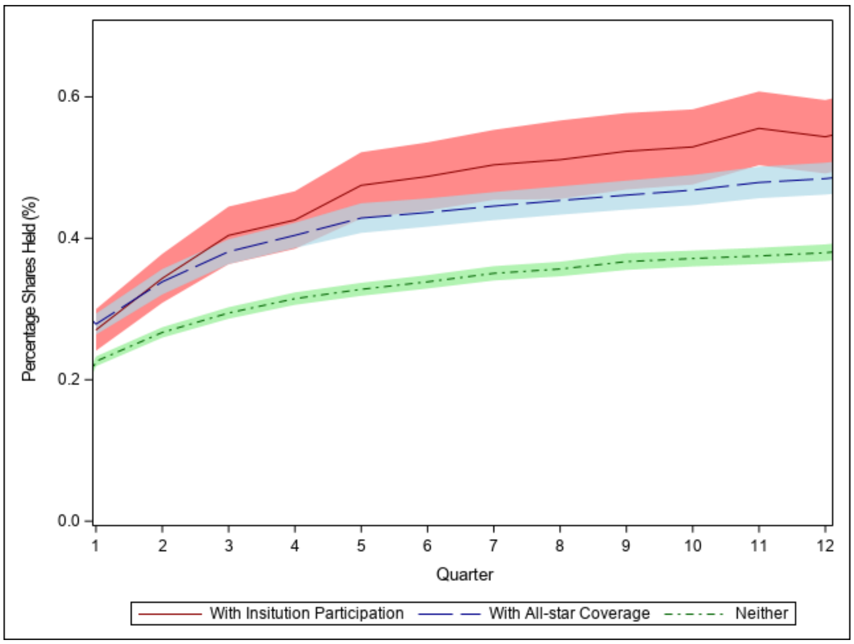


Figure 6: Post-IPO Institutional Ownership

This figure plots average institutional ownership over 12 quarters after the IPO quarter. We plot the average fraction of shares held as well as the 95% confidence intervals for IPO firms with all-star analyst coverage, pre-IPO institutions' participation, and neither. The fraction of shares held is defined as shares held by all institutions scaled by total shares outstanding.

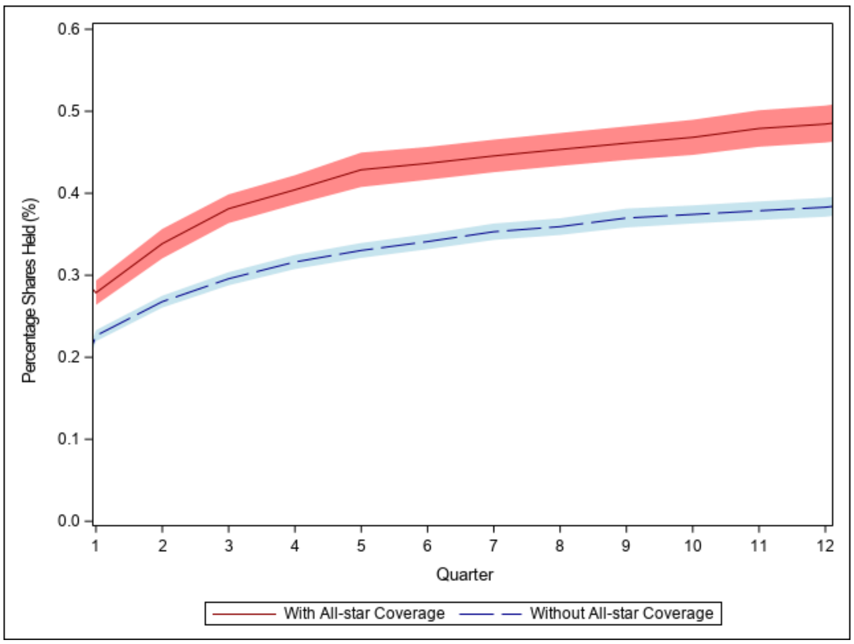
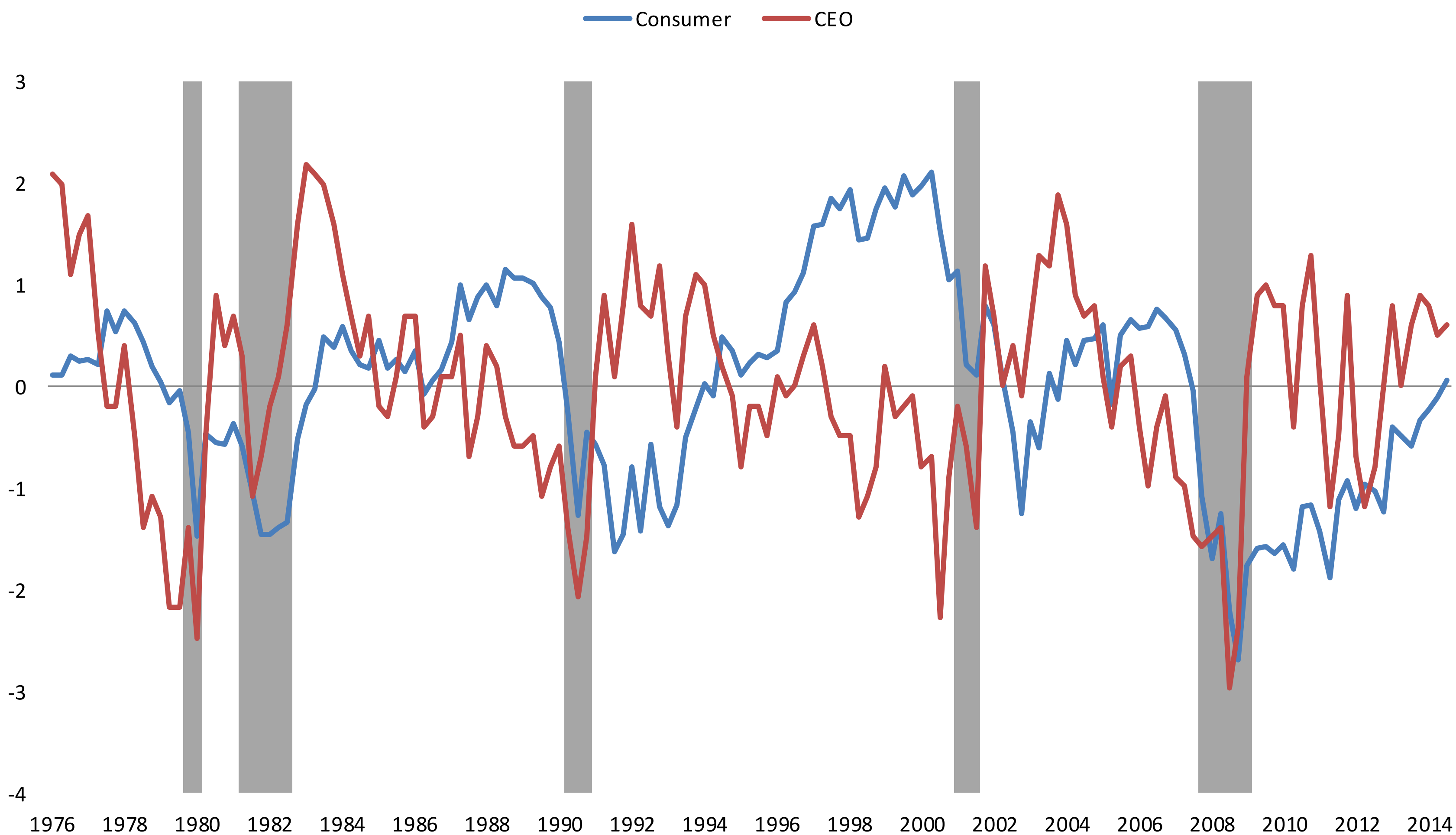


Figure A1: All-Star Coverage Effect on Post-IPO Institutional Ownership

This figure plots average institutional ownership ratio over 12 quarters after the IPO quarter. We plot the average fraction of shares held as well as the 95% confidence intervals. The fraction of shares held is defined as shares held by all institutions scaled by total shares outstanding. We plot the average institutional ownership ratio for IPO firms with and without all-star analyst coverage.

Figure 7: CEO and Consumer Optimism

The figure plots quarterly standardized values of our aggregate CEO and Consumer Optimism Indices. The survey data is obtained from *The Conference Board* and covers the period 1976-2014. NBER recession periods are designated by gray bars. The sample period covers 1976-2014.



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