Essays on Investor Attention and Information Acquisition

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DEDICATION

To Jessie, for supporting my endeavors and making life lovelier.

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ABSTRACT

This dissertation examines the relation between capital market participants' attention to fundamental information and capital market outcomes. In the first essay, I examine whether investors' attention to fundamental information helps predict merger and acquisition activity. In the second essay, I examine the impact of passive ownership on the acquisition of fundamental information.

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CHAPTER I

INFORMATION ACQUISITION AND ACQUISITION OUTCOMES

Abstract

Given the large abnormal returns to targets of mergers and acquisitions, investors have long sought information to predict subsequent takeover announcements. As firms are often targeted because of an attractive fundamental position, I test whether investors' attention to fundamental information acquired via the SEC's EDGAR website predicts subsequent takeover activity. In the period following takeover bids, investors focus information gathering activities on less informationally opaque industry peers of targeted firms. Following an acquisition announcement, EDGAR download activity helps predict subsequent takeover bids for peer firms in the same industry. This effect is especially strong among firms with low market-to-book ratios and those with low prior returns, supporting the view that M&A announcements incite interest in undervalued firms in a competitive takeover market. Attention to fundamentals is a better predictor of future bids in peers with high announcement period returns, suggesting that the market impounds into prices information related to EDGAR download activity. Fundamental information gathering predicts increased incidence and hazard rate of deal completion.

1. Introduction

Abnormal returns to firms targeted for acquisition are around 20% to 30% in the days around takeover announcements.[1] The magnitude of these returns generates substantial interest among academics and practitioners in determining whether public information can be used to predict which firms will be targeted in mergers and acquisitions (M&A).

An early strand of the literature dedicated to the *ex-ante* identification of firms targeted in M&A examines whether information found in financial statements can predict M&A outcomes. Supporting the view that underperforming firms are targeted because potential gains might accrue to bidders that make operational improvements in acquired firms, prior studies demonstrate that low pre-announcement period stock returns (Palepu (1986)) and market-to-book ratios (Wansley, Roenfeldt, and Cooley (1983); Hasbrouck (1985); Barber, Palmer, and Wallace (1995)) help determine which firms will become takeover targets.

Recent work also suggests that industry linkages to firms targeted in an M&A impact subsequent merger activity. Mergers tend to cluster across industries over time because of industry shocks brought about by input price volatility, deregulation, or technological change (e.g., Mitchell and Mulherin (1996); Harford (2005)). When firms announce that they have been targeted, industry peers experience abnormal returns (Eckbo (1983, 1985)). Song and Walking (2000) demonstrate that these abnormal returns are higher for peer firms that subsequently become targets. Their results support the view that after initial M&A announcements, returns to industry peers increase because of the increased probability that they will be targeted next.

My paper brings together these two strands of literature. I first examine whether attention to industry peers' fundamental information increases around the announcement that another firm in the

[1] See Ang and Cheng (2006), Netter, Stegemoller, and Wintoki (2011), and Graham, Harvey, and Puri (2013), among many others.

industry has been targeted for a merger or acquisition. Then, I test whether these increases in attention to fundamental information predict that peer firms will be subsequently targeted. I define firms that are initially targeted for acquisition as "primary firms," and firms in the same 3-digit SIC industry as the primary firms as "peer" firms.

I construct measures of attention to fundamental information using the log file of the Security and Exchange Commission's (SEC's) Electronic Data Gathering, Analysis, and Retrieval (EDGAR) search traffic. These data allow me to observe the IP addresses of each computer that downloads a document from EDGAR, the date and time of the download, and the firm-specific filing that was downloaded. I consider views of filings, such as 10-Ks and8- Ks, as an indication of attention to firms' fundamental information.

I find that increases in investors' acquisition of industry peers' fundamental information via EDGAR after M&A announcements significantly predicts which peers are subsequently targeted. This effect is especially strong among firms with low market to book ratios and firms with low prior stock returns, supporting the view that takeover announcements incite interest in undervalued firms. Attention to fundamentals is a better predictor of future bids in peers for which high cumulative announcement period returns (CARs) provide a confirming signal about the probability of receiving a bid. This suggests that the market impounds into industry peer firms' prices information acquired from EDGAR downloads that informs estimates of the probability that they will be targeted. The results of my final set of tests demonstrate that attention to fundamental information is associated with increases in both the incidence and hazard rate of deal completion.

The link between the acquisition of industry peers' fundamental information after M&A announcements and the probability that they will receive future takeover bids could be causal or predictive. On one hand, M&A announcements could cause potential bidders or their investment bankers to acquire industry peers' information in an effort to determine which among a dwindling pool of potential targets are most suitable for acquisition. It could also be the case that after M&A announcements, hedge funds acquire fundamental information on the set of industry peers they predict are most likely to receive takeover bids in order to inform stock purchase decisions. Because of data limitations, I am unable to ascertain the identities of information acquirers and so make no conjecture regarding their characteristics. Rather, I address six key questions in my analyses.

First, I test whether investors' attention to peer firms' fundamental information changes in the 20-day period around primary firm announcements. In these tests, I regress the daily sum of views of each peer firm's documents on day dummy variables. I find no significant increases in attention to the full set of peers on average around primary firm announcements. However, there is cross-sectional variation in attention to fundamental information. Focusing on the set of peers that are subsequently acquired, I find that attention to fundamental information increases in the days after primary firm announcements. For peers that are targeted for acquisition within 30 days of primary firm announcements, the number of views on the day of the announcement increases by over 10% (=1.23/12.31) relative to the mean number of views over the prior 10 days. For firms targeted within 90 days, the number of views on the announcement day increases by 4.6% relative to the mean number of views over the prior 10 days. These results provide the first evidence that information acquisition may inform the probability of subsequent takeover.

In my second analysis, I examine whether informational opacity impacts the degree to which investors pay attention to peer firms' fundamental information. I consider high bid ask spreads, low analyst following, high discretionary accruals, and small firm size to be proxies for higher informational opacity. I sort firms into opacity terciles and regress the daily number of views of each peers' SEC filings on a high opacity tercile dummy variable that is interacted with an indicator that is equal to one if the day on which the view occurs is in days [-10, -1] around the primary firm announcement, and zero if the day on which view occurs is in days [0, +9]. I find that relative to less informationally opaque peers, smaller firms and those with higher spreads and lower analyst following receive less attention on days [0, +9] than on days [-10, -1]. In addition, I find no significant variation in post-announcement peer attention to firms with respect to accruals.

In my third analysis, I formally test whether increased attention to industry peers' fundamental information predicts that they will be subsequently targeted after controlling for other known predictors of M&A targeting. In these tests, I regress indicator variables for whether a peer firm is targeted in 30 or 90 days on the change in the number of peer views around a primary firm announcement and peer control variables. Overall, the results support my previous findings and show that increases in attention to fundamentals predict subsequent merger announcements and that this predictive ability is incremental to other known predictors of M&A. In terms of economic significance, a one standard deviation increase in the number of a peer firm's document views is associated with a 46.6% relative increase in the likelihood that it will be targeted within the next 30 days and a 17.7% relative increase in the likelihood that it will be targeted in the next 90 days.

In my fourth analysis, I examine whether attention to firms' fundamental information is a better predictor of future bids in peer firms for which high announcement period returns provide a confirming signal about the probability of receiving a bid. This analysis is motivated by Song and Walkling's (2000) Acquisition Probability Hypothesis, which asserts that peers earn abnormal returns because of the increased probability that they will become targets themselves. They show that while all peers earn significantly positive CARs around primary firm announcements, abnormal returns to peers that are subsequently targeted are significantly greater. They also demonstrate that announcement period CARs are systematically related to variables associated with the probability of acquisition. Taken together, their results suggest that some market participants correctly anticipate future acquisition attempts.

To test whether attention to firms' fundamental information is a better predictor of future bids in peer firms for which high announcement period returns provide a confirming signal about the probability of receiving a bid, I sort peer firms into primary firm announcement-level CAR terciles and interact tercile dummies with a measure of change in attention. Regression results indicate that relative to peers in the lowest primary firm announcement period CAR tercile, a one standard deviation increase in attention to those in the highest CAR tercile is associated with a 26.6% increase in the relative likelihood that they will be targeted for merger or acquisition in the following 30 days.

Fifth, I test for which firms is the bridge between the acquisition of information and subsequent acquisition activity strongest. Agency theories suggest that firms are targeted for takeover because of potential gains to bidders associated with mitigating value-decreasing agency problems at target firms (Jensen and Meckling (1976)), while neoclassical explanations suggest that M&As are mechanisms through which bidders seek to profit from the redeployment of under-utilized assets and therefore target firms with low market values relative to book values. To determine whether attention is a more informative predictor for underperforming firms, I sort peer firms into primary firm announcement-level terciles of either Tobin's Q or one-year buy and hold abnormal returns (BHARs) and interact tercile dummies with the change in attention measure. The results indicate that relative to the best performers, a one standard deviation increase in attention to a firm in the lowest Tobin's Q tercile is associated with an 17.9% increase in the probability that it will be targeted in the following 30 days. Also, a one standard deviation increase in attention to a firm in the lowest BHAR tercile is associated with an 22.2% increase in the probability that it will be targeted in the following 30 days.

In my final set of analyses, I examine whether fundamental information gathering around the announcement that firms are targeted for M&A predicts deal completion and affects the timing of deal completion. For these tests, I focus on the set of firms that are targeted. Overall, I find that the firms with the largest increase in abnormal attention to fundamentals are more likely to have the deal completed and the amount of time between the announcement and the completion of their deals decreases. In terms of economic significance, a one standard deviation increase in abnormal views is associated with an 7.7% increase in the likelihood that an announced merger will be completed as well as a 53% increase in the hazard rate of completion (i.e., the probability that it will be taken over at any given time given that it has survived until that point).

This paper contributes to three areas of the literature, the first of which is the determinants of M&A. To my knowledge, this is the first paper to examine whether information gleaned from the SEC's EDGAR log file predicts M&A outcomes. Prior work on M&A prediction models has focused on the ability of performance (Palepu (1986); Morck, Schleifer and Vishny (1988, 1989); Lang, Stulz, and Walkling (1989)), leverage (Stevens (1973); Billett (1996)), or firm size (Offerberg (2009)) to predict which firms are targeted. I extend this work by showing that a measure of attention to fundamental information significantly predicts which firms will receive future M&A bids. Additionally, I contribute to the discussion of whether performance predicts which firms are targeted by demonstrating that attention to fundamental information is a stronger predictor of future bids in underperforming firms.

This paper also contributes to the literature on the relation between industry linkages and merger activity. Prior work shows that mergers tend to cluster within industries over time (Mitchell and Mulherin (1996); Harford (2005)), that industry links result in abnormal returns to peers around primary firm announcements (Eckbo (1983, 1985)), and that these returns are greater for peers with characteristics associated with an increased probability of receiving a bid (Song and Walkling (2000)). I extend this work by showing that increases in attention to the fundamental information of industry peer firms after primary firm announcements predict which industry peers will be targeted.

Finally, I extend the literature on investor attention. Prior research shows that investor attention is time-varying and that it impacts stock prices (Da, Engelberg, and Gao (2011)). Further, cross-sectional differences in stock returns, growth rates, and profitability ratios are better explained by investor attention to fundamentals than by industry classifications (Lee, Ma, and Wang (2014)). Investor attention to fundamental information changes around corporate events. Demand for firms' SEC filings around earning announcements is associated with increased accounting discretion and negative earnings shocks (Drake, Roulstone, and Thornock (2015)). Attention to fundamentals can also predict corporate outcomes. Increased gathering of initial public offering filers' fundamental information is positively related to deal success and can predict both price revisions and initial returns (Bauguess, Cooney, and Hanley (2015)). I extend this literature by studying investor attention to fundamental information in the setting of M&A and by showing that it impacts the probability that firms are targeted in M&A as well as M&A outcomes.

The rest of the paper is organized as follows. Section 2 describes my sample of M&As, the construction of attention measures using the EDGAR Log file dataset, and the construction of control variables. Section 3 provides empirical results. Section 4 concludes.

2. Databases and Sample Selection

2.1. EDGAR Log File Data

I use demand for firms' financial disclosures and forms as a direct measure of investors' attention to fundamental information. Securities regulations require publicly traded firms to file periodic reports (e.g., 10-K, 10-Q), material disclosures (e.g., 8-K), proxy materials (e.g., DEF 14A), and other forms (e.g., DFAN 14A) with the SEC. The SEC makes these documents available for download via their EDGAR website. When investors download a firm's documents from EDGAR, the SEC captures metadata pertaining to the download requests. These data are recorded in the EDGAR log file dataset and include the downloader's partially anonymized IP address, the date and time of the download, firm-specific CIK numbers (which are used on the SEC's computer systems to identify corporations and individual people who have filed disclosures with the SEC), and accession numbers (unique identifiers assigned to an accepted submission by the EDGAR filing system that identify each filing). EDGAR log file data are available on the SEC website from 1/1/2003 to 6/30/2017.[2] As Loughran and McDonald (2017) note, observations in the EDGAR Log file dataset beginning 9/24/2005 and ending 5/10/2006 are either lost or damaged. Therefore, following prior work, I collect log file data for the period beginning 6/1/2006 and ending 12/31/2016.

In my study, I focus on investor attention, proxied by downloads of SEC documents. Importantly, many downloads of SEC documents recorded in the EDGAR log file dataset are performed by "robots," or algorithms that are used to download a large number of multiple firms' documents in a short period of time. Loughran and McDonald (2017) classify successful downloads from EDGAR of non-index pages by IP addresses not self-identified as web crawlers in which the requestor downloads fewer than 50 documents per day as "non-robot" downloads. They find that during March 2012, robots downloaded nearly 45 million documents from EDGAR, whereas non-robots downloaded less than one million documents. Because I am interested in learning about the effects of investor attention on M&A outcomes, I clean the EDGAR log file data of robot downloads following the method in Ryans (2017). This process involves excluding IP addresses that downloaded more than 25 documents per

[2] These data can be downloaded at https://www.sec.gov/dera/data/EDGAR-log-file-data-set.html.

minute, the documents of more than three firms per minute, or more than 500 total documents in any day. Further, I eliminate downloads in which the user self-identifies as a web crawler, logs of visits to index pages, or downloads not identified with Apache code 200 (which indicates that a request is received and understood by the SEC's computer system).

Not every SEC filing is relevant to my analysis. For example, many investors examine firms' Form 4 filings to obtain information regarding insider transactions. Because I expect insider transactions to convey little information about firm or industry specific conditions pertinent to mergers and acquisitions, I omit views of Form 4s from my analysis and focus on documents that are related to material disclosures, fundamentals, proxy statements, and material events.[3] I obtain the name of each sample form type from WRDS SEC Analytics by merging on accession number, and GVKEY and PERMO identifiers from the CRSP/COMPUSTAT Merged database. For each sample firm, I calculate my primary attention measure, *Views*, as the number of its documents downloaded across all IP addresses each day.

2.2. Merger and Acquisition Data

I obtain merger data from Thomson Reuters' Securities Data Corporation (SDC) database. Because data available in the EDGAR log file range from 6/1/2006 to 12/31/2016 and I require 180 days of attention data before announcements and 10 days of attention data after announcements, I collect data on mergers involving public US targets that were announced between 1/1/2007 and 12/20/2016. In my main tests, I focus on peer attention around announcements of primary firm mergers that are ultimately acquired. Following Baker, Pan, and Wurgler (2012), I require that acquirers hold

[3] I use downloads of the following SEC forms in my analyses: 10-K, 10-K/A, 10-Q,10-Q/A, 11-K, 11-K/A, 20-F, 20- F/A, 40-F, 40-F/A, DEF 14A, DEF 14C, DEFA 14A, DEFA 14C, DEFC 14A, DEFC 14C, DEFM 14A, DEFM 14C, DEFN 14A, DEFR 14A, DEFR 14C, DEFS 14A, DFAN 14A, DFRN 14A, 8-K, 8-K/A, 6-K, and 6-K/A. For robustness, I conduct my main analyses without excluding Form 4 views, which are the omitted form type to which investors pay the most attention. The results of these robustness tests are qualitatively similar to those I present in Table 5.

less than 50% of primary firms' shares at the time of deal announcement and that they own 100% after deal completion. I eliminate from the sample all deals that SDC characterizes as repurchases or recapitalizations. I also eliminate all deals involving targets in the finance and utility industries because mergers involving firms in these industries are heavily regulated and may be systematically different from mergers involving firms in other industries. I require that primary firms' accounting information and stock return data are available in CRSP and COMPUSTAT for the full fiscal year before their announcement date. Additionally, I eliminate observations for which a firm's first appearance in the EDGAR log file dataset occurs less than 180 calendar days before the announcement date because I use these data to construct a baseline measure of attention. Panel A of Table 1 presents the number of primary firm announcements that occur each year. The final sample contains 1,085 completed primary firm mergers or acquisitions. On average, nearly 122 deals are announced per year in the first half of the sample and around 95 are announced per year in the second half.

For each of the primary firms targeted in my main sample, I define its peer firms as those in the same 3-digit SIC industry.[4] The final sample contains 110,086 primary-peer firm combinations. On average, there are 101 peers per primary firm announcement. Table 2 presents the average number of views of peer firm documents on each of the [-10, +9] days around primary firm announcements. Panel A presents data for the full sample, and Panel B (C) presents data for the sample of firms that are targeted within 30 (90) days. Relative to the average number of views over days [-10, -1], the average number of views on days [0, +9] increases by 1.06% in the full sample and 1.24% (1.12%) in the sample of firms that are targeted in the following 30 (90) days. In the full sample, there appears to be a small spike in attention to peer firms around primary firm announcement dates ($Date_{t=0}$). In the

[4] For robustness, I perform my main analyses using peer firms defined to be those in the same two or four-digit SIC industries as primary firms and find that my main results remain unchanged

subsamples of peer firms that are taken over in the 30 or 90 days following primary firm M&A announcements, spikes in peer firm attention around initial announcement dates appear to be persistent and larger than those in the full sample. This provides the first evidence that primary announcements may pique investor attention in the industry peer firms that are subsequently acquired.

2.3. Variable Construction

The main variables of interest in my primary analyses measure changes in attention to industry peer firms' fundamental information around primary firm announcement dates. To test whether increases in attention to peer firms' information predicts that they will be subsequently targeted for an acquisition, I construct Δ*Views*, which equals the average number of views of a peer firm *i*'s documents during the 10-day period beginning on primary firm *j*'s announcement date minus the average daily number of views of the peer firm's documents during the prior 10-day period. In these tests, I define *Targeted 30 (Targeted 90)* as an indicator variable that is equal to one if a peer firm is targeted within 30 (90) days of a primary firm announcement and zero otherwise. In tests of the relation between attention and deal completion, I define *ABViews* as the average number of views of firm *j*'s documents during the [-10, +9] days around its merger or acquisition announcement scaled by the average number of views of its documents in the [-180, -11] days before the announcement. The dependent variable in these tests is *Completed*, which is an indicator variable that equals one if the deal is completed, and zero if the deal is withdrawn.

In my multivariate tests, I include controls for known predictors of M&A. These variables are defined in the appendix. This set of control variables includes firm size, book leverage, Tobin's Q, the property ratio (the ratio of fixed assets to total assets), the liquidity ratio (cash and short-term investments scaled by assets), year over year sales growth, return on assets (ROA), and a Herfindahl-Hirschman Index based on sales. When examining whether attention predicts that peer firms will be targeted for an acquisition, I control for one-year buy and hold abnormal returns for each peer firm over the fiscal year ending before the associated primary firm's announcement date as well as peer firms' two-day cumulative abnormal returns (CARs) around primary firms' announcement dates.

In tests examining the relation between attention and deal completion, I create a subsample of firms in which announcements indicate that either bidders seek to own 100% of a target firm's shares and the firm is ultimately acquired or else that bidders seek to own 100% of a target firm's shares and the deal is ultimately withdrawn. In these tests, I control for BHARs to each target firm over the one-year period ending on its announcement date, its CARs in the two-day period around the announcement date, bidding firms' public status, and conditional upon deal completion, the percentage of the target firm's shares tendered in the deal, and indicator variables for whether the bidder pays for the target firm completely with cash or whether the payment method includes a portion of the acquiring firm's stock.

These controls are motivated by the M&A literature.[5] Small firms may be attractive takeover targets because of the size-related transaction costs associated with buying firms, the costs associated with prolonged proxy fights, and the costs associated with organizationally absorbing and integrating target firms (Palepu (1986); Ambrose and Megginson (1992); Song and Walkling (1993); Comment and Schwert (1995)). On the other hand, Offenberg (2009) calculates the change in shareholder value around nearly 8,000 acquisitions that take place in the U.S. during between 1980 and 1999, and finds that among firms that make value-destroying bids, the largest are more likely than the smallest to be targets of disciplinary takeovers.

[5] Apart from my variable of interest and CARs, each of the controls in my models follow Karpoff et al. (2017). In comparing the sign and magnitude of the coefficients presented in their Table 4 with my main results, I find that *Size* is the only covariate for which Karpoff et al. (2017) reports an estimated coefficient that is statistically significant and differently signed than a statistically significant coefficient reported in Table 4 of my paper.

Merger activity may also be greater in industries that are dominated by one or a few large firms to the extent that mergers of smaller firms increase their economies of scale (Gort (1969)). However, bidder returns tend to increase as industry concentration decreases (Eckbo, Maksimovic, and Williams (1990); Song and Walkling (2000)), because more competitive environments may encourage firms to make better acquisition decisions.

Agency problems and poor management practices result in opportunities for acquirers to benefit from operational improvements after an acquisition (Jensen and Meckling (1976)). Low leverage and excess liquidity may make firms attractive takeover targets because of potential agency conflicts (Jensen (1986)) or because many acquisitions are financed with debt (Palepu (1986); Karpoff, Schonlau, and Wehrly (2017)). Similarly, firms with lower levels of ROA and Tobin's Q or those with higher property ratios may make attractive acquisition targets because they have more assets in place which can be used to collateralize acquisition loans. To the extent that they are indicative of financial distress, lower levels of sales growth, ROA, and Tobin's Q may attract potential acquirers who hope to profit from making operational improvements to acquired firms.

Song and Walkling (2000) find that CARs are greater for peer firms that have increased probability of acquisition. I control for peer firm CARs in the [-2, +2] day window around primary firm announcement dates. Drake, Roulstone, and Thornock (2015) use the EDGAR log files to examine the determinants and consequences of information acquisition via EDGAR. They find that investor demand for information is negatively related to abnormal stock performance. Because I am interested in examining the effect of investor attention to peer firms originating from primary firm announcements rather than from peer firm performance, I include controls for one-year BHARs for peer firms over the fiscal year ending before the primary firms' announcement date.

In tests of whether investors pay more attention to informationally opaque firms after the announcement that other firms in the industry have been targeted for acquisition, I consider four opacity measures that are standard in the literature. The first is peers' bid ask spreads (*Spreads*), which compensate traders for adverse selection risk associated with asymmetric information between buyers and sellers (Glosten and Milgrom (1985)). I calculate each peer's daily spread as the difference in the bid and the ask price in CRSP, which is scaled by the midpoint. I calculate *Spreads* as each firm's average bid-ask spread over the one year prior to the announcement of the primary firm's takeover bid.

In a sample of firms spanning 34 countries, Li, Chen, An, and Murong (2017) find that positive announcement-period returns to international industry peers of firms targeted for acquisition predict that the peers will be targeted, and that this result is stronger for peers that are followed by analysts. Their results provide evidence that the information that analysts gather can help predict merger activity. In order to examine whether analyst following impacts attention to peers around the time that other firms in the industry have been targeted for acquisition, I construct a measure of the number of analysts (*Analysts*) that follow a peer as of the forecast nearest but prior to the primary firm's announcement date.

A number of studies demonstrate that managers can use discretionary accruals in order to elicit personal benefits at shareholder expense ((Healy (1985); Armstrong, Jagolinzer, and Larker (2010)). Because one function of takeover is to replace managers who purse their own self-interest at shareholders' expense (Malatesta (1983)), the third opacity measure that I consider is the absolute value of discretionary accruals in the year prior to announcement (*Accruals*), which is estimated following the method proposed by Kothari, Leone, and Wasley (2005). As information asymmetry is decreasing in firm size (Diamond and Verrecchia (1991)), the final opacity measure that I consider is firm size *(Size*).

In tests of whether investor attention affects the probability or hazard rate of deal completion, I control for bidding firms' public status because mergers cluster in industries with publicly-traded firms and not as much with privately traded firms (Netter, Stegemoller, and Wintoki (2011); Maksimovic, Phillips, and Yang (2013)). I include controls for the method of payment because stock payments can signal uncertainty about a target firm's value (Hansen (1987)), while cash payments can signal bidders' confidence in their estimation of a target's value because all cash bidders risk absorbing the losses arising from overpayment (Fishman (1989)). Providing support for this view, Huang, Officer, and Powell (2016) find that the use of stock to finance deals is associated with lower rates of deal completion compared to cash and mixed deals. Finally, I control for the percentage of acquired firms' shares tendered because this impacts the hazard rate of deal completion. Relative to acquirers in mergers, acquirers in tender offers conduct longer due diligence in the pre- announcement period and shorter due diligence in the post-announcement period (Offenberg and Pirinsky (2015)).

Panel B of Table 1 reports descriptive statistics for the main sample. All continuous variables are winsorized at their 1st and 99th percentiles. Of the 113,284 primary-peer firm combinations, 1.7% (4.81%) involve peer firms that are targeted for takeover in the 30 (90) days following primary firm announcements. The mean value of $\Delta Views$ is 6.72, which implies that on average, there are over six more views of peer firm documents per day in the period after primary firm announcements relative to the average number of daily views in the period before.

3. Empirical Results

3.1. Attention to Peer Firms' Fundamental Information around Primary Announcements

To examine whether investors tend to pay more attention to peer firms after primary firms' deals are announced, I estimate the following OLS regression:

$$VIEWS_{i,t,j} = \sum_{-10}^{+9} \alpha_i Date_{t,j} + \beta_2 Day_t + \beta_3 News\ Day_{i,t,j} + \beta_4 Dividend\ Day_{i,t,j}$$

$$+ \beta_5 Earnings\ Announcement\ Day_{i,t,j} + \tau_{i,j} + \epsilon_{i,t,j}\ , \qquad (1)$$

where $Views_{i,t,j}$ is the sum of views of peer firm *i*'s SEC filings on day *t* of the [-10, +9] calendar day period around the announcement that primary firm *j* is targeted for takeover. The main variables of interest, $Date_{t,j}$, are indicator variables for each of the days, *t*, in the [-10, +9] day period around primary firm *j*'s announcement date. While $Date_{-10}$ is included in the sample, it is excluded from the results because the coefficients of $Date_{-9,+9}$ are measured relative to $Date_{-10}$. Thus, all coefficients on $Date_{-9,+9}$ are interpreted relative to $Date_{-10}$. Day_t is a dummy variable for each day of the week that I include to control for the possibility that investor attention could be systematically higher on certain days (e.g., Mondays) compared to other days (e.g., Saturdays). I also control for the potential that increases in attention could be driven by firm-specific events by including $News\ Day_{i,t,j}$, an indicator variable equal to one if a news article is reported in Ravenpack for peer firm *i* on day *t* and 0 otherwise, $Dividend\ Day_{i,t,j}$, an indicator variable that equals one if peer firm *i* declares dividends on day *t* and zero otherwise, and $Earning\ Announcement\ Day_{i,t,j}$, an indicator variable that equals one if peer firm *i* announces earnings on day *t* and zero otherwise.

In these tests, I also include primary-peer firm fixed effects $\tau_{i,j}$ to control for any variable or factor that varies at the peer-primary-deal level that could be correlated with investor attention around the primary firm announcement. For instance, primary-peer firm fixed effects control for both peer and primary firm-specific characteristics, like primary and peer firm size and peer firm market-to-book ratios that have been shown to affect the probability that firms are targeted for a takeover (Karpoff, Schonlau, and Wehrly (2017)), as well as time and industry varying trends in acquisition activity (Mitchell and Mulherin (1996); Harford (2005)). Primary-peer firm fixed effects also control for the effects of BHARs before announcements and CARs around announcements on investor attention. In short, primary-peer firm fixed effects that I use in these models subsume all peer and primary firm characteristics that are time-invariant over the [-10, +9] days around the deal. In addition, I cluster standard errors at the primary firm deal level to account for the concern that residuals are serially correlated across peer firms for each particular deal.

Panel A of Table 3 presents results of these regressions. The results in Column 1 show a positive but statistically insignificant increase in average attention to all sample peer firms on primary firm announcement dates ($Date_{t=0}$), which indicates investors do not tend to pay increased attention to *all* peer firms around primary firm announcements. In Column 2 (3), I restrict the sample to firms that are targeted within 30 (90) days of announcement. The results show that primary firm announcements are associated with investors paying increased attention to the set of industry peer firms that will be targeted for takeover shortly thereafter. The positive and statistically significant coefficient estimate of 1.28 on $Date_{t=0}$ in Column 2 implies that for the sample of peer firms that have an announcement within 30 days of primary firms' announcement dates, average attention increases by 1.28 views of peer firms' documents on primary firms' announcement dates. This is a 10.8% increase relative to the 12.31 average views on $Date_{t-10}$ reported in Panel B of Table 2. In addition, the estimated coefficient on $Date_t$ of 1.79 in Column 2 indicates that for these firms, the average number of peer firm views increases by about 14.5% on the day after initial announcements. The results in Column 3 are similar to those in Column 2. The estimated coefficient on $Date_{t=0}$ of 0.55 indicates that for peer firms that are targeted within 90 days of primary firm announcements, the number of views increases by about 4.7% on the day after primary firm announcements, relative to the 11.61 average views on $Date_{t-10}$ reported in Panel C of Table 2. The estimated coefficient on $Date_{t=1}$ is 0.48 which implies that these firms experience a 4.1% increase in attention on the second day after primary firm announcements relative to the average views on $Date_{t-10}$. In sum, the coefficients on the post-announcement *Date* variables reported in Column 2 and Column 3 indicate that on initial announcement dates and over most of the nine days thereafter, investor attention to peer firms that are subsequently acquired increases significantly.

In an additional test of whether investor attention to peer firms increases after primary firm announcements, I construct an indicator variable *POST* which equals one if $Date_{t,j}$ falls on primary firm *j*'s announcement or in the nine days thereafter and zero if $Date_{t,j}$ falls in the 10 day period before *j*'s announcement date. I estimate the following OLS regressions on the same three samples of firms described above:

$$VIEWS_{i,t,j} = \alpha_1 POST_{t,j} + \beta_2 Day_t + \beta_3 News\ Day_{i,t,j} + \beta_4 Dividend\ Day_{i,t,j}$$

$$+ \beta_5 Earnings\ Announcement\ Day_{i,t,j} + \tau_{i,j} + \epsilon_{i,t,j}. \quad (2)$$

I present results of this analysis in Panel B of Table 3. The estimated coefficient on $POST_{t,j}$ in Column 2 of Panel B is 1.99 and statistically significant at the 1% level. This implies that for firms that are acquired within 30 days of primary firm announcements, investor attention increases by 1.9 views per day in the period after primary firm announcements relative to the prior period. This is a 15.4% (=1.9/12.31) increase in views relative to the mean number of views over $Days_{t-10,t-1}$ reported in Panel B of Table 2. For firms that are targeted for acquisition within 90 days of primary firm announcements, average investor attention increases by 0.69 views per day in the period after primary announcements, a 5.8% (=0.69/11.9) increase relative to the number of views in the pre-announcement period.

3.2. Information Asymmetry Cross-Sectional Analyses

The results of the previous set of analyses demonstrate that investor attention to industry peers increases after the announcement that other firms in the industry have been targeted for merger or acquisition. In my next set of tests, I examine whether these increases in attention are focused on the most informationally opaque peer firms. As this is the first study to my knowledge that examines the impact of investor attention on M&A outcomes, I have little theory to rely upon. On one hand, it is plausible that after the announcement that a firm has been targeted for acquisition, other potential acquirers focus their attention on informationally opaque peers of the targeted firm in order to estimate whether valuation discrepancies or potential gains from transparency enhancements exist. Potential acquirers may have good reason for focusing attention on the most opaque peers. Using share turnover as a proxy for discrepancies in valuation, previous studies find that valuation discrepancy predicts increased incidence of merger (Gort (1969)) and probability of being targeted for merger or acquisition (Cornett, Tanyeri, and Tehranian (2011)). Officer, Poulsen, and Stegemoller (2009) find that among a sample of acquirers bidding on targets in stock-for stock transactions, those that bid on difficult-to-value targets earn significantly higher returns. On the other hand, it is also plausible that observed increases in attention derive from investors or analysts who focus information acquisition efforts on the least opaque of a targeted firm's industry peers in order to quickly and efficiently estimate how the potential merger will change the industry's competitive landscape. Drake, Roulstone, and Thornock (2011) demonstrate firms' information environments are positively associated with investors' attention to their SEC filings in EDGAR. They find that the number of documents viewed in EDGAR is greater for larger firms and firms with greater analyst following.

In order to examine whether observed increases in attention to peer firms' fundamental information varies with respect to their information environments, I construct four commonly used measures of information asymmetry. The first measure of information asymmetry that I consider is bid-ask spreads (*Spreads*), which have been shown to be larger for more informationally opaque firms (Glosten and Milgrom (1985)). Second, as merger announcements are likely to garner attention from analysts, I examine the extent to which attention to peers of firms targeted in M&A varies with respect to analyst following. Drake, Roulstone, and Thornock (2011) demonstrate that the acquisition of fundamental information via EDGAR is increasing in analyst following. The second opacity measure that I consider is analyst following (*Analysts*). The third measure that I consider is discretionary accruals (*Accruals*). Higher levels of discretionary accruals could signal the existence of agency problems, the mitigation of which by means of takeover could improve firm value (Jensen (1986)). The last measure of opacity that I consider is firm size. Diamond and Verrechia (1991) argue that information is incorporated into the prices of larger firms' stock more quickly. I consider high bid ask spreads, low analyst following, high discretionary accruals, and small firm size to be proxies for higher informational opacity.

In these tests, I calculate each peer firm's *Spreads*, *Analysts*, *Accruals*, and *Size*, and I sort them into terciles accordingly. I create dummy variables that are equal to one if a firm is sorted into a high opacity tercile, and zero otherwise. Because opacity is increasing in bid-ask spreads and accruals, firms in the highest (lowest) *Spreads* and *Accruals* terciles are those in the highest (lowest) opacity groups. As opacity is decreasing in firm size and analyst following, firms in the lowest (highest) *Size* and *Analysts* terciles are sorted into the highest (lowest) opacity groups. I interact the high opacity dummy variables with POST, which equals one if $Date_{t,j}$ falls on primary firm *j*'s announcement or in the nine days thereafter and zero if $Date_{t,j}$ falls on *j*'s in the 10 day period before *j*'s announcement date. In order to test whether attention to firms' fundamental information after the announcement that other firms in the industry have been targeted for M&A varies with respect to opacity, I estimate the following OLS regression:

$$VIEWS_{i,t,j} = \beta_1 POST_{t,j} + \beta_2 POST_{t,j} * High\ Opacity_{i,t,j} + \beta_3 POST_{t,j} * Medium\ Opacity_{i,t,j}$$

$$+\beta_4 High\ Opacity_{i,t,j} + \beta_5 Medium\ Opacity_{i,t,j} + \beta_6 Day_t + \beta_8 Dividend\ Day_{i,t,j}$$

$$+ \beta_9 Earnings\ Announcement\ Day_{i,t,j} + \tau_{i,j} + \epsilon_{i,t,j}, \quad (3)$$

where standard errors are clustered at the primary firm deal level. I present the results of these tests in Table 4. As in the analyses presented in Table 2, I conduct these tests on three samples. The first sample consists of every primary peer firm combination. I present the results of analyses that consider the full sample of firms in Column 1 of each panel. The second (third) samples consist of peer firms that are targeted in 30 (90) days, and I present the results of tests that consider these samples in Column 2 (3) of each panel.

Generally, the results provide evidence that investors tend to pay less attention to the fundamental information of highly opaque firms in the post-announcement period than they do to firms that are less informationally opaque. This effect seems to be stronger for peers that receive a takeover bid in the following 30 or 90 days. I report results of tests that investigate whether bid-ask spreads impact attention to sample firms in Panel A. The estimated coefficient on *High Spreads * POST* in Column 1 of Panel A is -0.595 and statistically significant at the 1% confidence level. In the full sample, relative to the peer firms in the lowest *Spread* tercile, peer firms with the highest spreads experience and average of 0.595 fewer views per day in the post-announcement period. The estimated coefficient on *High Spreads * POST* in Column 2 of Panel A is -1.386 and statistically significant at the 1% confidence level. In the sample of firms that are targeted in 30 days of a primary firm announcement, relative to the peer firms in the lowest *Spread* tercile, peer firms with the highest spreads experience an average of 1.386 fewer views per day in the post-announcement period.

I report results of tests that investigate whether analyst following impacts attention to sample firms in Panel B of Table 4. The estimated coefficient on *Low Analysts * POST* in Column 1 of Panel B is -0.62 and statistically significant at the 1% confidence level. In the full sample, relative to the peer firms in the highest *Analysts* tercile, peer firms with the lowest analyst following experience and average of 0.62 fewer views per day in the post-announcement period. The estimated coefficient on *Low Analysts * POST* in Column 2 of Panel B is -1.471 and statistically significant at the 1% confidence level. In the sample of firms that are targeted in 30 days of a primary firm announcement, relative to the peer firms in the highest *Analysts* tercile, peer firms with the lowest analyst following experience an average of 1.471 fewer views per day in the post-announcement period.

I report results of tests that investigate whether discretionary accruals impact attention to sample firms in Panel C of Table 4. In each column, the estimated coefficients on *High Accruals * POST* is insignificantly different from zero, which indicates that investor attention to peer firms around primary firm announcements does not vary significantly with respect to peer firms' discretionary accruals. I report results of tests that investigate whether size impacts attention to sample firms in Panel D. The estimated coefficient on *Small Size * POST* in Column 1 of Panel D is -0.638 and statistically significant at the 1% confidence level. In the full sample, relative to the largest peer firms, the smallest peer firms experience an average of 0.638 fewer views per day in the post-announcement period. The estimated coefficient on *Small Size * POST* in Column 2 of Panel D is -1.104 and statistically significant at the 1% confidence level. In the sample of firms that are targeted within 30 days of a primary firm announcement, relative to the largest peer firms, the smallest peer firms experience an average of 1.104 fewer views per day in the post-announcement period. Overall, these results in Table 4 demonstrate that in the post-announcement period, investor attention is decreasing in informational opacity, and that this effect seems to be stronger for informationally opaque peers that are targeted in the 30 or 90 days of a primary firm announcement.

3.3. Predicting Takeover Targets

While the prior results are suggestive, in my second analysis, I more formally test whether increased attention to industry peer firms predicts that they will be subsequently targeted. To examine whether increases in peer firm attention around primary firm announcement dates predict that peer firms will subsequently be targeted, I regress indicator variables that are set to one if a peer firm is targeted in the 30 or 90 days after primary firm announcements on the change in views around the announcement ($\Delta Views$) and peer firm controls.

In this analysis, I estimate both linear probability models and logistic regressions that include primary firm deal fixed effects that account for time and primary firm-specific characteristics and cluster standard errors at the primary firm deal level. My prediction models are of the form:

$$\begin{aligned} Targeted\ 30 = {} & \beta_1 \Delta Views_{i,t,j} + \beta_2 CARs_{i,t,j} + \mathrm{B}_3 Size_{i,t,j} + \beta_4 Leverage_{i,t,j} + \beta_5 TQ_{i,t,j} \\ & + \beta_6 Property\ Ratio_{i,t,j} + \beta_7\ Liquidity\ Ratio_{i,t,j} + \beta_8\ Sales\ Growth_{i,t,j} \\ & + \beta_9 ROA_{i,t,j} + \beta_{10} BHARs_{i,t,j} + \beta_{11}\ Industry\ Concentration_{i,t,j} \\ & + \gamma_{j,t} + \epsilon_{i,t,j}, \end{aligned} \tag{4}$$

where $\gamma_{j,t}$ are primary deal firm fixed effects, and all control variables are constructed as defined in the appendix and standardized to have a mean of zero and a standard deviation of one.

I present the results of these regressions in Table 5.[6] Columns 1 and 2 report the results from the linear probability model and show that larger increases in views are associated with a higher likelihood of a takeover offer. The estimated coefficient on *ΔViews* in Column 1 indicates that a one

[6] In order to mitigate the concern that multicollinearity might bias my results, I estimate the models in Table 4 without *Size*, which I find to be most strongly correlated with the other covariates, and again without any controls at all. In each of these robustness tests, the estimated coefficient on ΔViews is significant at the .01 level and larger in magnitude than those presented in Table .

standard deviation increase in the number of views around the primary firm announcement is associated with a 0.782 percentage point increase in the probability that a peer firm will be targeted within 30 days of primary firm announcements. Relative to the 1.68% of sample firms that are targeted within 30 days, this 0.782 percentage point increase represents a relative increase in the likelihood that a firm is targeted of 46.5%. The coefficient on *ΔViews* in Column 2 indicates that a one standard deviation increase in *ΔViews* is associated with a 0.839 percentage point increase in the probability that firms will be targeted for takeover in the following 90 days. Relative to the 4.73% of sample firms that are targeted within 90 days, this 0.839 percentage point increase represents a relative increase in the likelihood that a firm is targeted of 17.7%. Overall, the results presented in Table 5 demonstrate that increases in investor attention to industry peer firms around the announcement that other firms have been targeted for acquisition significantly predict that peers will subsequently receive a bid.[7]

3.4. Firm Performance and Announcement Period Return Cross-Sectional Analyses

Song and Walkling (2000) demonstrate that peer firm CARs around primary firm announcements are greater for peers with increased probability of acquisition. The positive and statistically significant coefficients on *CARs* reported Table in 5 are consistent with their findings. The estimated coefficient on *CARs* in Column 1 is 0.268, which implies that a one standard deviation increase in CARs is associated with a 0.268 percentage point increase in the probability that a firm is targeted within 30 days. Importantly, the estimated coefficient on *ΔViews* in Column 1 of Table 5 is 2.9 times larger in magnitude than the estimated coefficient on CARs, which indicates that the ability of

[7] In robustness tests, I perform my main analyses after eliminating observations in which peer firms are targeted within 10 days of primary firm announcements. In additional robustness tests, I measure ΔViews as Views[-3, -1] - Views[0, +2] and drop from the sample all observations in which a peer is targeted in days [0, 3]. The results of these robustness tests are qualitatively similar to those presented in Table 5.

peer attention around primary firm announcements to predict peer takeovers is both incremental and economically more significant than that of peer CARs.

The results of the logistic regressions in Columns 3 and 4 of Table 4 are similar. The positive and statistically significant coefficients of 0.358 and 0.161 on *ΔViews* in Columns 3 and 4 translate into marginal effects of 0.54% and 0.64%, respectively. Thus, based on the estimates in Columns 3 and 4, a one standard deviation increase in the change in views is associated with relative increases in the probability of being acquired in the next 30 or 90 days of 32.1% (=0.54/1.68) and 13.5% (=0.64/4.73), respectively.

Song and Walkling's (2000) Acquisition Probability Hypothesis asserts that peers earn abnormal returns because of the increased probability that they will become targets themselves. To test whether information acquisition is a better predictor of future bids in peer firms for which high announcement period returns provide a confirming signal about the probability of receiving a bid, I sort peer firms into primary firm announcement-level CAR terciles. *Middle (High) CAR* is equal to 1 if a peer firm's [-2, +2] day CARs are in the middle (upper) tercile. Table 6 presents results of OLS and logistic regressions of the probability that a peer will be targeted on *ΔViews* and interactions with CAR tercile dummies. Regression results indicate that attention to fundamental information is a better predictor of which firms will be targeted in peer firms for which high announcement period CARs provide a confirming signal about the probability of receiving a bid. The estimated coefficient on *ΔViews* * *High CAR* in Column 1 is 0.43 and statistically significant. This means that relative to peer firms in the lowest primary firm CAR tercile, being in the highest primary firm level CAR tercile in conjunction with a one standard deviation increase in *ΔViews* is associated with an additional 0.43% increase in the probability of receiving a bid in the following 30 days, which is a 25.6% (=0.43/1.68) increase relative to the mean.

In order to test whether the link between fundamental analysis and future M&A activity is stronger for underperforming firms, I sort peer firms into primary firm announcement-level *Tobin's Q* and *BHAR* terciles. *Low (Middle) TQ* is equal to 1 if a peer firm's *Tobin's Q* is in the lower (middle) tercile, and *Low (Middle) BHAR* is equal to 1 if its *BHAR* is in the lower (middle) tercile. Table 7 presents results of OLS and logistic regressions of the probability that a peer will be targeted on *ΔViews* and interactions with *Tobin's Q* tercile dummies and Table 8 presents results of similar OLS and logistic regressions in which *ΔViews* is interacted with BHAR tercile dummies. The results of both sets of tests are similar and suggest that attention to fundamental information is a better predictor of which firms will receive M&A bids when attention is paid to underperforming firms. In Column 1 of Table 7, the coefficient on *ΔViews* * *Low TQ* is equal to 0.302, with a t-statistic of 1.97. Taken together with the sample average of 1.68% of peers that are targeted in 30 days, this implies that relative to firms in the highest Tobin's Q tercile, being in the lowest Tobin's Q tercile in conjunction with a one standard deviation increase in *ΔViews* increases the probability that a firm will be targeted for takeover in the following 30 days by and additional 0.302 percentage points, which is an 17.9% (=0.302/1.68) increase relative to the mean.

In Column 1 of Table 8, the coefficient on *ΔViews* * *Low BHAR* is equal to 0.373. Taken together with the sample average of 1.68% of peers that are targeted in 30 days, this implies that relative to firms in the highest BHAR tercile, being in the lowest BHAR tercile in conjunction with a one standard deviation increase in *ΔViews* increases the probability that a firm will be targeted for takeover in the following 30 days by 0.373 percentage points, which is a 22.2% (=0.373/1.68) increase relative to the mean. In sum, the results presented in Tables 7 and 8 indicate that fundamental information gathering has a much stronger impact on the probability that firms will receive future bids for those firms with the worst performance in the runup period.

3.5. Predicting Takeover Completions

In my final set of analyses, I examine the relation between attention to fundamental information and the likelihood and timing of deal completions. I create a subsample of deals in which: (1) either bidders seek to own 100% of peer firms' shares and the deal is ultimately withdrawn, or (2) bidders own 100% of peer firm shares after the deal is completed. In these tests, my primary variable of interest is a measure of abnormal views (*ABViews*), which equals the average number of views of a firm's documents in the [-10, +9] days around its announcement date scaled by average number of views of its documents over days [-180, -11].[8] Table 10 presents descriptive statistics for the subsample of firms that I use in these tests. Of the 1,240 announced deals in this subsample, 84.2% or 1,044 deals culminate in acquisition and 196 are withdrawn. On average, the number of searches in the [-10, +9] days around firms' announcement dates is over 1.8 times greater than the average over days [-180, -11]. To examine whether increased attention around announcements predicts deal completion, I estimate the following logistic regression:

$$\begin{aligned}
Completed = {} & \beta_1 ABViews_{j,t} + \beta_2 CARs_{j,t} + \beta_3 Size_{j,t} + \beta_4 Leverage_{j,t} \\
& + \beta_5 TQ_{j,t} + \beta_6 Property\ Ratio_{j,t} + \beta_7\ Liquidity\ Ratio_{j,t} \\
& + \beta_8\ Sales\ Growth_{j,t} + \beta_9 ROA_{j,t} + \beta_{10} BHARs_{j,t} \\
& + \beta_{11}\ Industry\ Concentration_{j,t} + \beta_{12} Public\ Bidder_{j,t} + \beta_{13} Stock\ Deal_{j,t} \\
& + \beta_{14} All\ Cash\ Deal_{j,t} + \beta_{15} Percentage\ Tendered_{j,t} + \theta_t + P_j + \epsilon_{j,t}, \qquad (5)
\end{aligned}$$

[8] For robustness, I repeat this set of analyses using a measure of *ABViews* in which abnormal views are measured over days [-6, +7] relative to days [-180, -8] to account for potential day of the week effects on investor attention. The results of these analyses are qualitatively similar to those reported in Table 10.

where *Completed* is an indicator variable that equals one if the deal is completed, and zero if the deal is withdrawn, θ_t are year×month fixed effects, and P_j are 3-digit SIC industry fixed effects. In these tests, I cluster standard errors at the year×month level to account for the concern that residuals are serially correlated across firms with merger announcements in the same month. All control variables are constructed as defined in the appendix and standardized to have a mean of zero and a standard deviation of one. In addition to the control variables used in my prior tests, I also include a number of firm and merger deal-specific characteristics. *CARs* and *BHARs* are the [-2, +2] and one year buy and hold abnormal returns calculated relative to each firm *j*'s own announcement date. *Public Bidder* is an indicator variable equal to one if the common stock of the acquirer is publicly traded and zero otherwise. *Stock Deal* is an indicator variable that equals one if the acquirer uses a portion of its stock to pay for the deal and zero otherwise. *All Cash Deal* is an indicator variable equal to one if *j*'s acquirer pays for the acquisition entirely with cash. Finally, *Percentage Tendered* is the percentage of firm *j*'s shares tendered in the deal.

Overall, the results in Column 1 of Table 10 show that increases in abnormal views are associated with a greater likelihood that an announced merger is eventually completed. In terms of economic significance, the coefficient on *ABViews* is 0.773, which translates into a marginal effect of 0.0878. Thus, the estimate implies that a one standard deviation increase in abnormal views is associated with a 8.78 percentage point increase in the marginal probability of deal completion and a 10.4% (=8.78/84.2) increase in the relative probability of deal completion. The positive and statistically significant coefficient estimated on *CAR* is also consistent with Song and Walkling (2000), who demonstrate that announcement period CARs are greater for those firms with higher takeover likelihood. Moreover, in terms of economic significance, the effect of *CAR* on *Completion* is smaller than *ABViews*. Thus, the effect of abnormal views is not only incremental to effect of abnormal returns, but also economically more significant.

In Column 2, I examine whether abnormal investor attention affects the timing of deal completion relative to deal announcement using a Cox proportional hazard model. A hazard model like the Cox model accounts for both the occurrence and timing of deal completion. In this setting, a failure event represents a deal completion. If an announced merger is not completed before the end of my sample period, this observation would be considered a censored observation. For these censored observations, I set the censoring time to 643 days, which is the maximum number of days between announcement and effective dates for firms that are eventually taken over. The hazard function thus provides the probability of takeover on a particular day conditional on the fact that the firm has survived up to that day. The control variables are the same as those used in Column 1. Overall, the results in Column 2 suggest that investor attention affects the timing of deal completion. The estimated coefficient on *ABViews* of 0.529 is associated with a hazard ratio of 1.697, which indicates that as attention increases by one standard deviation, the hazard rate of completion increases by 69.7%.[9]

4. Conclusion

In this paper, I study whether investors' attention to the fundamental information of industry peers of firms that are targeted in M&A predicts that these peers will subsequently receive M&A bids. To capture attention to fundamental information, I use the number of times each sample firms' SEC filings are downloaded from the EDGAR website. I document six key results. First, around

[9] To account for potential confounding day of the week effects on investor attention, I replace *ABViews* with *ABViews7*, the average number of views of a firm's documents in over days [-6, 7] around its announcement scaled by the average number of its views over days [-180, -7]. I then re-estimate the models in Table 10 and find that the coefficient on *ABViews7* in the logit (hazard) model 0.996 (0.559) with a t-statistic of 4.84 (8.63).

announcements that firms are targeted in M&A, there is a significant increase in attention to the fundamental information of industry peers that subsequently receive M&A bids. Second, peer firms with the largest increases in attention to fundamentals are significantly more likely to be acquired in the next 30 or 90 days. Third, informationally opaque firms receive less investor attention than transparent firms in the post-announcement period. Fourth, market participants who gather firms' fundamental information after the announcement that other firms in the industry have been targeted for M&A correctly use the information to predict future M&A bids. Fifth, investors' attention to fundamental information is a stronger predictor of which firms will be targeted for acquisition in underperforming peers. Finally, for firms that are targeted for acquisition, those firms with the largest increase in attention to fundamental information around the M&A announcement are more likely to be acquired and the amount of time between the announcement and completion of their deals decreases.

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Appendix 1
Variable Definitions

Variable	Definition
ABViews	The average number of views of firm j's documents in the [-10, +9] days around its announcement date scaled by average number of views of its documents over days [-180, -11].
Accruals	The absolute value of discretionary accruals in the year prior to announcement, estimated following the method proposed by Kothari, Leone, and Wasley (2005).
All Cash	An indicator variable equal to one if an acquirer purchases a target using only cash, and zero otherwise.
Analysts	Number of analysts following peer firms as of forecast nearest but before primary firm announcement date.
BHARs	Buy and hold abnormal returns. For peer firms, BHARs are the one year buy and hold abnormal returns to peer firms over the fiscal year before primary firms' announcement date. For primary firms, BHARs are one year and hold abnormal returns calculated over the one-year period ending on the firm's M&A announcement date.
CARs	Cumulative abnormal returns. For peer firms, CARs are calculated in the [-2, 2] day period around primary firm announcement date. For primary firms, CARs are calculated in the [-2, 2] day period around their announcement date.
Completed	An indicator variable equal to 1 if a peer firm is acquired within the sample period, 0 otherwise.
Day	Indicator variables for day of the week.
Dividend Day	An indicator variable equal to 1 if a peer firm announces a dividend distribution on day t, 0 otherwise.
Earnings Announcement Day	An indicator variable equal to 1 if a peer firm announces earnings on day t, 0 otherwise.
Industry Concentration	A Herfindahl-Hirschman index based on 3-digit SIC industry sales.
Leverage	Book value of debt in current liabilities + long-term debt scaled by book value of assets ((dlc+dltt)/at).
Liquidity Ratio	Cash and short-term investments scaled by assets (che/at).
News Day	An indicator variable equal to 1 if Ravenpack reports a news article for a peer firm on day t, 0 otherwise.
Percentage Tendered	The percentage of target firm j's commons shares tendered.
POST	An indicator variable equal to 1 if $Date_t$ falls in the 10-day period before primary firm j's announcement date and 0 if $Date_t$ falls on primary firm j's announcement date or the nine days thereafter.
Property Ratio	Total value of property, plant and equipment scaled by assets (ppent/at).

Appendix 1, Continued
Variable Definitions

Variable	Definition
Public Bidder	An indicator variable equal to one if a bidder's common stock is publicly traded, and zero otherwise.
ROA	Return on assets. Operating income after depreciation scaled by assets. (oiadp/at)
Sales Growth	Year over year growth in net sales.
Size	Log of assets reported
Spreads	Daily bid-ask spreads are the difference in the ask and bid prices scaled by the midpoint. Spreads are the average bid-ask spread over the year prior to announcement.
Targeted 30 (90)	An indicator variable equal to 1 if a peer firm i is targeted within 30 (90) days of primary firm j's announcement date, 0 otherwise.
TQ	Tobin's Q is the market value of assets scaled by the book value of assets ((at-ceq+prcc_f * chso)/at)).
Views	The average number of views of peer firm i's documents during the 10-day period beginning on primary firm j's announcement date subtracted by the average number of views of i's documents during the prior 10-day period.
ΔViews	The average number of views of peer firm i's documents during the 10-day period beginning on primary firm j's announcement date subtracted by the average number of views of i's documents during the prior 10-day period.

Table 1
Descriptive Statistics

Deals include all US targets not operating in finance or utility industries in which the bidder holds less than 50% of the target's shares before announcement and owns 100% of the target's shares after the deal is effective. Panel A presents the number of primary firm deals by year. Panel B presents descriptive statistics for the main sample of 110,086 primary-peer firm combinations. All variables are defined in the appendix. All continuous variables are winsorized at their 1st and 99th percentiles.

Panel A: Number of primary firm deals by year.

Year	Number of Deals
2007	167
2008	102
2009	93
2010	133
2011	113
2012	106
2013	85
2014	89
2015	99
2016	98
Total	1,085

Panel B: Main sample of 110,086 primary-peer firm combinations.

Variable	Mean	Std. Dev.	P25	P50	P75
Targeted (30) (%)	1.68				
Targeted (90) (%)	4.73				
ΔViews	6.386	69.349	-16.000	2.000	23
CARs (%)	0.167	6.571	-2.906	-0.043	2.947
BHAR (%)	-2.848	54.999	-36.060	-9.453	18.333
Firm Size	5.994	2.034	4.548	5.802	7.321
Leverage	0.152	0.199	0.000	0.064	0.248
Tobin's Q	2.553	1.916	1.323	1.928	3.082
Property Ratio	0.138	0.178	0.033	0.073	0.158
Liquidity Ratio	0.345	0.255	0.132	0.294	0.519
Sales Growth	0.233	0.773	-0.028	0.101	0.274
ROA	-0.040	0.284	-0.074	0.044	0.106
Industry Concentration	0.061	0.038	0.041	0.054	0.059
Analysts	8.883	7.708	3	6	12
Accruals	0.109	0.135	0.029	0.068	0.131
Spreads	0.007	0.013	0.001	0.002	0.007

Table 2
Average Number of Views of Peer Firm Documents Around Primary Firms' Announcement Dates

This table presents descriptive statistics for the number of views of peer firm documents viewed (*Views*) on each of the [-10, +9] days around primary firms' announcement dates ($Date_{t=0}$). Panel A presents descriptive statistics for the full sample. Panel B (C) presents data for the subsample of peer firms that are targeted for acquisition in the 30 (90) days after primary firms' announcement dates.

	Panel A: Full Sample				Panel B: Firms targeted within 30 days				Panel C: Firms targeted within 90 days			
Date	N	Mean	P50	Std. Dev.	N	Mean	P50	Std. Dev.	N	Mean	P50	Std. Dev.
-10	110,086	9.99	5	16.27	1,849	11.83	6	18.36	5,207	11.61	6	17.70
-9	110,086	8.39	4	15.51	1,849	10.18	5	17.61	5,207	9.95	5	17.08
-8	110,086	9.48	4	16.43	1,849	11.69	6	18.48	5,207	11.39	5	18.30
-7	110,086	12.05	6	18.38	1,849	14.77	8	19.94	5,207	14.37	8	20.03
-6	110,086	11.65	6	17.92	1,849	14.56	8	20.39	5,207	14.15	7	20.33
-5	110,086	10.54	5	17.33	1,849	12.89	6	19.71	5,207	12.50	6	18.97
-4	110,086	10.04	5	16.85	1,849	12.41	6	19.12	5,207	11.92	6	18.67
-3	110,086	9.56	4	16.39	1,849	12.05	6	18.75	5,207	11.60	6	18.25
-2	110,086	8.38	3	15.64	1,849	10.54	5	17.75	5,207	10.07	4	17.22
-1	110,086	9.43	4	16.56	1,849	12.15	6	18.85	5,207	11.44	5	18.37
0	110,086	12.23	6	18.92	1,849	16.27	9	22.85	5,207	14.89	8	21.46
1	110,086	11.70	6	18.24	1,849	16.07	8	22.99	5,207	14.30	7	21.11
2	110,086	10.58	5	17.61	1,849	15.00	7	22.54	5,207	13.15	6	20.47
3	110,086	9.95	5	16.97	1,849	14.53	7	21.67	5,207	12.41	6	19.70
4	110,086	9.20	4	15.66	1,849	13.14	7	19.45	5,207	11.76	6	18.40
5	110,086	8.44	3	16.09	1,849	12.39	5	20.45	5,207	10.70	5	18.36
6	110,086	9.66	4	17.14	1,849	14.82	7	22.82	5,207	12.63	6	20.25
7	110,086	11.71	6	18.08	1,849	18.08	10	23.77	5,207	15.37	8	21.60
8	110,086	11.67	6	18.36	1,849	17.03	9	22.59	5,207	14.99	8	21.61
9	110,086	10.34	5	17.16	1,849	15.26	8	21.82	5,207	13.14	6	19.90

Table 3
Announcement Period Attention to Peer Firms' Fundamental Information

Panel A presents results of OLS regressions of daily Views on indicator variables for the each of the [-10, +9] days around primary firm announcement dates. The coefficients are measured relative to Date-10. In Panel B, POST is equal to 1 if Datet,j is in [0, +9], and it is equal to 0 if Datet,j is in [-10, -1]. Column 1 presents regression results for the full sample. In Column 2 (3), the sample is restricted to peer firms that are targeted for acquisition in the 30 (90) days after primary firm announcement. All models include primary-peer fixed effects and standard errors are clustered at the primary firm deal level. Controls include indicators for days of the week, Ravenpack news days, earnings announcement days and dividend declaration days. All variables are defined in the appendix. All continuous variables are winsorized at their 1st and 99th percentiles. t-statistics calculated by standard errors clustered at the primary firm deal level are presented in parentheses.

Panel A: Individual Days Around Announcement

	Dependent Variable = Views					
	(1)		(2)		(3)	
DATE	Full Sample		Targeted (30)		Targeted (90)	
-9	-0.38***	(-3.65)	0.07	(0.21)	-0.20	(-1.02)
-8	-0.23	(-1.42)	0.01	(0.04)	-0.06	(-0.28)
-7	-0.02	(-0.23)	-0.05	(-0.13)	0.07	(0.32)
-6	-0.13	(-0.76)	0.38	(0.91)	0.35	(1.42)
-5	-0.13	(-0.76)	0.29	(0.73)	0.20	(0.84)
-4	-0.24	(-1.33)	0.13	(0.34)	-0.00	(-0.01)
-3	-0.43*	(-1.87)	0.17	(0.49)	-0.04	(-0.18)
-2	-0.38*	(-1.87)	0.42	(1.16)	-0.05	(-0.22)
-1	-0.28	(-1.27)	0.56	(1.41)	0.01	(0.03)
0	0.14	(0.59)	1.28***	(2.86)	0.55*	(1.94)
1	-0.08	(-0.42)	1.79***	(3.85)	0.48*	(1.80)
2	-0.09	(-0.35)	2.22***	(4.72)	0.81***	(2.75)
3	-0.30	(-1.21)	2.10***	(4.79)	0.55**	(2.03)
4	-0.76***	(-3.72)	1.13***	(3.09)	0.11	(0.48)
5	-0.33	(-1.09)	2.15***	(4.81)	0.54*	(1.76)
6	-0.03	(-0.12)	3.10***	(5.90)	1.21***	(3.77)
7	-0.33	(-1.27)	3.07***	(6.11)	1.06***	(3.36)
8	-0.11	(-0.39)	2.61***	(6.04)	1.14***	(4.16)
9	-0.32	(-1.29)	2.49***	(5.68)	0.82***	(2.95)
Controls	Yes		Yes		Yes	
Primary Peer FE	Yes		Yes		Yes	
N	2,201,720		36,980		104,140	
Adj. R-sq.	0.733		0.695		0.729	

Panel B: Post Announcement Period Effects

	Dependent Variable = Views					
	(1)		(2)		(3)	
	Full Sample		Targeted (30)		Targeted (90)	
POST	-0.01	(-0.06)	1.99***	(8.62)	0.69***	(4.51)
Controls	Yes		Yes		Yes	
Primary Peer FE	Yes		Yes		Yes	
N	2,201,720		36,980		104,140	
Adj. R-sq.	0.733		0.695		0.729	

Table 4
Announcement Period Attention to Informationally Opaque Peers Firms' Information

The dependent variable is Views, the number of peer firm i's documents accessed on day *t* around a primary firm *j*'s acquisition announcement. POST is equal to 1 if $Date_{t,j}$ is in [0, +9], and it is equal to 0 if $Date_{t,j}$ is in [-10, -1]. All models include controls for Day, News Day, Dividend Day, and Earnings Announcement Day. All models include primary-peer fixed effects and standard errors are clustered at the primary firm deal level. All variables are defined in the appendix. All continuous variables are winsorized at their 1st and 99th percentiles. t-statistics calculated by standard errors clustered at the primary firm deal level are presented in parentheses.

Panel A: Bid-Ask Spreads

	Dependent Variable = Views					
	(1)		(2)		(3)	
	Full Sample		Targeted (30)		Targeted (90)	
POST	0.325*	(1.83)	2.555***	(6.07)	1.117***	(3.98)
High Spreads * POST	-0.595***	(-5.36)	-1.386***	(-2.78)	-0.895***	(-3.14)
Middle Spreads * POST	-0.409***	(-4.44)	-0.759	(-1.46)	-0.697**	(-2.28)
N	2,201,720		36,980		104,140	
Adj. R-sq.	0.733		0.695		0.729	

Panel B: Analyst Following

	Dependent Variable = Views					
	(1)		(2)		(3)	
	Full Sample		Targeted (30)		Targeted (90)	
POST	0.348**	(2.47)	2.572***	(5.91)	1.145***	(4.37)
Low Analysts * POST	-0.620***	(-4.11)	-1.471***	(-2.90)	-1.115***	(-3.66)
Middle Analysts * POST	-0.406***	(-2.79)	-0.320	(-0.56)	-0.398	(-1.39)
N	1,907,600		34,360		96,800	
Adj. R-sq.	0.740		0.698		0.732	

Panel C: Discretionary Accruals

	Dependent Variable = Views					
	(1)		(2)		(3)	
	Full Sample		Targeted (30)		Targeted (90)	
POST	0.045	(0.38)	1.757***	(4.99)	0.651***	(3.37)
High Accruals * POST	-0.117	(-1.10)	0.175	(0.35)	-0.031	(-0.11)
Middle Accruals * POST	-0.042	(-0.64)	0.508	(1.01)	0.121	(0.50)
N	2,184,760		36,580		103,320	
Adj. R-sq.	0.733		0.696		0.730	

Panel D: Firm Size

	Dependent Variable = Views					
	(1)		(2)		(3)	
	Full Sample		Targeted (30)		Targeted (90)	
POST	0.375***	(2.64)	2.381***	(6.15)	1.246***	(4.65)
Small Size * POST	-0.638***	(-4.50)	-1.104**	(-2.36)	-1.038***	(-3.81)
Middle Size * POST	-0.516***	(-4.78)	-0.354	(-0.67)	-0.904***	(-3.19)
N	2,201,720		36,980		104,140	
Adj. R-sq.	0.733		0.695		0.729	

Table 5
Probability that Peer Firms will be Targeted

The dependent variables in Columns 1 and 3 are equal to 1 if firm *i* is targeted for acquisition 30 days after primary firm announcement date and 0 otherwise. The dependent variables in Columns 2 and 4 are equal to 1 if the firm is targeted 90 days after primary firm announcement date and 0 otherwise. Δ*Views* is the change in *Views* on days [-10, -1] – *Views* on days [0, +9]. Columns 1 and 2 present results of OLS regressions. Columns 3 and 4 present results of logistic regressions. All covariates are defined in the appendix and have been standardized to have a mean of 0 and a standard deviation of 1. All continuous variables are winsorized at their 1st and 99th percentiles. t-statistics (z-statistics) calculated by standard errors clustered at the primary firm deal level are presented in parentheses in Columns 1 and 2 (3 and 4). All specifications include primary deal fixed effects and standard errors are clustered at the primary deal level. All coefficients are reported as percentages.

	OLS		Logit	
	Targeted (30)	Targeted (90)	Targeted (30)	Targeted (90)
	(1)	(2)	(3)	(4)
ΔViews	0.782***	0.839***	0.358***	0.161***
	(9.66)	(8.00)	(12.49)	(8.83)
CARs	0.268***	0.221***	0.166***	0.058***
	(5.47)	(3.26)	(5.82)	(3.37)
Firm Size	0.368***	1.134***	0.144***	0.215***
	(6.82)	(11.94)	(4.39)	(10.86)
Leverage	-0.057	-0.063	-0.029	-0.010
	(-1.19)	(-0.82)	(-0.83)	(-0.49)
Tobin's Q	-0.102***	-0.178***	-0.093***	-0.057***
	(-2.87)	(-2.74)	(-3.05)	(-3.05)
Property Ratio	-0.199***	-0.544***	-0.130***	-0.125***
	(-3.59)	(-5.78)	(-3.30)	(-5.23)
Liquidity Ratio	-0.042	-0.287***	-0.023	-0.060***
	(-0.88)	(-3.81)	(-0.70)	(-3.19)
Sales Growth	-0.106***	-0.295***	-0.145***	-0.153***
	(-4.25)	(-6.05)	(-3.33)	(-4.69)
ROA	-0.005	0.067	0.094**	0.109***
	(-0.11)	(0.89)	(2.00)	(3.84)
BHARs	0.051	0.107	0.049**	0.037**
	(1.41)	(1.62)	(1.99)	(2.33)
Industry Concentration	-0.338	-0.323	-0.460	-0.088
	(-0.96)	(-0.34)	(-1.12)	(-0.31)
N	110,086	110,086	95,838	106,110
Adj. / Pseudo R-sq.	0.010	0.014	0.030	0.017

Table 6
Attention and Announcement Period Returns

The dependent variables in Columns 1 and 3 are equal to 1 if firm *i* is targeted for acquisition 30 days after primary firm announcement date and 0 otherwise. The dependent variables in Columns 2 and 4 are equal to 1 if the firm is targeted 90 days after primary firm announcement date and 0 otherwise. *Low*, *Middle*, and *High CARs* are dummy variables indicating whether a peer firm's *CARs* calculated in the [-2, 2] day period around primary firm announcements are in the first, second and third terciles relative to other peers, respectively. Columns 1 and 2 present results of OLS regressions. Columns 3 and 4 present results of logistic regressions. All covariates are defined in the appendix. All continuous covariates are standardized to have a mean of 0 and a standard deviation of 1 and are winsorized at their 1st and 99th percentiles. t-statistics (z-statistics) calculated by standard errors clustered at the primary firm deal level are presented in parentheses in Columns 1 and 2 (3 and 4). All specifications include primary deal fixed effects and standard errors are clustered at the primary deal level. All coefficients are reported as percentages.

	OLS		Logit	
	Targeted (30)	Targeted (90)	Targeted (30)	Targeted (90)
	(1)	(2)	(3)	(4)
ΔViews	0.674***	0.734***	0.344***	0.154***
	(6.08)	(4.77)	(7.54)	(5.04)
ΔViews * High CAR	0.430***	0.490**	0.084*	0.082**
	(2.61)	(2.37)	(1.66)	(2.23)
ΔViews * Middle CAR	-0.104	-0.152	-0.060	-0.049
	(-0.76)	(-0.71)	(-1.11)	(-1.25)
High CAR	0.026	-0.241	0.003	-0.081
	(0.14)	(-0.91)	(0.02)	(-1.31)
Middle CAR	-0.182	0.124	-0.059	0.022
	(-1.53)	(0.68)	(-0.81)	(0.53)
CAR	0.252***	0.294***	0.148***	0.080***
	(2.90)	(2.69)	(3.13)	(2.91)
Firm Size	0.384***	1.119***	0.154***	0.212***
	(7.04)	(11.83)	(4.66)	(10.78)
Leverage	-0.061	-0.062	-0.031	-0.010
	(-1.27)	(-0.80)	(-0.90)	(-0.50)
Tobin's Q	-0.103***	-0.180***	-0.093***	-0.057***
	(-2.90)	(-2.77)	(-3.05)	(-3.06)
Property Ratio	-0.200***	-0.538***	-0.129***	-0.123***
	(-3.61)	(-5.72)	(-3.29)	(-5.15)
Liquidity Ratio	-0.042	-0.284***	-0.022	-0.059***
	(-0.89)	(-3.76)	(-0.68)	(-3.13)
Sales Growth	-0.108***	-0.293***	-0.147***	-0.152***
	(-4.36)	(-6.02)	(-3.36)	(-4.67)
ROA	-0.001	0.062	0.094**	0.107***
	(-0.01)	(0.82)	(2.00)	(3.77)
BHAR	0.051	0.108*	0.048**	0.037**
	(1.39)	(1.65)	(1.97)	(2.35)
Industry Concentration	-0.343	-0.314	-0.495	-0.095
	(-0.98)	(-0.33)	(-1.24)	(-0.33)
N	110,086	110,086	95,838	106,110
Adj. / Pseudo R-sq.	0.011	0.014	0.031	0.018

Table 7
Attention and Tobin's Q

The dependent variables in Columns 1 and 3 are equal to 1 if firm *i* is targeted for acquisition 30 days after primary firm announcement date and 0 otherwise. The dependent variables in Columns 2 and 4 are equal to 1 if the firm is targeted 90 days after primary firm announcement date and 0 otherwise. *Low*, *Middle*, and *High TQ* are dummy variables indicating whether a peer firm's *Tobin's Q* at primary firm announcement is in the first, second and third terciles relative to other peers, respectively. Columns 1 and 2 present results of OLS regressions. Columns 3 and 4 present results of logistic regressions. All covariates are defined in the appendix. All continuous covariates are standardized to have a mean of 0 and a standard deviation of 1 and are winsorized at their 1st and 99th percentiles. t-statistics (z-statistics) calculated by standard errors clustered at the primary firm deal level are presented in parentheses in Columns 1 and 2 (3 and 4). All specifications include primary deal fixed effects and standard errors are clustered at the primary deal level. All coefficients are reported as percentages.

	OLS		Logit	
	Targeted (30)	Targeted (90)	Targeted (30)	Targeted (90)
	(1)	(2)	(3)	(4)
ΔViews	0.681***	0.641***	0.342***	0.135***
	(6.36)	(4.80)	(7.87)	(4.88)
ΔViews * Low TQ	0.302**	0.362*	0.117**	0.074*
	(1.97)	(1.70)	(2.26)	(1.83)
ΔViews * Middle TQ	0.054	0.279	-0.038	0.019
	(0.39)	(1.43)	(-0.76)	(0.55)
Low TQ	-0.373**	-0.497**	-0.309***	-0.134**
	(-2.53)	(-2.09)	(-2.86)	(-2.18)
Middle TQ	0.134	0.646***	0.058	0.116**
	(1.06)	(3.26)	(0.69)	(2.45)
CAR	0.270***	0.224***	0.167***	0.059***
	(5.51)	(3.31)	(5.84)	(3.38)
Firm Size	0.344***	1.080***	0.131***	0.205***
	(6.37)	(11.44)	(3.99)	(10.35)
Leverage	-0.062	-0.079	-0.032	-0.014
	(-1.28)	(-1.01)	(-0.93)	(-0.72)
Tobin's Q	-0.162***	-0.201**	-0.153***	-0.070**
	(-3.09)	(-2.21)	(-2.99)	(-2.43)
Property Ratio	-0.184***	-0.506***	-0.121***	-0.116***
	(-3.31)	(-5.39)	(-3.05)	(-4.86)
Liquidity Ratio	-0.057	-0.304***	-0.032	-0.065***
	(-1.18)	(-3.98)	(-0.96)	(-3.38)
Sales Growth	-0.106***	-0.293***	-0.145***	-0.152***
	(-4.25)	(-6.02)	(-3.30)	(-4.64)
ROA	-0.014	0.056	0.084*	0.102***
	(-0.31)	(0.74)	(1.79)	(3.61)
BHAR	0.031	0.066	0.031	0.025
	(0.84)	(1.01)	(1.27)	(1.63)
Industry Concentration	-0.339	-0.331	-0.468	-0.075
	(-0.97)	(-0.34)	(-1.08)	(-0.26)
N	110,160	110,160	95,863	106,156
Adj. / Pseudo R-sq.	0.011	0.015	0.033	0.018

Table 8
Attention and Prior Year Returns

The dependent variables in Columns 1 and 3 are equal to 1 if firm *i* is targeted for acquisition 30 days after primary firm announcement date and 0 otherwise. The dependent variables in Columns 2 and 4 are equal to 1 if the firm is targeted 90 days after primary firm announcement date and 0 otherwise. *Low*, *Middle*, and *High BHAR* are dummy variables indicating whether a peer firm's *BHARs* calculated over the year prior to primary firm announcement are in the first, second and third terciles relative to other peers, respectively. Columns 1 and 2 present results of OLS regressions. Columns 3 and 4 present results of logistic regressions. All covariates are defined in the appendix. All continuous covariates are standardized to have a mean of 0 and a standard deviation of 1 and are winsorized at their 1st and 99th percentiles. t-statistics (z-statistics) calculated by standard errors clustered at the primary firm deal level are presented in parentheses in Columns 1 and 2 (3 and 4). All specifications include primary deal fixed effects, and standard errors are clustered at the primary deal level. All coefficients are reported as percentages

	OLS		Logit	
	Targeted (30)	Targeted (90)	Targeted (30)	Targeted (90)
	(1)	(2)	(3)	(4)
ΔViews	0.564***	0.546***	0.275***	0.106***
	(4.78)	(3.80)	(5.90)	(3.86)
ΔViews * Low BHAR	0.373**	0.614***	0.179***	0.135***
	(2.30)	(2.80)	(3.19)	(3.42)
ΔViews * Middle BHAR	0.311**	0.328*	0.087*	0.048
	(2.07)	(1.68)	(1.71)	(1.42)
Low BHAR	-0.340**	-0.465*	-0.315***	-0.127**
	(-2.17)	(-1.69)	(-3.10)	(-2.02)
Middle BHAR	-0.034	0.118	-0.072	0.009
	(-0.27)	(0.58)	(-0.96)	(0.20)
CAR	0.269***	0.222***	0.167***	0.059***
	(5.48)	(3.27)	(5.85)	(3.39)
Firm Size	0.344***	1.093***	0.128***	0.206***
	(6.25)	(11.35)	(3.83)	(10.19)
Leverage	-0.052	-0.054	-0.026	-0.008
	(-1.08)	(-0.70)	(-0.74)	(-0.39)
Tobin's Q	-0.112***	-0.193***	-0.097***	-0.059***
	(-3.07)	(-2.88)	(-3.14)	(-3.08)
Property Ratio	-0.195***	-0.536***	-0.128***	-0.124***
	(-3.51)	(-5.70)	(-3.25)	(-5.17)
Liquidity Ratio	-0.038	-0.282***	-0.020	-0.059***
	(-0.80)	(-3.73)	(-0.62)	(-3.14)
Sales Growth	-0.104***	-0.292***	-0.144***	-0.152***
	(-4.21)	(-6.03)	(-3.34)	(-4.69)
ROA	-0.020	0.040	0.081*	0.101***
	(-0.44)	(0.53)	(1.74)	(3.61)
BHAR	-0.044	-0.016	-0.028	0.006
	(-0.85)	(-0.17)	(-0.75)	(0.25)
Industry Concentration	-0.345	-0.328	-0.482	-0.094
	(-0.98)	(-0.34)	(-1.21)	(-0.33)
N	110,086	110,086	95,838	106,110
Adj. / Pseudo R-sq.	0.011	0.014	0.032	0.018

Table 9
Descriptive Statistics for Subsample of Completed and Withdrawn Deals

Descriptive statistics for the subsample of 1,240 withdrawn deals in which a bidder initially seeks to own 100% of target firms' shares as well as completed deals in which a bidder owns 100% of target firms' shares after completion. All variables are defined in the appendix. All continuous variables are winsorized at their 1st and 99th percentiles.

Variable	Mean	Std. Dev.	P25	P50	P75
Completed	0.842				
ABViews	2.095	0.713	1.597	2.002	2.573
CAR	1.828	0.567	1.430	1.786	2.197
Firm Size	27.120	23.540	10.939	22.85	39.845
Leverage	5.919	1.710	4.642	5.807	7.076
Tobin's Q	0.202	0.223	0.000	0.129	0.331
Property Ratio	1.813	1.126	1.126	1.465	2.093
Liquidity Ratio	0.212	0.222	0.054	0.119	0.296
Sales Growth	0.234	0.225	0.049	0.156	0.364
ROA	0.088	0.312	-0.043	0.049	0.165
BHAR	0.006	0.178	-0.021	0.049	0.098
Industry Concentration	14.316	54.575	-20.692	7.975	42.076
Percentage Tendered	17.53	36.65	0.00	0.00	0.00
Public Bidder	0.554				
All Cash	0.659				
Stock Deal	0.233				

Table 10
Effect of Attention on Probability and Rate of Deal Completion

In these tests, the sample consists of withdrawn deals in which a bidder initially seeks to own 100% of target firms' shares as well as completed deals in which bidder owns 100% of target firms' shares after completion. Column 1 presents results of logistic regressions of *Completed* on *ABViews*. Column 2 presents results of a Cox proportional hazards regression of *Completed* on *ABViews* in which the completion hazard is right-censored at 643 days. Completed is equal to 1 if firm *i* is taken over within the sample period and 0 otherwise. *ABViews* is a firm's average *Views* in days [-10, +9] / its average *Views* in days [-180, -11] around the merger or acquisition announcement date. All covariates are defined in the appendix and have been standardized to have a mean of zero and a standard deviation of one. All continuous variables are winsorized at their 1st and 99th percentiles. z- statistics calculated by standard errors clustered by year × month are presented in parentheses.

	Logit		Hazard	
	(1)		(2)	
ABViews	0.773***	(4.22)	0.529***	(3.09)
CAR	0.454***	(2.76)	0.126***	(3.18)
Firm Size	-0.067	(-0.41)	-0.341***	(-4.79)
Leverage	-0.085	(-0.42)	0.144**	(2.35)
Tobin's Q	0.135	(0.62)	0.011	(0.52)
Property Ratio	0.124	(0.45)	-0.128	(-1.51)
Liquidity Ratio	0.255	(1.35)	0.153**	(2.52)
Sales Growth	-0.176	(-1.40)	-0.043	(-0.69)
Return on Assets	0.368*	(1.84)	0.098	(1.97)
BHAR	0.090	(0.48)	0.104*	(2.37)
Industry Concentration	-0.438	(-0.76)	0.246	(0.58)
Public Bidder	-0.446	(-1.28)	0.044	(0.76)
All Cash Deal	0.173	(0.38)	0.256	(1.77)
Stock Deal	1.233**	(2.08)	0.138	(0.86)
Percentage Tendered	2.759***	(4.69)	0.614***	(11.56)
Industry FE	Yes		Yes	
Year × Month FE	Yes		Yes	
N	824		1,215	
Pseudo. R-sq. / Chi-sq.	0.346		0.072	

CHAPTER II

INDEXING AND INFORMATION ACQUISITION

Abstract

I investigate the impact of passive ownership on information acquisition using measures of firm-level information acquired via the Security and Exchange Commission's EDGAR website and from Google searches. I identify variation in passive ownership that is uncorrelated with underlying firm fundamentals by exploiting discontinuities in passive ownership among firms in the Russell indices. On average, I find that passive ownership causes information acquisition to decrease. These results are consistent with the view that the costs to passive owners of acquiring and analyzing portfolio firm information exceed the expected benefits of monitoring. Notwithstanding, cross-sectional analyses support the view that passive owners are not passive monitors. Decreases in information acquisition associated with passive ownership are not as large for the least transparent firms.

1. Introduction

In this paper, I investigate whether passive ownership gives rise to changes in information gathering. Because monitoring of any form requires that shareholders first gather firm-specific information in order to determine whether intervention is warranted, my investigation provides insight into the role that passive owners play as monitors. The managers of actively managed mutual funds (collectively, active investors or active owners) can buy and sell shares of firms in their portfolios in an attempt to earn abnormal returns. On the other hand, the managers of passively managed mutual funds (i.e., passive investors or passive owners) hold portfolios that mimic indices to which they are benchmarked.

Passive owners play an increasingly important role in the United States' (U.S.) equity markets. Appel, Gormley and Keim (2016a) estimate that between 1998 and 2014, the share of equity mutual fund assets held in passively managed funds tripled to 33.5% and the proportion of the total U.S. market capitalization held by passively managed funds quadrupled to more than 8%. Given the prevalence of passive investing, the extent to which passive owners monitor their portfolio firms has become an increasingly important topic of discussion among regulators and academics.

Investors can monitor by selling shares of poorly managed or poorly performing firms (exit), participating in shareholder proposals (voice), and by communicating concerns directly to managers (jawboning). Because passive investors are contractually obligated to hold portfolios that mimic their benchmarks, they are limited in their ability to use the exit mechanism. Gathering and analyzing information is necessary for monitoring but it is costly, and passive funds that track the same index compete with each other on the basis of cost. For these reasons and because there are many passive funds tracking any given index, managers of a given passive fund may be disincentivized to pay monitoring costs that would benefit other funds benchmarked to the same index (Bebchuck and Hirst (2020)). On the other hand, passive fund managers could be incentivized to monitor, especially through voice, because they are limited in their ability to exit positions in poorly performing stocks (Fisch, Hamdani and Solomon (2020)). In an opinion piece published in the Wall Street Journal in July 2017, chairman and then-CEO of Vanguard, Bill McNabb[10] wrote:

> Index fund managers care about good governance more than anyone else. We are the ultimate long-term investors because we own stocks forever. Therefore, our vote and our voice on governance are the most important levers we have to protect our clients' investments. When we detect material risks to a company's long-term value (such as bad leadership, poor disclosure, misaligned compensation structures, or threats to shareholder rights), we act with our voice and our vote.

In this paper, I compare the amount of information acquired on a treatment group of firms with high levels of passive ownership to that of a control group with lower levels of passive ownership. I find that on average, passive ownership decreases information acquisition, but results of cross-sectional analyses reveal that these decreases are not as pronounced for the least transparent firms.

This investigation presents two empirical challenges: how to best identify exogenous variation in passive ownership and how to measure information acquisition. My empirical strategy overcomes endogeneity concerns by exploiting discontinuities in passive ownership among firms around the breakpoint between the Russell 1000 and 2000 indices. The Russell indices are constructed on the basis of firms' market capitalizations. Firms in the Russell 1000 have larger market capitalizations than firms in the Russell 2000, yet the smallest firms in the Russell 1000 have lower levels of passive ownership than the largest firms in the Russell 2000. My empirical

[10] https://www.wsj.com/articles/proxy-votes-certainly-matter-to-index-funds-1499289400

design uses inclusion in the Russell 2000 index as a proxy for passive ownership. I measure information gathering using two information sources. The first information source is the Security and Exchange Commission's (SEC) Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database. EDGAR is a publicly available database of all publicly traded firms' SEC filings. EDGAR data are used by institutional investors (Crane, Crotty, and Ulmar (2018); Chen, Kelly, and Wu (2018); Iliev, Kalodimos, and Lowry (2019)) as well as others, including academics and retail investors. There are drawbacks to using EDGAR data as a sole measure of information acquisition. For instance, investors can obtain information from multiple sources, and they may have good reasons for doing so. While the SEC does not charge a fee to access EDGAR data, analyzing the data can be costly. It is possible that the cost to passive owners of analyzing their portfolio firms' SEC filings may prohibit them from doing so. It is also possible that passive owners gather their portfolio firms' information from EDGAR and supplement it with information from other sources. Therefore, I also examine whether passive ownership impacts the number of searches for their portfolio firms' information on the internet. To do so, I obtain a search volume index of Google web searches for firms' ticker symbols from the Google Trends database.

Identifying variation in passive ownership that is uncorrelated with underlying firm fundamentals presents an empirical challenge because the amount of passive money invested in a stock is not random. For example, many stock market indices are constructed on the basis of firms' market capitalizations. If an investor analyses a firm's information and decides to trade a sufficiently large number of the firm's shares because of this analysis, the trade will impact the stock's price. Sufficiently large changes in the firm's stock price could change its market capitalization ranking such that it will be added to or removed from an index. Therefore, because passive owners are benchmarked to indices, information gathering could determine passive ownership. The Russell indices provide a laboratory in which to examine the impact of exogenous variation in passive ownership on outcome variables because the indices are constructed on the basis of market capitalization (which is plausibly outside of firms' control), they are value-weighted, and they are popular benchmarks for passively-managed index funds. The Russell 1000 is comprised of the 1,000 largest-ranked stocks traded on US exchanges, and the Russell 2000 is comprised of the next 2,000 largest stocks. Because the indices are value-weighted, more passive money is invested in the largest firms in the Russell 2000 than in the smallest firms in the Russell 1000.[11]

I use discontinuities in passive ownership around the breakpoint between the Russell indices to identify variation in passive ownership that is uncorrelated with firms' fundamental information. The sample consists of firms within +/- 200 ranks of the breakpoint between the Russell indices each year. I begin by demonstrating that passive ownership is significantly greater for firms in the Russell 2000, and I then use Russell 2000 membership as a proxy for higher passive ownership in regressions of information acquisition on passive ownership. Consistent with the view that passive owners' monitoring costs exceed their expected benefits, I find that on average, less information is gathered on firms with higher passive ownership. The average monthly views of a firm's data from the EDGAR website over the 10 months after the reconstitution of the Russell indices is over 467 views per month, with a standard deviation of over 402. I find that inclusion in the Russell 2000 causes a reduction in the average number of views over this time period of over 117 views per month, which is equivalent to a 0.29 (=117/402) standard deviation change in the mean number of monthly views.

[11] See Chang et al. (2015); Appel et al. (2016a); Crane et al. (2016); Schmidt and Fahlenbrach (2017); Coles et al. (2019); and Heath et al. (2019), among many others.

Cross-sectional evidence suggests that passive owners may not forgo monitoring completely, but rather continue to gather information for the least transparent firms. The reduction in information gathering caused by passive ownership is not as great for smaller firms, firms with greater abnormal accruals, and firms that are followed by fewer analysts. Relative to firms with the lowest levels of accruals, firms with the highest accruals experience 29% less of a reduction in information gathering via EDGAR over the 10 months after the reconstitution of the Russell indices that is associated with passive ownership. Relative to firms with the greatest analyst following, firms with the least analyst following experience an 87% reduction in the decrease in the acquisition of information via EDGAR over this same time period that is associated with passive ownership. Relative to the largest firms, the smallest firms experience a 45% reduction in the decrease in the average number of monthly views via EDGAR in the 10 months after reconstitution that is associated with passive ownership.

Given the increasing amount of passive money in the United States' capital markets, the degree to which passive owners monitor their portfolio firms has become an increasingly important debate in the literature. Fisch, Hamdani, and Solomon (2020) argue that passive owners are incentivized to monitor because although their ability to use the exit strategy is limited, the same is not true for their investors. If passive funds underperform active funds on a fee-adjusted basis, they risk investors moving their capital to active funds. They argue that in this way, passive funds compete with active funds for investors, and because they cannot monitor via exit, they do so with voice. Providing an alternative view, Bebchuk and Hirst (2020) suggest that passive funds compete for investors' capital not with active funds, but with each other. They point out that passive funds outperform active funds on average (e.g., see Wermers (2000)). To the extent that this drives investors from active to passive funds, passive funds compete with each other on the basis of cost because investors in any given passive fund could earn similar returns by choosing to invest in another fund that tracks the same benchmark. Bebchuk and Hirst (2020) argue that passive funds are disincentivized to monitor because if a given passive fund increases its fees to cover monitoring costs, it will earn the same gross return as rival passive funds but its net returns will be reduced by the amount of its monitoring costs. In addition, expected gains from governance improvements will disproportionately accrue to active funds with portfolios that overweight the improved firms.

The extent to which passive owners acquire their portfolio firms' information is ultimately an empirical question. It is possible that passive owners acquire more information than non-passive owners because their exit limitations give rise to increased monitoring incentives, and monitoring is predicated upon the acquisition of information. Providing support for the view that passive owners actively monitor firms in their portfolio, Appel, Gormley, and Keim (2016) find that passive ownership results in fewer takeover defenses, larger voting blocks, and increases in the number of independent directors. Boone and White (2015) argue that because quasi-indexers must buy and sell shares of the index portfolio in response to fund flows, they have preferences for increased managerial disclosure because it results in improved liquidity and lower transaction costs by reducing information asymmetry. They provide evidence that quasi-indexers' informational preferences impact firms' disclosure practices: firms with more quasi-indexer ownership produce more voluntary SEC Form 8-K filings and management forecasts, their forecasts are timelier (i.e., they are released earlier in the fiscal period), and they exhibit greater specificity (i.e., the forecasts tend to be point, rather than range, earnings forecasts).

It could be the case that passive owners play a less active monitoring role and acquire less information on their portfolio firms than non-passive owners. Heath, Macciocchi, Michaely, and Ringgenberg (2019) demonstrate that relative to the active managers, passive managers are less likely to vote against portfolio firms' management and do not engage in jawboning. Providing further support for the view that passive ownership weakens firm governance, Schmidt and Fahlenbrach (2017) find that firms react to increases in passive ownership by increasing CEO power, decreasing the appointment of new independent directors, and making worse acquisitions.

This paper contributes to the discussion of the role that passive owners play as monitors because the acquisition of information is required for monitoring. In cross-sectional analyses, I find that the reduction in information brought about by passive ownership is not as great for the least transparent firms. These results are consistent with the view that while the costs to passive owners of acquiring and analyzing information on each of their portfolio firms may outweigh the benefits, they focus their resources on the firms with the greatest information asymmetry. I also contribute to a growing area of the literature that examines institutional investors' information acquisition practices. A number of studies provide evidence that institutional investors profit from information acquisition. Crane et al. (2018) demonstrate that hedge funds that use EDGAR to acquire firms' SEC filings earn 1.5% higher annualized returns than non-users. Chen et al. (2018) demonstrate that after exogenous reductions in analyst coverage due to closures of brokerage firms, hedge funds increase the acquisition of information via EDGAR, trade more aggressively, and earn higher returns on the affected stocks. Chen, Cohen, Gurun, Lou, and Malloy (2020) identify institutions that use EDGAR to track inside sales of particular individuals at particular firms. They provide evidence that institutions are skilled at using EDGAR to estimate which inside sales are conducted for liquidity purchases. Other papers demonstrate that a lack of attention to portfolio firms' fundamental information is costly. Ben-Raphael et al. (2017) examine institutions' news searching and news reading activity though Bloomberg terminals and show that price drifts following earnings announcements and analyst recommendation changes are driven by announcements to which institutions fail to pay sufficient attention. I contribute to the discussion of institutional investors' attention and information acquisition activity by demonstrating that on average, ownership by passive investors causes decreases in the amount of attention that investors pay to firms. Further, I demonstrate that these decreases in attention are not as pronounced for the least transparent firms.

Finally, I contribute to the literature that examines the type of firms that passive owners monitor. Iliev and Lowry (2015) examine the extent to which mutual funds independently assess and evaluate the issues up for vote in shareholder proposals rather than relying on recommendations from Institutional Shareholder Services, a proxy advisory service company. They show that index funds are more likely to vote actively on a firm's proposals if the firm constitutes a greater proportion of the fund's total net assets or if the fund holds a greater percentage of the firm's shares. I contribute to this discussion by demonstrating that although passive ownership decreases information gathering on average, these decreases are attenuated for the least transparent firms. These results are consistent with the view that passive owners focus their limited resources on the most informationally opaque firms in their portfolios.

The rest of the paper is organized as follows. Section 2 provides a background on Russell indices and describes my data and research design. I present results in Section 3, and Section 4 concludes.

2. Data and Research Design

I examine the impact of passive ownership on information gathering. To do so, I use variation in Russell index membership to identify variation in passive ownership that is not correlated with firm fundamentals. I combine stock data from the Center for Research in Security Prices (CRSP) with firm data from Compustat and mutual fund holdings data from the Thomson

Reuters S12 ownership database with Russell 3000 index membership data from the Russell group. Russell data cover the 2007 -2016 reconstitutions and include firm names, Committee on Uniform Securities Identification Procedure (CUSIP) numbers, ticker symbols, index membership data, proprietarily calculated estimates of firms' float-adjusted end-of-June market capitalizations, and the final index weights that are based on the end-of-June market capitalizations. In addition, I obtain analyst data from the Institutional Brokers' Estimate System (IBES), and I obtain information acquisition data from the SEC's EDGAR and Google Trends websites.

2.1. Background on the Russell Indices

The Russell 1000 is a value-weighted index of the largest 1,000 stocks listed on U.S. exchanges. The Russell 2000 is a value-weighted index of the next 2,000 largest stocks. The Russell indices are popular benchmarks for passive funds. The Russell group reports that 97% of institutional small cap products are benchmarked against the Russell 2000 (FTSE Russell (2014)). Because the Russell indices are value-weighted, the smallest firms in the Russell 1000 have smaller portfolio weights than the largest firms in the Russell 2000. Crane, Michenaud, and Weston (2016) find that in 2005, the combined index weight of the ten largest firms in the Russell 2000 is 575 times larger than the combined index weights of the ten smallest firms in the Russell 1000. A result of benchmarking to the value-weighted Russell indices is that passive ownership of the largest firms in the Russell 2000 is greater than that of the smallest firms in the Russell 1000. Appel, Gormley, and Keim (2015) estimate that passive institutional ownership is 66% higher for the 250 largest firms in the Russell 2000 compared to that of the 250 smallest firms in the Russell 1000.

The Russell indices are reconstituted each June. To determine index membership, Russell ranks stocks traded on major U.S. exchanges by market capitalization on the last trading day of May. The list of index memberships is released to the public in June and the indices are reconstituted on the last trading day of June. Index assignment is determined by firms' end-of-May market capitalization and index rank is determined by float-adjusted market capitalization as of the last trading day of June. Prior to 2007, the Russell group used a simple rule based on end-of-May market capitalization to determine index membership: the 1,000 largest firms were sorted into the Russell 1000 and the 1,001st – 3,000th largest firms were sorted into the Russell 2000. From 2007 onward, Russell introduced a banding rule to limit the turnover of stocks within the indices. Under banding, Russell computes upper and lower bands around the breakpoint between the two indices (i.e., the 1,000th ranked firm). Banded firms retain their previous index assignment. For example, if the market capitalization of the 1,001st ranked firm (i.e., largest firm in the Russell 2000) increases by one rank when Russell determines index assignment in the following year, under banding, it will remain in the Russell 2000 after reconstitution.

2.2. Russell Data and Research Design

The Russell 1000 and 2000 indices provide a laboratory in which to examine the impact of exogenous variation in passive ownership on outcome variables. In the pre-banding regime, index assignment and passive ownership are discontinuous around the breakpoint between the two indices (Chang et al. (2015); Appel et al. (2016a); Crane et al. (2016)). Further, index membership is solely determined by firms' end-of-May market capitalizations in the pre-banding regime, and it is based on an arbitrary breakpoint between the two indices which occurs between the 1,000th and 1,001st largest firms. Because index rankings are based on end-of-May market capitalizations, which are arguably outside of firms' control, the reconstitution event provides exogenous variation in index membership around the breakpoint. My sample period is 2007 – 2016, which is entirely under the banding regime.

In my main analysis, I use Russell 2000 membership as a proxy for passive ownership and I include controls for sample firms' rank within each index and for whether they are near the upper band because these variables impact the proportion of firms' shares owned by passive investors through their impact on firms' position within the Russell 2000 and through their impact on the probability that a firm will be assigned to the Russell 2000, respectively.[12] In additional tests, I follow Appel et al. (2016) and use Russell 2000 membership as an instrument for passive ownership in two-stage least squares (2SLS) regressions of information acquisition on passive ownership. These 2SLS regressions include the controls for rank and imputed membership in the upper band as well.

Banding and rank depend on firms' end-of-May market capitalizations, which I calculate following the procedure proposed by Chang et al. (2015). Specifically, I compute firms' end-of-May market capitalization (*CAP*) using all publicly available price and share data in CRSP and Compustat. For each firm in the Russell 3000 after the 2007 - 2016 reconstitutions, I construct *CAP* as the product of its dividend and split-adjusted stock price as of the last trading day in May from CRSP and the maximum of the dividend and split-adjusted shares from CRSP or the number of company-level shares outstanding that are recorded in Compustat. I sort all Russell 3000 stocks each year on *CAP* to produce an imputed Russell 3000 ranking variable *CAPrank,* with *CAPrank* = 1 being equal to the position of the largest firm in the Russell 1000. I then impute the yearly breakpoint between the Russell 1000 and Russell 2000 indices as the breakpoint between the firm with *CAPrank* = 1,000 and *CAPrank* = 1,001. I follow Coles et al. (2020) and the procedure outlined in the Russell group's construction and methodology guide to impute the bands.[13]

[12] The banding procedure is described in detail below. Whether a firm is banded depends fully on its ranking in the Russell 3000, whether it was in the Russell 1000/2000 in the prior year, the market capitalization of the 1000th ranked stock, the total market capitalization of the Russell 3000, and the cumulative market capitalization of all closely-ranked stocks. See Coles et al. (2020) and the FTSE Russell handbook for more information.
[13] The guide can be accessed at https://research.ftserussell.com/products/downloads/Russell-US-indexes.pdf

In order to compute the bands, I rank all Russell 3000 stocks on *CAP*, and compute the running market capitalization of each firm. The running market capitalization of the 1st ranked firm in the Russell 3000 (i.e., the largest) is equal to its *CAP*. The running market capitalization of the 2nd ranked firm is equal to the sum of its *CAP* and that of the 1st ranked firm. The running market capitalization of the 3rd ranked firm is equal to the sum of its *CAP* and the *CAPs* of the 1st and 2nd ranked firms, and so on. I then sum *CAP* across all firms in the Russell 3000 (this is also equal to the running market capitalization of the 3000th ranked firm). The upper band is the rank for which the running market capitalization is just below the running market capitalization of the 1000th ranked firm minus 2.5% of the total market cap of the Russell 3000.

I classify funds that are benchmarked to the Russell 2000 as passive funds. To do so, I calculate the ownership of each sample firm by each fund in the Thomson S12 mutual fund ownership database as of the December prior to reconstitution (i.e., in year *t-1*). I use Cremers and Petajisto's (2009) Active Share measure to classify funds as active or passive. Specifically, for each fund *f*, I calculate its *Active Share* relative to Russell 2000 as:

$$Active\ Share_f = \frac{1}{2}\sum_{i=1}^{N}\left|\omega_{f.i} - \omega_{Russell\ 2000,i}\right|. \quad (1)$$

Following Cremers and Petajisto (2009) and Coles et al. (2018), I classify funds with Active Share less than 0.6 as passive and funds with Active Share greater than or equal to 0.6 as active. For each sample stock, I calculate the percent of its market capitalization owned by passive funds $\left(FundOwn_{i,t}^{PASSIVE}\right)$, active funds $\left(FundOwn_{i,t}^{ACTIVE}\right)$, or all funds in the S12 database $\left(FundOwn_{i,t}\right)$ as of December of reconstitution year *t*. I present descriptive statistics in Table 11. The sample firms are similar in size by construction, with an average imputed end-of-May market capitalization of $2.34 billion. The average passive fund ownership is 3.7% of shares outstanding with a standard deviation of 2.5%. The average active fund ownership is 36.5% of shares outstanding with a standard deviation of 16.6%.

2.3. EDGAR Data

Securities regulations require publicly traded firms to file financial documents which the SEC makes available on their EDGAR website. These documents include periodic reports, material disclosures, proxy materials, and other forms. When investors access these documents via the SEC's EDGAR website, the SEC captures metadata pertaining to the information requests which are recorded in the EDGAR log file dataset and available on the SEC's website.[14] EDGAR log file data include the information accessor's partially anonymized IP address, the date and time of the interaction with the website, firm identifiers, and form identifiers. EDGAR log file data are available from January 2003 until June 2017, but data from September 2005 – May 2006 are missing or corrupted. Therefore, I collect EDGAR log file data for the period beginning in May 2007 and ending in April 2017.[15] I follow the previous literature and clean the data of machine-generated information requests using the method proposed by Ryans (2017). The process involves excluding from my sample records pertaining to IP addresses that access a) more than 25 documents per minute, b) documents from more than three firms per minute, and c) more than 500 documents in any calendar day. I also eliminate requests in which the user self-identifies as a web crawler, logs of visits to index pages, or requests that are not marked with Apache Code 200 (a verification that a request is received and understood by the SEC's computer system).

[14] These data can be downloaded at https://www.sec.gov/dera/data/EDGAR-log-file-data-set.html.

[15] My Russell data include index constituents following the 2007 – 2016 reconstitutions. Because my attention measures capture attention to Russell index members over the 10 months after index reconstitution, I collect EDGAR data from January 2007 through April 2017. Because Google SVI measures are scaled by a time-series maximum, I collect each firm's SVI from January 2007 through December 2017, though my analyses do not utilize SVI measures after April 2017.

Russell index membership is determined in May of each year, and each firms' ranking within the index to which they are assigned is determined in June. Because index assignment and ranking impact cash flows from passive owners to index constituents, it is likely that arbitragers increase information gathering during these months. Therefore, I do not include in my analyses data on information acquisition that occurs in the calendar months of May and June. For each sample firm *i* over each month *m* from the July after the Russell reconstitution until the following April, I calculate the average number of times that its SEC filings are accessed via EDGAR ($EVIEWS_{i,m}$).[16] I present descriptive statistics in Table 11. The mean $EVIEWS$ in July (i.e., $EVIEWS_{m+1}$) is 446.5, which indicates that on average, each sample firm's documents are accessed 446.5 times each day in the month of July after Russell's annual reconstitution. As there are 31 days in the month of July, this amounts to an average of over 15 views per firm per day.

2.4. Google Trends Data

Passive owners' low fee structure may limit their incentive or ability to acquire and analyze complex data. A vast quantity of firm-level data are easily accessible on the internet, however. It is also plausible that passive owners collect relatively complex data like SEC filings and supplement these data with occasional quick web searches. I use the Google Trends database to determine if the frequency of Google web searches for firms' ticker symbols increases with passive ownership. Google Trends is a website produced by Google that analyses the popularity of web queries made through Google Search. For any given query, Google Trends returns the Google Search Volume Index (SVI), the query's search volume history, which is scaled by its time-series maximum.

[16] *EVIEWS* includes views of all SEC filings. Loughran and McDonald (2017) report that although there are 609 unique SEC form types, over 45% of filings in EDGAR are Form 4s and periodic reports (e.g., Form 10K/Q).

Da, Engleberg, and Gao (2011) examine the SVI of searches for ticker symbols of stocks in the Russell 3000 from 2004 -2008. Using retail order execution from SEC Rule 11Ac1-5 reports, they find a strong and direct link between changes in SVI and trading by retail investors. They show a positive time-series correlation between SVI and other measures of retail investor attention: extreme returns, turnover, and news-based measures of investor sentiment. Further, they show abnormal SVI predicts stock price increases over the subsequent two weeks, which are followed by reversals. While Da et al. (2011) show that Google searches for firms' tickers are more likely to come from individuals than institutions, there is little reason to expect that changes in individual investor behavior is correlated with variation in passive ownership.

One limitation of the SVI data is that each observation reported by Google is constructed using a representative sample of searches for a term in a given region. Thus, multiple requests for a given SVI over a given time frame in a given region could return slightly different results. Google SVI is available at daily or monthly frequencies, and range in value from zero to 100, with 100 representing the time-series maximum. I obtain each sample firm's monthly SVI over the period beginning on January 2007 and ending on December 2017, which is the range over which each firm's time-series maximum is calculated. $GVIEWS_{i,m}$ is each sample firm i's monthly SVI, which I retain each month m from the July after the Russell reconstitution until the following April. I present descriptive statistics in Table 11. The average $GVIEWS$ in July after reconstitution is 42.4 with a standard deviation of 23.9, and the monthly average $GVIEWS$ across July$_t$ to April$_{t+1}$ is 42.8 with a standard deviation of 22.3.

3. Results

3.1 Main Analyses

In my main analyses, I use Russell 2000 membership as a proxy for passive ownership. Because the Russell group implemented banding to reduce index switching among banded firms, I impute the bands based on Russell's published guide and I control for whether firms are predicted to be sorted into the imputed upper band (i.e., firms in the Russell 1000 in *t-1* that are predicted to remain in the Russell 1000 even though their imputed *CAPrank* is greater than 1000). Because *CAPrank* determines index membership, and because the value weighting of the Russell 2000 gives rise to greater passive ownership of more highly ranked firms, I also include controls for *CAPrank*. In order to allow the functional form of the regression results to vary on either side of the breakpoint between the two indices, I control for $CAPrank^2$ as well.

To exploit discontinuities in passive ownership around the breakpoint between the indices, I follow Wei and Young (2015) and Boone and White (2015) and limit the sample to +/- 200 firms around the breakpoint. I present descriptive statistics in Table 11. The sample contains 4,002 firms/year observations of firms that are near the breakpoint between the Russell indices from 2007 – 2016. Sample firms are similar in size by construction. The mean market capitalization (*CAP*) is $2.34 billion and its standard deviation is $760 million.

In order to verify Russell 2000 satisfies the relevance condition for its use as an instrument for passive ownership and to examine the impact of inclusion in the Russell 2000 on active and total fund ownership, I estimate the following OLS regression model to measure the impact of index assignment on fund ownership in December of reconstitution year *t*:

$$Y_{i,t} = \beta R2000_{i,t} + I\{Upperband_{i,t}\}\, x\, f(CAPrank_{i,t}) + I\{Upperband_{i,t}\}\, x\, \gamma_t$$

$$+I\{Upperband_{i,t}\} + f(CAPrank_{i,t}) + \gamma_t + \epsilon_{i,t}, \tag{2}$$

where the dependent variables are $FundOwn_{i,t}$, $FundOwn_{i,t}^{ACTIVE}$, and $FundOwn_{i,t}^{PASSIVE}$. $R2000_{i,t}$ is equal to one if stock *i* is assigned to the Russell 2000 in year *t*, and equal to zero if it is assigned to the Russell 1000. $Upperband_{i,t}$ is an indicator variable which is equal to 1 if firm *i* is within +/- 100 ranks of the imputed upper band and 0 otherwise. $f(CAPrank_{i,t})$ controls for differences in the functional form of the data. It is equal to $CAPrank_{i,t}$ and $CAPrank_{i,t}^2$ and allows the slope of the regression to differ on each side of the threshold. $Upperband_{i,t}$ controls for the potential differences in outcomes that result from banding, and it is interacted with γ_t, which are year fixed effects. In addition, I cluster standard errors at the firm level to account for the concern that residuals are serially correlated across firms. I verify that the relevance condition is satisfied (i.e., that passive ownership is greater for Russell 2000) constituents by estimating the regression equation described in Equation 2. I present the results of these tests in Table 12. The results indicate that relative to sample firms in the Russell 1000, no significant difference in active or total fund ownership exists among sample firms in the Russell 2000. Importantly, the estimated coefficient on $R2000$ presented in Column 3 is 2.76, which demonstrates that firms in the Russell 2000 exhibit over 74% (= 2.76/3.71) greater passive ownership relative to the mean level of passive ownership among sample firms, which is 3.71%. As the standard deviation of $FundOwn^{Passive}$ is 2.5%, the increase in the proportion of firms' shares owned by passive funds is greater for firms in the Russell 2000 by an amount which is equal to 1.1 (=2.76/2.5) standard deviations from the mean $FundOwn^{Passive}$.

To examine the impact of passive ownership on information acquisition via EDGAR, I regress EDGAR information acquisition measures on passive ownership using the model described in Equation 2. The dependent variables $EVIEWS_{m+1}$, $EVIEWS_{m+1,m+3}$, $EVIEWS_{m+1,m+6}$, and $EVIEWS_{m+1,m+10}$ in Columns 1, 2, 3, and 4 are calculated in the month of July after each reconstitution in year t, July$_t$ – September$_t$, July$_t$ – December$_t$, and July$_t$ – April$_{t+1,}$ respectively. I present the results of these tests in Table 13. The estimated coefficient on $R2000$ in Column 1 is -117.29 with a t-statistic of -7.26, which indicates that on average, passive ownership causes 117.29 fewer views of firms' information in the EDGAR website in the month of July. The average number of views of each sample firms' information in EDGAR in the month of July is 446.5. Taken together, this indicates that passive ownership causes a greater than 26% (=117.29/446.5) reduction in the information acquisition via EDGAR in the month of July. That a 2.76% increase in passive ownership gives rise to a 26% reduction in information acquisition may seem implausible at first blush. However, the standard deviation of $FundOwn^{Passive}$ is 2.46%. Therefore, Russell 2000 membership is associated with an increase in shares owned by passive institutions of 1.1 (=2.76/2.46) standard deviations from the mean. Given that the standard deviation of $EVIEWS_{m+1}$ is 402.2, this result demonstrates that as $FundOwn^{Passive}$ increases by about 1.1 standard deviations with Russell 2000 membership, $EVIEWS_{m+1}$ decreases by only 0.29 (=-117.29/402.2) standard deviations. In comparison, Appel et al. (2016) use Russell 2000 membership as a proxy for passive ownership and find that a one standard deviation increase in passive ownership is associated with a 0.65 – 0.76 standard deviation increase in board independence. The estimated coefficients on $R2000$ presented in Columns 2, 3, and 4 are all negative, similar in magnitude, statistically significant, and equal to -109.83, -105.93, and –109.34, respectively. The average number of monthly views of passively-owned firms' information is consistently about 23% – 26% lower over the 10-month period following the reconstitution of the Russell indices.

In my next set of analyses, I examine whether passive ownership gives rise to changes in the acquisition of information via Google. $GVIEWS$ are provided at a monthly frequency, so $GVIEWS_{i,m+1}$ is equal to $GVIEWS$ in the month of July, $GVIEWS_{i,m+1,m+6}$ is the average firm-level $GVIEWS$ over July – September, $GVIEWS_{i,m+1,m+6}$ is the average over July – December, and $GVIEWS_{i,m+1,m+10}$ is the average $GVIEWS$ over July$_t$ – April$_{t+1}$. I regress the various $GVIEWS$ measures on $R2000$ using the model described in Equation 2. The estimated coefficient on $R2000$ in Column 1 is -3.64 with a t-statistic of -2.63, which indicates that on average, passive ownership causes a statistically significant 8.59% (=3.64/42.39) reduction in the July $GVIEWS$, relative to the sample mean. As the standard deviation of $GVIEWS_{m+1}$is 23.88, this is a decrease of about 0.15 (=3.64/23.88) standard deviations. The estimated coefficient on $R2000$ in Column 4 is -3.57, which indicates that the estimated number of monthly Google searches for ticker symbols of firms in the high passive ownership group over the period beginning in July after the reconstitution of the Russell indices and ending in the following April is about 8.3% (=3.57/42.84) lower than the average number of monthly searches for funds with exogenously lower levels of passive ownership. This is a reduction in average $GVIEWS$ of 0.16 (=3.75/22.3) standard deviations from the mean.

3.2. Cross-Sectional Analyses

The prior results demonstrate that passive ownership causes a reduction in the acquisition via EDGAR and Google. This is consistent with the view that on average, the costs to passive owners of monitoring firms in their portfolios outweigh the expected benefits. These expected monitoring benefits may not be evenly distributed across passive funds' portfolio firms, however.

It could be the case the passive fund managers allocate resources to acquiring and analyzing the information of firms for which expected value improvements from monitoring are greatest. Because the acquisition of information reduces information asymmetry, the expected benefits of monitoring could be greatest for firms with greater information asymmetry.

In cross-sectional analyses, I examine whether the reduction in the acquisition of information caused by passive ownership varies with firm transparency. I construct three measures of opacity, sort firms into above and below median opacity groups, and interact dummy variables which are equal to one if a firm is sorted into the high-opacity group with $R2000$ in order to determine whether passive owners acquire more information from EDGAR on firms with these characteristics. A number of studies provide evidence that managerial discretion with regard to the reporting of accruals can provide managers with personal benefits related to their bonus contracts ((Healy (1985); Armstrong, Jagolinzer, and Larker (2010)) which, if misused, could be detrimental to other shareholders. Therefore, the first measure of opacity that I use is the absolute value of discretionary accruals estimated following the method proposed Kothari, Leone, and Wasley (2005), and measured in year *t-1*. $High\ Accruals_{i,t,}$ is equal to one if a firm's level of discretionary accruals is greater than the sample median of 5.028, and zero otherwise.

Small firms' shares are less liquid than larger firms' shares and they exhibit greater liquidity risk (Pastor and Stambaugh (2003)), but transparency increases liquidity (Diamond and Verrecchia (1991)). Because passive owners are contractually obligated to hold portfolios that mimic the index to which they are benchmarked, they must rebalance their portfolios in order to maintain index weights. They may therefore have preferences for more liquid stocks, which could incentivize them to acquire and analyze information on smaller firms in their portfolios. In order to examine whether the reduction in information acquisition caused by passive ownership differs with firm size, I construct a measure each sample firms' size as the log of its assets reported in Compustat in its last fiscal quarter before the reconstitution of the Russell indices. Because small firms exhibit more information asymmetry, I set $Small\ Size_{i,t}$ equal to one if a firm's log of assets is below the sample median, and zero otherwise.

Chen Harford, and Lin (2015) demonstrate that analysts improve firm governance: they find that after firms experience decreases in analyst coverage due to broker closures and mergers, CEO pay and earnings management increases, firms make worse acquisitions, and shareholders respond by devaluing their cash holdings. It follows that to the extent that analysts reduce information asymmetry and serve as monitors, lower analyst coverage results in increased benefits to shareholders of doing so. I calculate the number of analysts following each sample firm in the month of May before each reconstitution in year t, and set $Low\ Analysts_{i,t}$ equal to one if a firm has a lower analyst following than the sample median, and zero otherwise.

In this set of tests, I estimate the impact of passive ownership and opacity on $EVIEWS_{i,m+1}$ and $EVIEWS_{i,m+1,m+10}$. As before, I include the same fixed effects described in Equation 2 and I cluster standard errors at the firm level. I present the results of these analyses in Table 16. In each cross-section, the estimated coefficients of the interaction variables indicate that passive ownership increases the acquisition of opaque firms' information. In each model, the estimated coefficients on $R2000$ are negative, statistically significant, and greater in magnitude than the coefficients on the interaction terms, which demonstrates that passive ownership causes net reductions in attention regardless of the degree to which firms exhibit opacity. In addition, the estimated coefficients on each of the high opacity interaction terms are statistically significant. The results indicate that the reduction in information acquisition caused by passive ownership is not as great for more opaque firms. The estimated coefficient on $R2000 *\ High\ Accruals$ in Column 1 is equal to 44.47 with a t-statistic of 1.18, which taken together with the coefficient on *R2000* of -153.2 indicates that while passive ownership reduces attention firms with low accruals by about 153.2 views in the July after reconstitution, it only reduces attention to firms with high accruals by about 108.73 (=153.2-44.47) views on average. The results presented in Colum 4 demonstrate that relative to firms with greater analyst following, the average reduction in views over months m+1 to m+10 caused by passive ownership decreases by an average of 53.94 views for firms in the $Low\ Analyst$ group. Finally, the coefficient on $R2000 * Small\ Size$ in Column 6 is 44.79 with a t-statistic of -5.69, indicating that the decrease in the number of views in the month of July associated with passive ownership is attenuated for smaller firms. The average decrease in the number of large firms' views caused by passive ownership in that period is 131.45. Therefore, the reduction in the average number of views in that period caused by passive ownership is over 34% less (=44.79/-131.45) pronounced for small firms than for large firms. Overall, these results are consistent with the view that while passive owners' low-cost business models may inhibit them from acquiring information on firms in their portfolios, they may focus their information acquisition efforts on the least transparent firms for which monitoring may provide the greatest benefit.

3.3. Two-Stage Least Squares Analyses

The primary challenge in examining the impact of passive ownership on information acquisition is that passive ownership is endogenous. Firm characteristics like size and liquidity jointly impact passive ownership and information acquisition. The IV specification that I employ addresses the potential reverse causality or omitted variable bias. In order to estimate the treatment effect in an alternate manner, however, I conduct a conduct two-stage least squares (2SLS) analysis in which I regress $FundOwn^{Passive}$ on $R2000$ in the first stage. I then regress attention measures on fitted values of $FundOwn^{Passive}$ in the second stage. I include all controls described in Equation 2 and I cluster standard errors at the firm level. The dependent variables in Columns 1, 2, 3, and 4 are $EVIEWS_{i,m+1}$, $EVIEWS_{i,m+1,m+10}$, $GVIEWS_{i,m+1}$, and $GVIEWS_{i,m+1,m+10}$, respectively. In the first stage, the estimated coefficient on $R2000$ (untabulated) is 2.76 with a t-statistic of 31.12.[17] The Sanderson-Windmeijer multivariate F test of excluded instruments is 968.41, which is far greater than the generally-accepted threshold of 10 proposed by Stock and Yogo (2005). Thus, I reject the null that the instrument is weak. I present second-stage results in Table 16. The results of these tests provide further evidence that passive ownership decreases information acquisition. The coefficients on $FundOwn^{Passive}$ reported in Columns 1 and 2 are statistically significant at the 1% confidence level and equal to -42.53 and -38.42, respectively, which demonstrates that the acquisition of information via EDGAR decreases for passively-owned firms. Conceptually, the coefficients on $FundOwn^{Passive}$ indicate the correlation between the percentage of firms' market capitalization that is explained by Russell 2000 membership and the dependent variables. 2SLS coefficients estimate a local average treatment effect. Therefore, the estimated coefficient of -42.53 on $FundOwn^{Passive}$ indicates that a 1% change in the part of passive ownership that is explained by membership in the Russell 2000 gives rise 42.53 fewer views in the month of July after reconstitution. Similarly, the estimated coefficient on $FundOwn^{Passive}$ of -1.34 in Column 4 indicates that a 1% increase in the part of passive ownership that is explained by membership in the Russell 2000 gives rise to an average decrease in monthly $GVIEWS$ of 1.34 over the 10 months after the reconstitution of the Russell indices. As the standard deviation of $GVIEWS_{m+1,m+10}$ is 22.3, this amounts to a 0.06 standard deviation decrease relative to the mean.

[17] These first-stage results are for the first two models in which attention measures are constructed using the data from the EDGAR log files. The results of first stage estimations in which the dependent variables are constructed using Google data are qualitatively similar, but slightly different because of differences in degrees of freedom that arise from different *N*.

4. Conclusion

Passive owners are limited in their ability to exit positions in poorly performing or poorly managed firms. A number of recent studies suggest that passive owners are incentivized to monitor by voice because of exit limitations. Given that passively managed funds charge their investors lower fees and expenses than their actively managed counterparts do, monitoring may be too costly for passive funds to undertake. In addition, passive owners face a free rider problem because value enhancements associated with monitoring would accrue to other passive funds that track the same benchmark, even if these other funds do not share in the monitoring costs. Monitoring of any form requires that owners first gather information to determine if intervention is warranted. In this study, I examine whether passive ownership causes changes in information acquisition information. I use discontinuities in passive ownership around the threshold between the Russell indices to identify passive ownership, and I measure firm-level information acquired via Google and the SEC's EDGAR website. I find that on average, passive ownership decreases information acquisition. Evidence in the cross-sections suggests that passive owners are not completely passive monitors, however. Exogenous increases in passive ownership do not decrease the acquisition of highly opaque firms' information as much as they do for more transparent firms. These findings are consistent with the view that passive owners may gather more information on the least transparent firms in their portfolios for which potential value enhancements from monitoring are greatest.

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Table 11
Descriptive Statistics for Sample of Russell Constituents Around the Threshold

This table presents descriptive statistics. The sample consists of firms that are within +/- 200 ranks of the breakpoint between the Russell 1000 and Russell 2000 in year *t*. CAP is the imputed end-of-May market capitalization, and $\mathrm{FundOwn}_t^{Active}$, $\mathrm{FundOwn}_t^{Passive}$ and $\mathrm{FundOwn}_t$ are the percent of sample firms' CAP owned by active, passive or all funds, respectively. EVIEWS is the average monthly number of views of information from the EDGAR website, GVIEWS is the average monthly SVI score, Accruals are the absolute value of discretionary accruals in year t-1, Analysts is the number of analysts following a firm in May prior to reconstitution, and Size is the log of asset reported in the quarterly report just prior to reconstitution. All continuous variables are winsorized at the 1st and 99th percentiles.

	N	Mean	Std. Dev.	p10	p50	p90
CAP ($B)	4,002	2.342	0.764	1.424	2.272	3.386
$\mathrm{FundOwn}_t$ (%)	4,002	40.22	17.72	11.91	43.71	59.57
$\mathrm{FundOwn}_t^{\mathrm{Active}}$ (%)	4,002	36.51	16.62	10.16	39.53	54.78
$\mathrm{FundOwn}_t^{\mathrm{Passive}}$ (%)	4,002	3.712	2.46	0.151	3.377	6.738
EVIEWS_{m+1}	4,001	446.5	402.2	104	333	888
$\mathrm{EVIEWS}_{m+1,m+3}$	4,001	440	367.9	115.3	339.3	876.3
$\mathrm{EVIEWS}_{m+1,m+6}$	4,001	447.5	365.8	119.3	346.3	896.3
$\mathrm{EVIEWS}_{m+1,m+10}$	4,001	467.4	378.3	133.7	363.3	909.5
GVIEWS_{m+1}	4,002	42.39	23.88	11	42	75
$\mathrm{GVIEWS}_{m+1,m+3}$	4,002	42.59	22.73	12.33	42.33	73.67
$\mathrm{GVIEWS}_{m+1,m+6}$	4,002	42.45	22.3	12.83	42.17	73
$\mathrm{GVIEWS}_{m+1,m+10}$	4,002	42.84	22.27	13.1	42.6	73.2
Accruals (%)	3,003	7.573	8.246	0.745	5.028	16.72
Analysts	3,899	9.425	5.515	3	8	17
Size	4,000	7.87	1.023	6.574	7.843	9.277

Table 12
Fund Ownership

This table presents results of regressions of fund ownership measures on R2000, a dummy variable equal to one if a firm in the Russell 2000 in reconstitution year *t*. FundOwn, $FundOwn^{ACTIVE}$, and $FundOwn^{PASSIVE}$ are the percent total, active and passive fund ownership in December of reconstitution year *t*. Standard errors are clustered at the firm level. All continuous variables are winsorized at the 1st and 99th percentiles. t-statistics are reported in parentheses.

	FundOwn	$FundOwn^{ACTIVE}$	$FundOwn^{PASSIVE}$
	(1)	(2)	(3)
R2000	1.569	-1.190	2.759***
	(1.55)	(-1.25)	(31.14)
Observations	4,002	4,002	4,002
Adj. R-Squared	0.007	0.005	0.547
Band x Year FE	Yes	Yes	Yes
Band x CAPrank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
CAPrank FE	Yes	Yes	Yes
Ctrl Fn Deg	2	2	2

Table 13
Impact of Passive Ownership on the Acquisition of EDGAR Data

This table presents results of regressions of EDGAR information acquisition measures on R2000, a dummy variable which is equal to one if a firm is in the Russell 2000 and zero otherwise. Dependent variables in columns 1,2,3 and 4 are the monthly number of views of firm information in EDGAR (EVIEWS) in the month of July, as well as the average number of views from July – September, July – December, and July – April, respectively. Standard errors are clustered at the firm level. All continuous variables are winsorized at the 1st and 99th percentiles. t-statistics are reported in parentheses

	$EVIEWS_{m+1}$	$EVIEWS_{m+1,m+4}$	$EVIEWS_{m+1,m+7}$	$EVIEWS_{m+1,m+10}$
	(1)	(2)	(3)	(4)
R2000	-117.289***	-109.832***	-105.933***	-109.339***
	(-7.26)	(-7.07)	(-6.70)	(-6.71)
Observations	4,001	4,001	4,001	4,001
Adj. R-Squared	0.386	0.374	0.366	0.366
Band x Year FE	Yes	Yes	Yes	Yes
Band x CAPrank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
CAPrank FE	Yes	Yes	Yes	Yes
Ctrl Fn Deg	2	2	2	2

Table 14
Impact of Passive Ownership on the Acquisition of Google Data

This table presents results of regressions of Google information acquisition measures on R2000, a dummy variable which is equal to one if a firm is in the Russell 2000 and zero otherwise. Dependent variables in columns 1,2,3 and 4 are the SVI in the month of July, as well as the monthly average SVI number over July – September, July – December, and July – April, respectively. Standard errors are clustered at the firm level. All continuous variables are winsorized at the 1st and 99th percentiles. t-statistics are reported in parentheses.

	$GVIEWS_{m+1}$	$GVIEWS_{m+1,m+4}$	$GVIEWS_{m+1,m+7}$	$GVIEWS_{m+1,m+10}$
	(1)	(2)	(3)	(4)
R2000	-3.635***	-3.880***	-3.707***	-3.568***
	(-2.63)	(-2.84)	(-2.74)	(-2.60)
Observations	4,002	4,002	4,002	4,002
Adj. R-Squared	0.010	0.008	0.008	0.008
Band x Year FE	Yes	Yes	Yes	Yes
Band x CAPrank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
CAPrank FE	Yes	Yes	Yes	Yes
Ctrl Fn Deg	2	2	2	2

Table 15
Impact of Passive Ownership on the Acquisition of Opaque Firms' Information

This table presents results of regressions of EDGAR information acquisition measures on R2000, a dummy variable which is equal to one if a firm is in the Russell 2000, which are interacted with three measures of opacity: High Accruals is equal to 1 if a firm's discretionary accruals in *t-1* are greater than the sample median, and zero otherwise. Low Analysts is equal to one if the number of analysts following a firm in May$_t$ is lower than the sample mean, and equal to zero otherwise. Small Size is equal to one if the log of firm's log of assets, measured at the quarterly report date prior to reconstitution, is less than the sample median, and zero otherwise. Dependent variables in columns 1, 3 and 5 are the average monthly views in the month of July. Dependent variables in columns 2, 4, and 6 are average monthly views over July – September. Standard errors are clustered at the firm level. All continuous variables are winsorized at the 1st and 99th percentiles. t-statistics are reported in parentheses.

	EVIEWSm+1 (1)	EVIEWSm+1, m+10 (2)	EVIEWSm+1 (3)	EVIEWSm+1, m+10 (4)	EVIEWSm+1 (5)	EVIEWSm+1, m+10 (6)
R2000	-153.204***	-145.259***	-134.570***	-123.034***	-138.646***	-131.449***
	(-6.68)	(-6.48)	(-6.28)	(-5.71)	(-5.94)	(-5.69)
High Accruals	-7.323	-1.946				
	(-0.36)	(-0.10)				
R2000 * High Accruals	44.472*	42.289*				
	(1.81)	(1.79)				
Low Analysts	-7.323	-1.946	-101.137***	-107.261***		
			(-4.36)	(-4.79)		
R2000 * Low Analysts			53.475**	53.940**		
			(2.04)	(2.08)		
Small Size					-49.601*	-44.785*
					(-1.90)	(-1.75)
R2000 * Small Size					54.917*	59.773**
					(1.83)	(2.00)
Observations	3,002	3,002	3,898	3,898	3,999	3,999
Adj. R-Squared	0.403	0.391	0.400	0.379	0.387	0.368
Band x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Band x CAPrank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
CAPrank FE	Yes	Yes	Yes	Yes	Yes	Yes
Ctrl Fn Deg	2	2	2	2	2	2

Table 16
Impact of Passive Ownership on the Acquisition of EDGAR Data - 2SLS

This table reports estimates of second-stage regressions of measures of information acquired via EDGAR (in Columns 1 and 2) and Google (in Columns 3 and 4) on fitted values of $FundOwn^{Passive}$, the percentage of firms' shares outstanding which are owned by passive funds. Standard errors are clustered at the firm level. All continuous variables are winsorized at the first and 99th percentiles. t-statistics are reported in parentheses.

	$EVIEWS_{m+1}$	$EVIEWS_{m+1,m+10}$	$GVIEWS_{m+1}$	$GVIEWS_{m+1,m+10}$
	(1)	(2)	(3)	(4)
$\mathrm{FundOwn}^{Passive}$	-42.534***	-38.416***	-1.318***	-1.343***
	(-7.23)	(-6.71)	(-2.62)	(-2.72)
Observations	4,001	4,001	4,001	4,001
Adj. R-Squared	0.389	0.369	-0.003	-0.007
Band x Year FE	Yes	Yes	Yes	Yes
Band x CAPrank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
CAPrank FE	Yes	Yes	Yes	Yes
Ctrl Fn Deg	2	2	2	2

CONCLUSION

In this dissertation, I examine the relation between capital market participants' attention to fundamental information and capital market outcomes. In the first essay, I examine whether investors' attention to fundamental information helps predict merger and acquisition activity. As firms are often targeted because of an attractive fundamental position, I test whether investors' attention to fundamental information acquired via the SEC's EDGAR website predicts subsequent takeover activity. Following an acquisition announcement, EDGAR download activity helps predict subsequent takeover bids for peer firms in the same industry. This effect is especially strong among firms with low market-to-book ratios and those with low prior returns, supporting the view that M&A announcements incite interest in undervalued firms in a competitive takeover market. Attention to fundamentals is a better predictor of future bids in peers with high announcement period returns, suggesting that the market impounds into prices information related to EDGAR download activity. In addition, fundamental information gathering predicts increased incidence and hazard rate of deal completion.

In the second essay, I examine the impact of passive ownership on the acquisition of fundamental information via the Security and Exchange Commission's EDGAR website and from Google searches. I identify variation in passive ownership that is uncorrelated with underlying firm fundamentals by exploiting discontinuities in passive ownership among firms in the Russell indices. On average, I find that passive ownership causes information acquisition to decrease. These results are consistent with the view that the costs to passive owners of acquiring and analyzing portfolio firm information exceed the expected benefits of monitoring. Notwithstanding, cross-sectional analyses support the view that passive owners are not passive monitors. I find that decreases in information acquisition associated with passive ownership are not as large for the least transparent firms.

VITA

C. Asa Lambert was born and raised in Macon, Georgia. He earned a Bachelor of Arts degree with a major in Psychology and a minor in Philosophy at Mercer University in 2004. After completing his undergraduate degree, Asa worked as a FINRA Series 6 and 63-licensed financial specialist at Wachovia Bank, a credit analyst at Atlantic Southern Bank, and a credit counselor at Consumer Credit Counseling Services of Middle Georgia. Prior to commencing doctoral studies at the University of Tennessee, he earned a Master of Business Administration at Georgia College and a Master of Science in Finance at Florida State University. While at the University of Tennessee, Asa served as president of the Haslam College of Business's Doctoral Student Association and he earned the College's Outstanding Graduate Student Teacher and Doctoral Leadership awards. Asa earned his Ph.D. in Business Administration with a concentration in Finance at the University of Tennessee in August of 2020, the same month in which he and his lovely wife Jessica expect the birth of their first child, a son.