

Essays in Finance:
Spillovers, Heterogeneity, and Market Consequences
of Investor Attention

Dissertation zur Erlangung des akademischen Grades
Doctor rerum politicarum
der Fakultät für Wirtschaftswissenschaften
der Technischen Universität Dortmund

Kevin Rene Lehmann

July 12, 2026

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Acknowledgments

This dissertation marks the end of a journey that began as a utopia and has now become reality. I still remember vividly the first time I sat in a lecture hall, and how early on, the thought of one day pursuing a PhD first crossed my mind. That I am now able to fulfill this dream with the present dissertation, I owe to a great many people who supported me along the way.

I want to express my heartfelt gratitude to my doctoral supervisor, Peter N. Posch, for giving me the opportunity to conduct my research as an external PhD candidate. I am deeply thankful for all the support and guidance he provided, the many fruitful discussions, and his continuous encouragement throughout this journey. I want to thank Gerrit for paving my way back into academia, and for the sparring and support he offered me along the way. I also want to thank my fellow PhD colleagues Cathrin, Kira, Selin, Alex, and Patrick for the discussions, their support and company, as well as for activities beyond research, such as our tour through Naples, which I will fondly remember.

Beyond that, I want to thank my friends for their support. I am especially grateful to Fabienne and Sarah, who believed in me even more than I sometimes believed in myself. To Alina, my love, thank you for walking every step of this path with me. Your patience, your support, and your unwavering love carried me toward this goal. Last but not least, none of this would have been possible without my family. Mum, Dad, Conny, and Jürgen, thank you for everything you have given me over the years. It laid the foundation for this journey long before I knew where it would lead.

1 Introduction

Market participants are continuously exposed to a flood of information, and access to that information has become ever easier with advancing digitalization and globalization. Following the efficient market theory, new public information should be incorporated into prices without any delay. A large body of literature on behavioral and information economics examines the frictions that arise when information must be acquired and processed by investors. Among these frictions, limited attention is central, because attention is a scarce cognitive resource and investors cannot process all available information at once (Hirshleifer and Teoh, 2003; Peng and Xiong, 2006). Where investors direct their limited attention is therefore of direct economic consequence, as attention governs how quickly and how completely information is incorporated into prices. Consistent with this, prior work shows that even short-lived attention surges lasting only a few days can move prices and volatility in economically meaningful ways (Ballinari et al., 2022; Yang et al., 2017; Han et al., 2018).

This dissertation studies investor attention through a common empirical lens. We use the EDGAR log files, which document search traffic on the SEC’s filing platform, as our central proxy for institutional investor attention. Compared with broader measures such as Google search volume, which is believed to reflect retail search patterns and to carry more noise (Da et al., 2011), the EDGAR log files provide a direct record of the filings investors actively seek out, and the measure is widely used and thought to better capture the information-acquisition behavior of institutional and otherwise sophisticated investors (Lee et al., 2015; Gibbons et al., 2021). Prior studies validate EDGAR search traffic as a proxy for investor attention. Investors rely on EDGAR filings for information acquisition (Loughran and McDonald, 2017), this acquisition has measurable market consequences (Drake et al., 2015), and search traffic maps directly onto expected returns (Li and Sun, 2022). We nonetheless acknowledge that our proxy captures a relative trend rather than an absolute level of attention. Investors do not access filings through EDGAR alone and may instead rely on third-party providers such as Bloomberg. We expect, however, that the pattern of what investors search for directly on EDGAR is representative of their broader information demand, so that the true levels are shifted rather than fundamentally different. Under this assumption, the attention surges we observe in the EDGAR log files correspond to information that

investors would also seek through other channels.

Prior literature shows that investors pay disproportionately more attention to geographically closer and more familiar firms, following the home bias literature (e.g., French and Poterba, 1991; Huberman, 2001), as well as to economically similar firms (Lee et al., 2015). Much of this work treats attention as static and homogeneous, explores the investor-to-firm relation, while other work measures attention through broader proxies such as media coverage or aggregated searches. Three gaps follow from this, and together they motivate the present dissertation. First, if attention concentrates on related firms, it need not simply rest there but may travel between them, yet the transmission of attention from one firm to another, as distinct from the proximity of investors to a single firm, has remained largely unexamined. Second, as attention is typically treated as homogeneous, the more granular question of what kind of information sets it in motion remains hidden. Third, when it comes to how attention shapes the pricing of a firm’s own events, existing work concentrates on the determinants or the long-run consequences of corporate events, rather than on the short-run, information-processing channel through which attention conditions the market’s immediate reaction.

Derived from these gaps, this dissertation can be divided into two research strands. The first concerns the transmission of attention across firms. If attention is scarce and clusters around related firms, we examine whether an abnormal attention event at one firm draws attention toward geographically and economically proximate peers, and what governs the strength and reach of that spillover. The second concerns the consequences of attention for how markets absorb a firm’s own news, such as the appointment of a new director. Because price adjustments are expected to be slower and weaker when an event receives less attention (Hirshleifer and Teoh, 2003; Peng and Xiong, 2006), the amount of attention an event attracts should itself shape the market’s reaction. These two strands map onto the structure of this dissertation and onto its title. The first two papers study the spillovers of attention across firms and the heterogeneity of those spillovers, while the third studies the market consequences of attention for a firm’s own corporate events.

The first paper asks whether attention to one firm spills over to geographically and economically proximate firms. Methodologically, it combines EDGAR search traffic from 2003 to 2017 with firm geolocation and text-based product-similarity scores from the TNIC dataset (Hoberg and Phillips, 2010, 2016). We build a firm-pairwise panel in which each firm that experiences an abnormal attention shock, defined relative to its own 90-day average attention baseline, is paired with every other listed firm at the respective point in time. The identification comes from comparing these firm pairs to

the same pairs in control windows before and after the shock. We estimate both a regression model using a continuous distance measure and a model leveraging discrete distance bins. Firms receive significantly more attention when nearby firms are shocked. The effect decays with distance and over a horizon of about three days. Geography alone yields an economically small effect of around one percent, but adding product-market similarity reveals much larger effects, with attention up to fifty percent higher for firms that are both close and economically similar. This paper demonstrates the existence of firm-to-firm attention spillovers.

The second paper asks whether these spillovers differ systematically across SEC filing types. Methodologically, it carries over the firm-pairwise, shock-based spillover design of the first paper but refines the attention measure itself, decomposing search traffic into filing-type-specific attention. We decompose our attention measure into three distinct measures. These attention measures target regular filings (10-K and 10-Q), insider filings (Forms 3 to 5), and event-driven 8-K filings, so that shocks and their spillovers can be traced separately for each type of disclosure. Whereas the first paper treats EDGAR activity as a homogeneous information source, this decomposition allows attention networks to be identified filing type by filing type. Insider filings generate the strongest and most localized spillovers, raising peer-firm attention by more than two hundred percent among firms that are both economically similar and geographically close. Regular filings produce more moderate spillovers that reallocate attention mainly toward product-market peers. In contrast, 8-K filings generate broad spillovers that are not geographically confined. Additionally, an item-level analysis of the SEC's top-level 8-K categories shows that this non-localized pattern is driven almost entirely by discretionary, signaling-type items, namely Item 7 (Regulation FD) and Item 8 (Other Events), while routine mandatory items (Items 1 to 6) produce no comparable effect. This item-level decomposition moves beyond the filing type to identify precisely which kinds of disclosed information make attention diffuse widely rather than locally, thereby revealing attention networks that remain invisible in aggregate EDGAR data.

The third paper turns from the transmission of attention between firms to the consequences of attention for how the market absorbs a firm's own corporate events. We examine whether the gender of a newly appointed director affects the attention an appointment attracts, and whether attention conditions the market's reaction to it. Methodologically, it departs from the firm-pairwise spillover design of the first two papers and instead adopts an event-study approach around the appointment announcement. Linking U.S. board appointments from BoardEx to EDGAR log files and to Compustat data, it uses a two-stage design. The first stage focuses on the attention response to the announcement. In the second stage we examine abnormal

returns and abnormal trading volume conditional on that attention. Thereby, attention enters as a moderator rather than as an outcome transmitted between firms. Female appointments generate a significantly smaller attention surge than comparable male appointments, with the gap concentrated in supervisory roles but reversing for executive and CEO appointments. Higher attention mitigates the return penalty associated with female appointments while simultaneously amplifying the gender gap in abnormal trading volume, an asymmetric moderating role that separates price adjustment from trading intensity. To our knowledge, this is the first study to link EDGAR-based attention around board appointments directly to director gender and to treat attention as a moderator of gendered market reactions.

Taken together, this dissertation makes three connected contributions. The first paper introduces a new approach that brings firm geography and text-based product-market similarity together in the analysis of EDGAR search traffic, and uses this combination to document firm-to-firm attention spillovers. Our central finding is that this spillover effect is jointly structured by geographic proximity and economic similarity, remaining economically negligible when only geography is considered but rising to as much as fifty percent higher peer attention once product-market similarity is added. The second paper breaks with the assumption that EDGAR-measured attention is a homogeneous information source. By decomposing EDGAR search traffic data into filing-type-specific attention, it shows that the magnitude and spatial reach of spillovers differ systematically across disclosure types. In particular, the item-level analysis of 8-K categories reveals heterogeneous spillover dynamics depending on the specific nature of the underlying information. These findings reveal attention networks that are invisible in aggregate data. Finally, the third paper links EDGAR-based attention around board appointments directly to director gender, using a targeted measure of regulatory information acquisition, and shifting the focus from the determinants and long-run consequences of board diversity to the short-run, information-processing channel that shapes market reactions at the moment of appointment. It contributes to the literature by showing that attention operates not as a uniform amplifier but as an asymmetric moderator, reducing the return penalty associated with female appointments while widening the gender gap in trading volume, and thereby separating price adjustment from trading intensity.

In summary, the three papers point to a common conclusion. Investor attention is a dynamic and unevenly allocated resource, whose distribution has consequences both across firms and for how a firm's own information is absorbed. However, all three papers rely on EDGAR search traffic, which, as noted, captures an institutionally tilted and relative measure of attention rather than the full universe of investor information

acquisition. A further avenue of research could be to complement our studies with data from other channels to explore how far the documented patterns generalize. Furthermore, our analysis is restricted to U.S. stock-listed firms and, due to the granularity of the historical log files, to a sample period ending in 2017. Future research could add to this by examining whether the same dynamics hold in other regulatory environments and in more recent, higher-frequency information regimes. The findings, taken as a whole, speak to ongoing debates on market efficiency and information diffusion, to the design and informational scope of corporate disclosures, and to the literature on gender and corporate governance.

The remainder of this dissertation proceeds as follows. Chapter 2 presents the first paper, establishing the existence of proximity-powered attention spillovers. Chapter 3 presents the second paper, documenting heterogeneity in these spillovers across SEC filing types. Chapter 4 presents the third paper, examining how investor attention conditions market reactions to female board appointments.

1.1 Publication Details

Paper I (Chapter 2):

Proximity-Powered Attention: Exploring Spatial Spillover in Investor Attention

Author:

Kevin Rene Lehmann and Peter N. Posch

Abstract:

This paper examines the transmission of investor attention between firms based on geographical proximity and product market similarity using EDGAR search traffic data. We find that firms receive more attention when a nearby firm experiences abnormal attention, with the effect decreasing with distance and over time. The effect is stronger for firms in similar product markets. Effects are most concentrate among nearby firms, with attention increases exceeding 90% for very similar markets, dropping to 41% as distance increases. This research extends the literature on investor attention and local information bias, highlighting the importance of peer firm relationships in information transmission.

Publication Details:

Journal of Behavioral and Experimental Finance (2025)

<https://doi.org/10.1016/j.jbef.2025.101099>

Note: The version of this article included in Chapter 2 is not identical to the version published in the Journal of Behavioral and Experimental Finance, but a version prior to the last revisions made during the review process. The published version reflects these revisions, including a rewritten introduction, additional clarifications throughout the text, a unified approach in aggregating estimates into reported effect sizes, and an additional robustness analysis. The underlying data, regression models, and coefficient estimates are identical across both versions. The published article is licensed under the CC BY 4.0 license (<https://creativecommons.org/licenses/by/4.0/>), © 2025 The Author(s), published by Elsevier B.V.

Paper II (Chapter 3):

How Filing-Type Heterogeneity Shapes Investor Attention Spillovers Across Geographic and Product-Market Networks

Author:

Kevin Rene Lehmann and Peter N. Posch

Abstract:

We study how different corporate disclosures generate investor attention spillovers across U.S. firms using EDGAR search data from 2007 to 2017. While prior work documents the existence of attention spillovers, we show that their magnitude and geographic reach depend on the informational characteristics of the underlying disclosure. Salient and surprising filings generate intense but localized spillovers, predictable disclosures limit spillover strength, and broad-scope information drives wider diffusion.

In particular, we demonstrate that attention spillovers are highly heterogeneous across filing types. Insider filings (Forms 3–5) trigger large and tightly localized spillovers, with attention to nearby, product-market-similar firms more than doubling. Regular filings (Forms 10-K and 10-Q) produce weaker spillovers concentrated among economically similar firms. In contrast, event-driven filings (Form 8-K) generate broad, non-localized spillovers, particularly when they contain discretionary information. Overall, our results show that attention spillovers vary systematically across filing types, with insider, regular, and event-driven disclosures exhibiting distinct patterns of diffusion across firms.

Publication Details:

Working Paper. Submitted to Global Finance Journal.

Paper III (Chapter 4):

How Investor Attention Conditions Market Reactions to Female Board Appointments

Author:

Kevin Rene Lehmann and Peter N. Posch

Abstract:

This study examines whether the gender of newly appointed board directors affects investor attention and stock market reactions. Using EDGAR log files and board appointments from U.S. listed firms between 2007 and 2017, we show that announcements of female directors generate approximately 8% less EDGAR-based investor attention than comparable male appointments. This attention gap is concentrated in supervisory roles but reverses for executive and especially for CEO positions. We further show that investor attention conditions market reactions in sharply different ways across prices and trading activity. High attention mitigates the negative return differential associated with female appointments, suggesting that greater information acquisition helps narrow the price-reaction gap. At the same time, high attention amplifies the gender gap in abnormal trading volume, indicating that attention does not simply scale market reactions uniformly but changes the composition or intensity of trading responses. This dichotomy suggests that attention is associated with more favorable price adjustment while simultaneously reinforcing differences in market participation. We interpret these patterns as conditional associations, not identified causal effects, since attention is likely endogenous to firm and event characteristics.

Publication Details:

Working Paper. Submitted to Journal of Corporate Finance.

2 Proximity-Powered Attention: Exploring Spatial Spillover in Investor Attention

The following is based on Lehmann and Posch (2025).

2.1 Introduction

In today's world, investors have a wealth of information at their fingertips, but their ability to process them is still limited. Consequently, there is a growing literature on investor attention and behaviour. As financial markets become more global and increasing amounts of information are easily available in seconds from anywhere in the world, one could assume that geography should have only a limited impact on investor behaviour. In fact, several studies have shown that geography plays a significant role in various dimensions.

Bernile et al. (2015) show that investor proximity to firm activities creates local information asymmetries, influencing institutional investors' portfolio compositions and leading to higher returns. Consistently, Dyer (2021) find that institutional investors acquire more information for their local investments than non-local ones. Also for retail investors, Mengoli et al. (2023) confirm that investors pay twice as much attention to local firms. Similar observations have been documented for scheduled events, such as earnings announcements. Chen et al. (2022), Chi and Shanthikumar (2017) as well as Hollander and Litjens (2022) find that a local information bias exists, leading to increasing trading activities prior to earning announcements, while non-local investors rely more on public information released on the day of the earning announcement. While the previous studies suggest that a local information bias leads to information asymmetry and lower market efficiency, Chen (2023) demonstrates that a greater geographical diversification of investor attention can imply a more efficient market response. Although numerous studies focus on the geographic proximity of investors to firms, little is known about diffusion of investor attention among firms.

Our study aims to close this gap and contribute to a more comprehensive understanding of the dynamics of investor attention by shifting the focus from investor-to-firm proximity to the proximity between firms themselves. In light of this, we propose the

following hypotheses:

Hypothesis 1: Firms experience an increase in investor attention when geographically proximate firms receive abnormal attention.

Hypothesis 2: Proximity-powered spillover effects of investor attention are more pronounced among firms that operate in similar product markets.

Our hypotheses are inspired by and derived from various observations. First, there are numerous studies that examine the effect of proximity on the correlation of stock prices. Pirinsky and Wang (2006), Barker and Loughran (2007) as well as Eckel et al. (2011) demonstrate in their research that with increasing distance the correlation of stock return is decreasing. Considering the weak form of the efficient market hypothesis, which states that stock prices already reflect all historical available information, we conclude that increased stock price correlation between firms in close proximity may indicate local attention spillover effects among geographically close firms. Local information may not be immediately apparent from past price movements and cannot be fully explained by efficient market theory.

Second, Engelberg et al. (2018) reveals that investors are more likely to hold investments within the same industry and geographical area in order to reduce the cost of information processing. Therefore, Engelberg et al. (2018) concludes that investors gather information about firm groups to leverage the information of one firm for another. In addition, they observed that firm fundamentals are more correlated for firms within the same group than outside the group.

Third, Lee et al. (2015) analysed the search traffic of EDGAR users and found that firms in the search history of the same individual are similar in multiple dimensions. Therefore, in the process of information acquisition, investors are searching for related firms to substantiate their investment decisions. However, geographical similarities were not yet taken into account. Shangguan et al. (2020) further confirm these findings by analysing co-searches on digital platforms such as online financial portals. Therefore, they construct networks where each firm is linked to others based on the frequency of co-searches. Shangguan et al. (2020) findings suggest that attention can spillover through co-search linkages among proximate firms, where "proximate" refers to the relative distance within a digital network.

Our research starts by defining a measure of investor attention. Although Google search traffic measures reflect more the attention behaviour of less sophisticated investors (Da et al., 2011), we use EDGAR search traffic as a proxy for investor attention. The

Security Exchange Commission (SEC) launched the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system in 1993. The main objective was to democratise access to corporate information for investors and reduce information asymmetry in the market (Gomez, 2024). Using EDGAR data as a proxy for investor attention is a common approach and is believed to represent more the behaviours of institutional investors (Lee et al., 2015; Li and Sun, 2022; Gibbons et al., 2021). Furthermore, we restrict our focus to stock-listed firms, and subsequently enrich our attention data with the geographical information of each firm. Based on our prepared dataset, we create a sample consisting of firms that received an abnormal attention increase compared to its 90-day moving average attention. For each shocked firm, we construct pairwise records with every firm that is stock listed at the respective time. Consequently, we add control group data for the same firm pairs before and after the respective attention shock. Finally, we enrich our dataset by various controls from accounting as well as EDGAR data.

After data preparation, we perform three regression analyses to explore spatial attention spillovers. First, we use a continuous distance measure to test whether spillover effects are generally present. Second, following Eckel et al. (2011), we modify our model specification by using discrete distance bins, not only to capture nonlinear effects but also to further uncover the structure of attention spillovers. Lastly, we also consider product similarity between firms as an additional dimension to account for further economic relationships among firms. The latter is motivated by Engelberg et al. (2018) and Lee et al. (2015), who demonstrated that investors tend to collect information for groups of firms. We expect that spillover effects vary if firms are not only in geographical proximity but also operating in closer product markets. Therefore, we are using the 10-K Text-based Network Industry Classifications (TNIC) dataset published by Hoberg and Phillips (2010, 2016). The TNIC dataset contains similarity scores for firms based on the textual similarity of their product descriptions in their 10-K filings. Using product similarity as a grouping criterion for firms is considered more dynamic than using traditional industry classifications, such as SIC or NAICS codes (Hoberg and Phillips, 2010, 2016).

Our results suggest that firms in close proximity benefit from the increase in abnormal attention of their neighbours. Following an abnormal increase in one firm, surrounding firms receive increased attention compared to their 90-day mean. The effect decreases over time and with increasing distance. Analysis of discrete distance bins reveals that the effect is non-linear and only significant for near-range distances. Taking into account only geography as the relationship across all stock listed firms, the effect is observed in the 1% range. This indicates that spillover effects generally exist, even if the economic relevance is low on average across the whole population. Increasing the granularity

of our data by adding product similarity as another dimension reveals much more pronounced effects that also have considerable economic relevance. Firms in direct proximity to those with abnormal increased attention and operating in similar product spaces receive up to 50% higher attention than firms farther away. We observe spillover effects lasting for up to three days. The economic relevance of these results is further supported by studies such as those of Ballinari et al. (2022), Yang et al. (2017) and Han et al. (2018), which show that increased investor attention over a few days can already have a significant impact on share price movements and volatility. Our findings suggest that the combination of proximity and economic relationships between firms plays a critical role in the dynamics of attention spillovers.

Previous studies have predominantly focused on the proximity of investors to firms, but have largely overlooked how spatial attention spillover among firms influences the market. Addressing this gap, our research contributes to the literature by introducing a novel approach that incorporates the geographical data of firms as well as product similarity between firms into the analysis of EDGAR search traffic data. Especially, considering product similarity enables a more nuanced understanding of the dynamics of investor attention. Our research builds on existing findings regarding investor information acquisition, further confirming the local information bias as previously documented by Bernile et al. (2015), Dyer (2021) and Hollander and Litjens (2022). Taking into account economic relationships, our results further underpin the findings of Lee et al. (2015) that investors search related firms as part of their information gathering. Moreover, our findings extend these insights by demonstrating that a firm’s geography in conjunction with product similarity serves as an additional factor influencing the similarity of investor co-searches. For firms, our findings might suggest to account for geographical proximity and product similarity in their communication towards investors. In addition, our findings can also contribute to the considerations of strategic alliances and partnerships to increase their own visibility and awareness. For investors, our findings offer new insight into portfolio diversification and anticipating market movements, as temporarily increasing investor attention can trigger pronounced market reactions.

The remainder of our paper is organised as follows. In Section 2, we describe the used data, sample construction, as well as our data processing and merging of the various datasets. Section 3 discusses the methodology chosen to assess the effects of spatial attention spillover. In Section 4, we discuss our empirical findings. Section 5 concludes.

2.2 Data

In our data preparation, we combine multiple sources of data. For this, we initially prepare three datasets. The first dataset covers our proxy for investor attention. The second dataset consists of stock listing information for all stock exchange-listed firms within the research horizon. The third dataset contains geographical information. Once each dataset is prepared, we combine them in Section 2.2.4 to construct our sample. Sections 2.2.5 and 2.2.6 then describe further enrichments to our sample dataset.

2.2.1 Attention Data

The starting point for our measurement of investor attention is the EDGAR log files provided by the Security Exchange Commission (SEC) on the EDGAR website of the SEC. Log files are available daily for the periods from January 1, 2003 to March 31, 2017¹ and from May 19, 2020 onwards. Both datasets contain detailed information about search traffic for EDGAR filings via the SEC platform. The first dataset provides granular information including IP address ranges, request date and time, a firm-specific CIK (SEC Central Index Key) identifier associated with the requested filing, accession number of the requested filing, request status, and more. In contrast, the second dataset offers only a reduced level of detail, as it only contains the request date and time, and the relative path of the requested asset. Due to the difference in data granularity, we use the first dataset from January 1, 2003 to March 31, 2017, which also gives us the flexibility to perform data cleaning and remove noise as, for example, incomplete requests or automated bot crawls, which do not reflect true investor attention.

Downloading all EDGAR log files results in a total of 24,235,626,503 records, where each record represents a request from an end user to EDGAR. Following Köchling et al. (2021), Lee et al. (2015) and Ryans (2017), we remove the following types of records:

1. All incomplete requests identified by the web server's log file status code not equal to 200, which is the code for successfully completed requests.
2. Requests that refer to the index page and not to a specific filing identified by the `idx` dummy variable that is included in the log files.
3. Requests from users who have self-identified as web crawlers using the `crawler` variable that is included in the log files.

¹According to the SEC website, the log files should be available until June 30, 2017, but the second quarter of 2017 is no longer available at the time of our research. Therefore, only data up to March 31, 2017 were used.

4. Requests associated with IP addresses which have accessed more than 500 filings in one day.
5. Requests associated with IP addresses that have requested more than 25 filings or for more than three companies within one minute.

After removing the records described above, 867,947,657 records remain. Figure 2.1 shows the resulting distribution of investor access to EDGAR for each year.

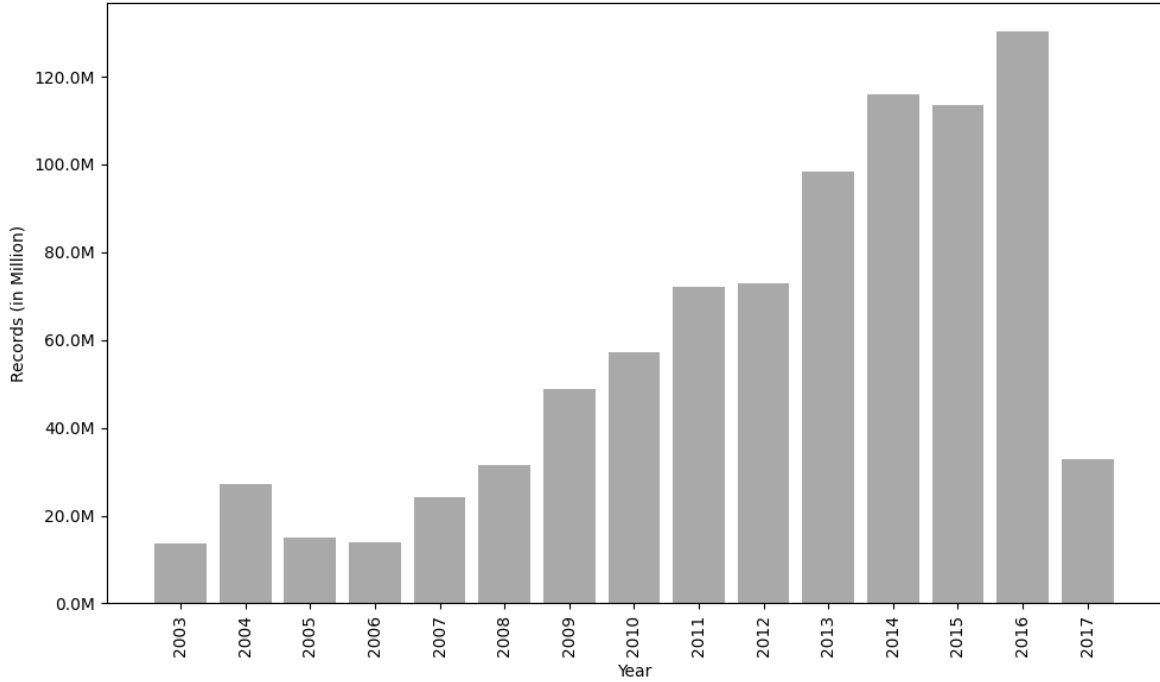


Figure 2.1: Distribution of investor access to EDGAR after data cleaning based on all EDGAR log files for January 1, 2003 to March 31, 2017.

Our preprocessed investor attention to EDGAR filings is increasing sharply, from 13 million requests in 2003 to its peak of over 130 million in 2016. In addition, the first quarter of 2017 shows that this pattern appears to be continuing. In the first quarter of 2017 alone, 32 million requests were processed, more than in any year up to 2008². A similar pattern can also be observed in the raw log files. This shows that the importance of EDGAR and the investors' demand for information is strongly increasing.

For each firm i in our attention dataset, we calculate the total daily attention on date t as follows:

$$Attention_{i,t} = \sum_{j \in \Omega_t} Count_{i,j}$$

²In 2008, a total of 31 million requests were recorded

where Ω_t represents the set of all records for date t , and $Count$ is a dummy variable, defined as:

$$Count_{i,j} = \begin{cases} 1 & \text{if record } j \text{ refers to firm } i \\ 0 & \text{otherwise} \end{cases}$$

In addition, for each firm i and each date t , we compute its moving average attention for the 90-day rolling window as follows:

$$MovingAverageAttention_{i,t}^{90D} = \sum_{n=1}^{90} \frac{Attention_{i,t-n}}{90}$$

Subsequently, we calculate the increase in relative attention for each firm i relative to its moving average of 90 days at date t as follows:

$$AttentionIncrease_{i,t} = \frac{Attention_{i,t}}{MovingAverageAttention_{i,t}^{90D}} - 1$$

Finally, we store the aggregated results at the firm CIK and date level, so that each record represents the entire attention of a specific firm on a specific date. The resulting dataset includes 79,897,812 records.

2.2.2 Collecting Stock Listing Information

Furthermore, we restrict our analysis to stock exchange-listed firms as they are more in the focus of investors who aim to substantiate their stock price expectations. This restriction also has several advantages. Stock exchange-listed firms are subject to disclosure and filing requirements, which provide greater data availability, such as annual or quarterly financial statements. They are also more strictly regulated, ensuring better data quality and comparability, as reporting is standardised and audited.

In order to identify stock-listed firms in our attention data, we use data from SEC-API.io. Their CIK, CUSIP, Ticker Mapping API provides lists of all stock-listed firms for several exchanges. We choose the two major exchanges, NASDAQ and NYSE. Furthermore, we only consider firms with domestic stock listings, traded in USD and located in the United States. The resulting exchange lists include the firm's CIK as well as start and end dates for each listing. This enables us to exclude attention records that do not belong to stock-listed firms, as well as records that are before the start or after the end date of the stock listing.

2.2.3 Firm Locations and Geocoding

In addition, we want to enrich our attention data with geographical information about the firms, in particular to determine the distance between them. First, to measure the distance between the headquarters of two firms, we need each firm's address at time t . The SEC provides an API that can be used to query the submission history for all filers. The submission history includes the major financial statement submissions of all EDGAR filers. The complete history is available as a bulk download and contains not only the historical filings but also basic company information such as the current address. Due to potential headquarter relocations, this address is not necessarily valid at time t but serves as a starting point for our data collection. Subsequently, we identify the first date for each firm on which a firm appears initially in our attention data. For the initial appearance date, we use the submission history to identify the accession number of the latest released filing at this date. This filing serves as a reference filing, which we use to access the corresponding index page. Each index page includes a footer that contains the company address at that time. We apply a scraping algorithm to collect the address information for each reference filing in our dataset. The SEC differentiates between business and mailing addresses. We primarily use the business address, and only if the filer did not provide a business address, we use the mailing address as a fallback. In most cases both addresses are identical. Since the address formats are not standardized and may contain non-geographical information, we perform data cleansing to align the address formats. Afterwards, we compare the scraped address with the current address as contained in the submission history. If both addresses are identical, we conclude that there have been no relocations between their initial appearance date and today. If the address deviates, at least one relocation took place between the initial appearance date and the ending date of our attention data. In this case, we perform a second iteration of address scraping. In this second iteration, we use the filing date of our initial reference filing as starting date. This date is then shifted forward by one year and used to choose a new reference filing. As in the initial scraping procedure, we scrape the address at this time. This is repeated until either the scrapped address is identical to the current address or the end date of the last appearance of this firm in our data is reached. We deem shifting the reference date by one year appropriate since addresses are not immediately updated in EDGAR. Moreover, this should only have a minor impact, as there is a transitional period during which the headquarters cannot yet be clearly separated. Also, for the second iteration of address scraping we perform data cleansing. We then create a master list of all available addresses, using the respective reference date as the valid since date.

Second, for the measurement of distance, we need to transform each raw address into its longitude and latitude representation. For this, we use the Google Maps Geocoding API. We limit the geocoding API to the US region to avoid mismatches. 23,706 unique company address combinations are contained in our dataset. Although we cleaned each address, incomplete addresses or alternating addresses may still be present in the data. Google’s fuzzy matching can catch many such data errors, but it cannot completely eliminate potential data errors. In addition to its longitude and latitude, the geocoding API response includes a *location type* indicating the precision of the geocode result. The location type rooftop indicates that the address has been mapped to a precise point in Google’s coordinate system, and the geocode result is precise on rooftop level. 21,591 of our results have this location type. The second location type, range interpolation, indicates that the address does not correspond to an explicit point but lies between two known points, usually on the same street. This type of geocode result is interpolated and is therefore less accurate than rooftop but can still be considered highly precise. 753 requests are associated with this location type. The remaining addresses have either the geometric centre type location or approximate. Both types indicate a higher uncertainty in precision. These location types occur, for example, with incomplete or low-quality address information. As the approximations do not have to be wrong by definition, we also look at the *type* argument of the geocode response, which provides information about the type of response address. From this we conclude that certain approximations, if they refer to a precise point (e.g., street or premises), are sufficient. Therefore, we added 616 of these approximations to our dataset. Since a single API request can contain multiple responses with different location types, we always favour the result with the highest precision. After merging, we use the geocoding result for 22,863 unique firm address combinations, while 800 firm address combinations are excluded due to the level of precision uncertainty.

Finally, we are able to measure the distance between two firms using the great circle distance. Following Admiralty (1922), this measurement assumes the Earth as a sphere and represents the shortest distance on the surface of the sphere. The expected error imposed by the great circle distance is less than 0.5%.

2.2.4 Sample Construction

After we prepared attention, stock listing, and geographical information individually, we are constructing our panel data as follows. Since the first years of our attention dataset include fewer requests than in the first quarter of 2017 and in order to reduce the amount of data to be processed, we limit our focus to the period from January 1, 2007 to March 31, 2017. After considering only attention data from stock-listed firms,

we end up with 18,816,766 records. Each record includes the total attention, moving average, and increase in attention of a given firm at date t .

Next, we identify shocked firms based on their excess attention over their moving average. We assume that a firm is shocked if its absolute attention is more than three times its 90-day-moving average. Furthermore, we require that an attention shock must have a certain size to be able to spill over to other firms. Therefore, we add a minimum of 500 end-user requests as a second condition to our shock definition³. Since attention shocks can last several consecutive days, we determine the start date of the shock and search for the first date on which the shock definition was met. This leaves us with 9,341 treated firm-date combinations in our sample. Table 2.1 shows which filing types are accessed by investors on all shock dates. About 80% of the attention is generated by only five types of filings. These are annual and quarterly reports, insider filings, announcements of major events (8-K) as well as the initial registration of new securities (S-1). On average, 39% of the attention is directed to new filings published in the last 10 days before the respective date. 70% of attention is attracted by information that is not older than one year. In particular, 8-K and S-1 filings are mainly consumed upon release and to a lesser extent from the past. Annual and quarterly reports, as well as insider information, have also historically been taken into account by investors. One reason for the unusually high number of investor requests these days can be attributed to relatively newly available information. However, older information is likely to also be used in the processing of newly available information, which in turn partially explains the requests for historical information. In particular, 10-K, 10-Q and insider information contain fundamental information on which investors base their expectations. On the other hand, another source of the increase in attention could be driven by further exogenous factors such as news articles or recent firm-unspecific events⁴.

As a control group, we select dates for the same group of firms on which no shock is present according to our shock definition. In order to ensure the representativeness of our control group, we select only dates that are at least 20 and at most 35 days before or after each shock⁵. By applying this time-window matching approach, we aim to minimize the impact of general time trends and to reflect that observed differences are due to the shock itself.⁶ For each firm, we randomly select up to two records from each interval of 20 to 35 days before and after the shock. Following from this, 37,008 additional control records are added to our sample.

³To support the robustness of these assumptions, our shock definition is subjected to further robustness checks in Appendix A.3.

⁴We account for these confounding factors using fixed effects as described in the methodology section.

⁵For these assumptions, further robustness tests are also presented in the appendix A.3.

⁶Nevertheless, we also consider time fixed effects in our regressions.

Form Group	Total Requests	Share (%)	of which: Filing Age (in %) :					
			$\leq 10D$	$\leq 1M$	$\leq 3M$	$\leq 1Y$	$> 1Y, \leq 5Y$	$> 5Y$
10-K	2,547,366	26.28	15.43	22.42	38.62	73.84	21.79	4.37
8-K	1,809,872	18.67	59.68	65.35	70.09	77.25	11.17	11.59
10-Q	1,468,306	15.15	20.03	27.26	43.64	66.50	25.96	7.54
S-1	1,012,875	10.45	78.33	85.18	89.56	93.58	3.56	2.86
Insider Filing (Form 3,4,5)	870,679	8.98	22.97	25.74	29.26	36.97	25.05	37.98
Other	1,983,966	20.47	51.25	55.82	61.11	72.27	12.93	14.78
Weighted Average			38.97	44.86	54.34	71.79	17.01	11.19

Table 2.1: Distribution of attention by filing type for firms that fulfill our shock definition of an abnormal increase in EDGAR search traffic. In case the condition was fulfilled on several consecutive days, only the initial day was taken into account. Data are derived from EDGAR log files and the history of submissions by each firm.

We then enrich the sample with the geographical information of the firms at the respective time. Therefore, we look up each firm’s longitude and latitude for each date by taking the last valid dataset from our geographic dataset. No information is available for about 1% of all records, which is the case if the firm is listed but its address is either not or only incompletely available on EDGAR or could not be geocoded. These records are removed from our sample and we end up with a total of 45,799 records.

To measure the spillover effect from shocked firms to any other firm (peer firms), we create pairwise records with every other firm that is stock listed at the respective time. Using the 45,799 treated and control records, we obtain 172,191,075 pairwise records. To account for a potential diffusion time for attention shocks to spill over to other firms, we add the attention information at $t + 0$ to $t + 3$ for the peer firms. Peer firms do not necessarily receive attention at the same dates as shocked firms. Therefore, our dataset may lack attention data for those days, so we set the absolute attention for these days to 0. Similarly, we recalculate the moving averages as well as attention increases for these days. As no attention increase can be calculated for 9,837 records, these records are excluded when used as dependent variable. Additionally, we add longitude and latitude information for the second firm of the generated pairs at the respective time. Similar to before, for about 1% of all firms there is no valid information in our geographic dataset. With the geographical information for the firm pairs on hand, we are now able to calculate the distance measured in miles between each firm combination. Subsequently, we calculate the logarithmic distance as $\log(1 + x)$, which takes into account the special case where two firms are registered at the same address and therefore the distance is equal to 0.

2.2.5 Additional Controls

Additionally, we incorporate quarterly controls using data from Compustat to account for various firm characteristics. For each firm at time t , we enrich our sample by the last available Compustat data. To control for firm size, we use the logarithm of total assets. This allows us to better compare firms of different sizes. We also include the Market to Book ratio, which is derived from the firms' market value divided by its total assets. The market value equals the sum of equity value and book value of debt. This control aims to take into account the firms' expected growth and profitability. Since firms that have suffered losses may behave differently, we add a loss dummy variable to our data. This variable is set to 1 if either the operating income before depreciation or net income is negative. Furthermore, we include the firm's leverage ratio, calculated as the sum of long-term debts and current portion of long-term debts, divided by total assets. The firms' efficiency is measured by a cash flow ratio, which is calculated as the operating activities net cash flow divided by total assets. Since the respective Compustat data are not available for all firms at each time, we exclude all records for which at least one control can not be calculated for either the shocked or peer firm. This reduces our sample from 170,070,989 to 123,657,740 records⁷.

We also include a control to measure whether the increase in attention results from a peer firm's recently published filing. Therefore, we use the submission history of filings. We introduce a dummy variable called *filing released*, which is set to 1 if the firm has published a filing between the last 10 days prior to the shock and time t .

2.2.6 Product Similarity

In the later course of our analysis, we want to examine not only the distance between two firms but also whether firms that have more in common are more exposed to spatial spillover effects. Traditional industry classifications such as SIC or NAICS help to broadly group firms, but do not capture the finer details and similarities that exist between firms beyond their industry belonging. In recent years, the research towards industry classification and differentiation evolved and more dynamic datasets are available. Hoberg and Phillips (2010, 2016) initiated a comprehensive research project in 2006 that examines the industry structure, product market competition, and firm relatedness. Following from this, Hoberg and Phillips (2010, 2016) published the 10-K Text-based Network Industry Classifications (TNIC) dataset. Instead of fixed industry classifications they estimate product similarities for firm pairs based on business descriptions of 10-K SEC filings. To compile this dataset, they collected

⁷In case attention increase is used as dependent variable. If absolute attention is used, 9,837 additional records are available.

all 10-K filings from EDGAR starting from 1996 and have continuously updated the dataset to the present. The complete TNIC dataset is available at the Hoberg-Phillips Data Library and comprises Compustat identifier (gvkey) for firm pairs as well as a similarity score for each year. The similarity measure is derived from a text-based analysis of product descriptions, which includes pre-processing steps such as stop word removal and text vectorization. The similarity of two firms is then calculated as the cosine similarity of these vectors, ranging from 0 to 1. Firms that use semantically similar vocabularies in their product descriptions are considered to have a high product similarity.

Following Hoberg and Phillips (2010, 2016), we enrich our panel data using the TNIC dataset. Previously, we already enriched our sample with Compustat data, which also included the gvkey identifier for peer and shocked firms. By merging the data, we get product similarity scores for 101,593,632 records. Several factors may contribute to missing product similarity scores in our data. These may include cases where firms are excluded due to design assumptions, e. g. firms with product descriptions that are too short are excluded by Hoberg and Phillips (2016). In addition, not all firms are fully captured in the TNIC dataset each year. Nevertheless, since the number of records where product similarity scores are available is still sufficiently large, we consider the use of a subsample to be adequate.

However, it should be noted that the use of textual similarities in product descriptions does not necessarily imply real economic competition between firms and only serves as a proxy. Additionally, textual based industry classifications lack further information about the market itself and do not take into account, for example, market size, distribution of market shares, strategic alliances, consumer awareness, and more. Furthermore, firms could describe the same product fundamentally differently, which may lead to an underestimation of their similarity scores. Lastly, interpreting product similarity scores is not as straightforward as using traditional industry classifications, which are broadly used. These limitations open the door for future research to examine alternative proxies for economic relationships and industry classifications. Alternative approaches might be found in the work of Kee (2019), who also proposed a text-similarity-based peer identification method using newspaper articles instead of product descriptions, shifting the perspective from the firm to the consumer. Alternatively, clusters of similar firms could be derived by adopting the co-search algorithm proposed by Lee et al. (2015), which uses EDGAR log files. The Herfindahl-Hirschman Index, which takes into account the market share of each firm in an industry, is also widely used to measure industry concentration and provides insights into the level of market competition (Li et al., 2020; Matsumoto et al., 2012).

2.3 Methodology

In order to examine whether attention shocks can geographically spill over from a shocked firm j to a peer firm i , we introduce three OLS regression models to conduct Difference-in-Difference (DiD) experiments. Each regression is based on the sample as constructed in the sections before. Only the last model uses a subset of our sample as the product similarity score is not available for all firms in our sample. Table 2.2 shows the descriptive statistics of the sample used.

	mean	std	min	25%	50%	75%	max
Explain Variables							
Shock Dummy	0.20	0.40	0.00	0.00	0.00	0.00	1.00
Distance (in miles)	1188.51	810.46	0.00	537.82	1025.65	1790.74	5147.91
$\log(1 + \text{Distance})$	6.66	1.22	0.00	6.29	6.93	7.49	8.55
$\log(1 + \text{Distance}) \times \text{Shock}$	1.32	2.71	0.00	0.00	0.00	0.00	8.55
Product Similarity (TNIC)	0.02	0.03	0.00	0.00	0.00	0.02	0.94
Dependent Variables							
Absolute Attention at t_0	42.20	160.12	0.00	7.00	19.00	43.00	88373.00
Attention Increase to 90-Day-Mean at t_0	0.14	1.68	-1.00	-0.60	-0.17	0.41	639.49
Controls							
Filing Released $_{t_0, \text{Peer}}$	0.59	0.49	0.00	0.00	1.00	1.00	1.00
$\text{Size}_{\text{Peer}}$	6.77	2.12	-5.30	5.40	6.79	8.15	14.76
MBR_{Peer}	1.93	20.45	0.00	0.73	1.14	1.93	2650.12
$\text{Loss}_{\text{Peer}}$	0.26	0.44	0.00	0.00	0.00	1.00	1.00
$\text{Leverage}_{\text{Peer}}$	0.26	2.56	0.00	0.03	0.17	0.36	496.52
$\text{Cashflow}_{\text{Peer}}$	0.00	0.60	-86.24	-0.00	0.02	0.07	3.36
$\text{Size}_{\text{Shocked}}$	9.00	2.41	-2.56	7.50	9.09	10.60	14.74
$\text{MBR}_{\text{Shocked}}$	1.81	2.48	0.00	0.80	1.23	2.04	84.94
$\text{Loss}_{\text{Shocked}}$	0.20	0.40	0.00	0.00	0.00	0.00	1.00
$\text{Leverage}_{\text{Shocked}}$	0.31	0.68	0.00	0.13	0.26	0.41	45.22
$\text{Cashflow}_{\text{Shocked}}$	0.03	0.19	-9.01	0.01	0.04	0.09	0.86

Table 2.2: Descriptive statistics for our sample using EDGAR data from January 01, 2007 to March 31, 2017. Compustat data were used to derive accounting related controls. The filing released dummy is based on the SEC submission history. Firm addresses are collected manually and geocoded as outlined in Section 2.2.3.

Our first model serves to confirm that proximity-driven spillover effects are generally present. Furthermore, to gain a baseline understanding of how the geographic relationship between firms can influence their attention. Hence, we propose the following regression model:

$$\begin{aligned}
AttentionIncrease_{i,t+n} = & \beta_1 \times Shock_{j,t} + \beta_2 \times \log(1 + Distance_{i,j,t}) \\
& + \beta_3 \times (Shock_{j,t} \times \log(1 + Distance_{i,j,t})) \\
& + \sum_{n=0}^N \beta_4 \times FilingReleased_{i,t+n} + Controls_{i,j,t} \\
& + v_i + u_t + x_{i,t} + \epsilon_{i,t}
\end{aligned}$$

As the dependent variable we utilize $AttentionIncrease_{i,t+n}$, which represents the relative attention increase compared to the 90-day moving average of a peer firm i at time t . To capture the spillover evolution over time, we estimate multiple distinct models, each offset by one day. Here, $t + 0$ denotes the day on which a shocked firm j first meets our shock definition. Our independent variables comprise the logarithmic distance between firm i and j as well as a dummy shock variable for firm j . $Shock_{j,t} \times \log(1 + Distance_{i,j,t})$ measures the interaction between the logarithmic transformed distance and attention shock. Therefore, β_3 is the coefficient of interest to examine a spatial spillover effect. Additionally, we include accounting controls and our filing released dummy. To capture unobserved heterogeneity and time invariant firm characteristics, we add fixed effects v_i for firm i . In order to account for systemic effects that are consistent across all firms, we also add time fixed effects u_t . In addition, we include interaction fixed effects $x_{i,t}$ between firm and month-year time fixed effects. Using month-year interactions allows us to capture firm-specific trends on a monthly average while still leaving room to identify daily spillover effects at shock dates. In contrast, daily interactions would fully absorb the expected spillover. Given the inclusion of fixed effects in our model, an intercept is deemed unnecessary and is therefore omitted to avoid redundancy in the regression's specification. Conceptually, it is noteworthy that although we are controlling for a large number of fixed effects, other omitted variables could still simultaneously be driving both proximity and the co-movement in attention. Therefore, we conducted a pre-shock analysis presented in Appendix A.2, accompanying our results in section 2.4. Our analysis demonstrates the validity of the parallel trend assumption by showing no remarkable differences in trends between treated and untreated firms prior to the shock. This suggests that our choice of controls is sufficient to account for confounding factors and supports the validity of our DiD approach.

Subsequently, we examine the geographical structure of potential spillover effects by introducing distance bin variables. This is motivated by the hypothesis that the transmission of an attention shock is not uniform across different levels of distances. Hence, introducing discrete distance bins allows us to also capture non-linear effects

within our analysis. To this end, we propose the following regression model:

$$\begin{aligned} \text{Attention}_{i,t+n} = & \beta_1 \times \text{Shock}_{j,t} + \beta_2 \times \log(1 + \text{Distance}_{i,j}) \\ & + \sum_{k=1}^N \beta_{k+2} \times (\text{Shock}_{j,t} \times D_{i,j}^{k,k+1}) + \text{Controls}_{i,j,t} + v_i + u_t + x_{i,t} + \epsilon_{i,t} \end{aligned}$$

Each bin $D_{i,j}^{k,k+1}$ represents a specific range of distances. The interaction terms between bin and shock variables enable us to assess the dynamics of how attention propagates through geographical networks. In addition, we utilize the same set of controls and fixed effects as outlined before.

Since in the previous regression analyses we focused only on the geographical relationship between firms, we want to extend our analysis and also take into account product similarities between firms. We aim to investigate whether firms that are operating in similar product spaces, and therefore can be considered as direct rivals, are more prone to proximity attention spillovers. Therefore, we are utilizing the Text-based Network Industry Classifications (TNIC) data as published by Hoberg and Phillips (2010, 2016). The dataset comprises a similarity score that indicates whether two firms use similar vocabulary in their product description and therefore are operating within the same industry / product space. Since we expect that the transmission of attention varies across different levels of distances, we extend the previously presented regression model utilizing discrete distance bins as follows:

$$\begin{aligned} \text{AttentionIncrease}_{i,t+n} = & \beta_1 \times \log(1 + \text{Distance}_{i,j,t}) + \beta_2 \times \text{Shock}_{j,t} \\ & + \beta_3 \times \text{ProductSimilarity}_{i,j,t} \\ & + \beta_4 \times \text{Shock}_{j,t} \times \text{ProductSimilarity}_{i,j,t} \\ & + \sum_{k=1}^M \beta_{5,k} \times D_{i,j}^{k,k+1} \times \text{Shock}_{j,t} \\ & + \sum_{k=1}^M \beta_{6,k} \times D_{i,j}^{k,k+1} \times \text{ProductSimilarity}_{i,j,t} \\ & + \sum_{k=1}^M \beta_{7,k} \times D_{i,j}^{k,k+1} \times \text{Shock}_{j,t} \times \text{ProductSimilarity}_{i,j,t} \\ & + \sum_{k=0}^N \beta_{8,k} \times \text{FilingReleased}_{i,t+n} + \text{Controls}_t \\ & + v_i + u_t + x_{i,t} + \epsilon_{i,t} \end{aligned}$$

Besides the logarithmic distance and shock variables, we add the Product Similarity, which represents the similarity score as obtained in the TNIC dataset. We introduce two-way interaction terms for every combination of distance bins $D_{i,j}^{k,k+1}$, *Shock* and *ProductSimilarity*, complemented by three-way interaction terms to capture the joint effect of these variables. Accounting controls and fixed effects remain consistent with our previous models.

2.4 Empirical Results

In this section, we present the empirical results of our three OLS regression models. We start with the analysis that spatial spillover effects are generally present. Table 2.3 summarizes the OLS regression results. The dependent variable is the relative attention increase of firm i compared to its 90-day moving average, and the independent variables are the logarithmic distance and shock dummy.

Initially, we examine the direct effects of shocks and distance to gain a basic understanding of how each of these variables influences investor attention in isolation. This analysis provides a foundational understanding before we introduce interactions. The results in the first column show that a shock at firm j has a slightly but statistically significant negative effect on the attention increase of firm i , as the shocked firm j seems to attract the attention of investors. Furthermore, the distance between treated and untreated firms does not have a significant effect on the attention of peer firms. In the subsequent columns, we show the regression results, introducing the interaction between distance and shock. Here we observe that the sign of the shock coefficient switches from -0.0035 to 0.0101, which indicates that the effect of an abnormal attention increase of firm j is not uniform and depends on additional factors such as the distance between firms. Additionally, the coefficient of logarithmic distance becomes statistically significant. The estimated coefficient of the interaction term is -0.002 and statistically significant at the 1% level. In summary, the results indicate that the interaction term has a moderating effect. The total attention increase is slightly positive for firms in direct proximity, but it turns negative as the distance increases. This effect diminishes over time, becoming insignificant by the second day after the shock at firm j . Figure 2.2 visualizes the combined expected attention increase of firm i depending on the distance to firm j ⁸. In addition to the spillover effect, the attention of firm i increases if it has recently released a filing itself. Moreover, the results indicate that increases in attention are weaker for larger firms. It should be noted that in this analysis, we are measuring

⁸The total effect is calculated as the sum of the effects of logarithmic distance, shock dummy, and the interaction variable. Insignificant coefficients are treated as 0.

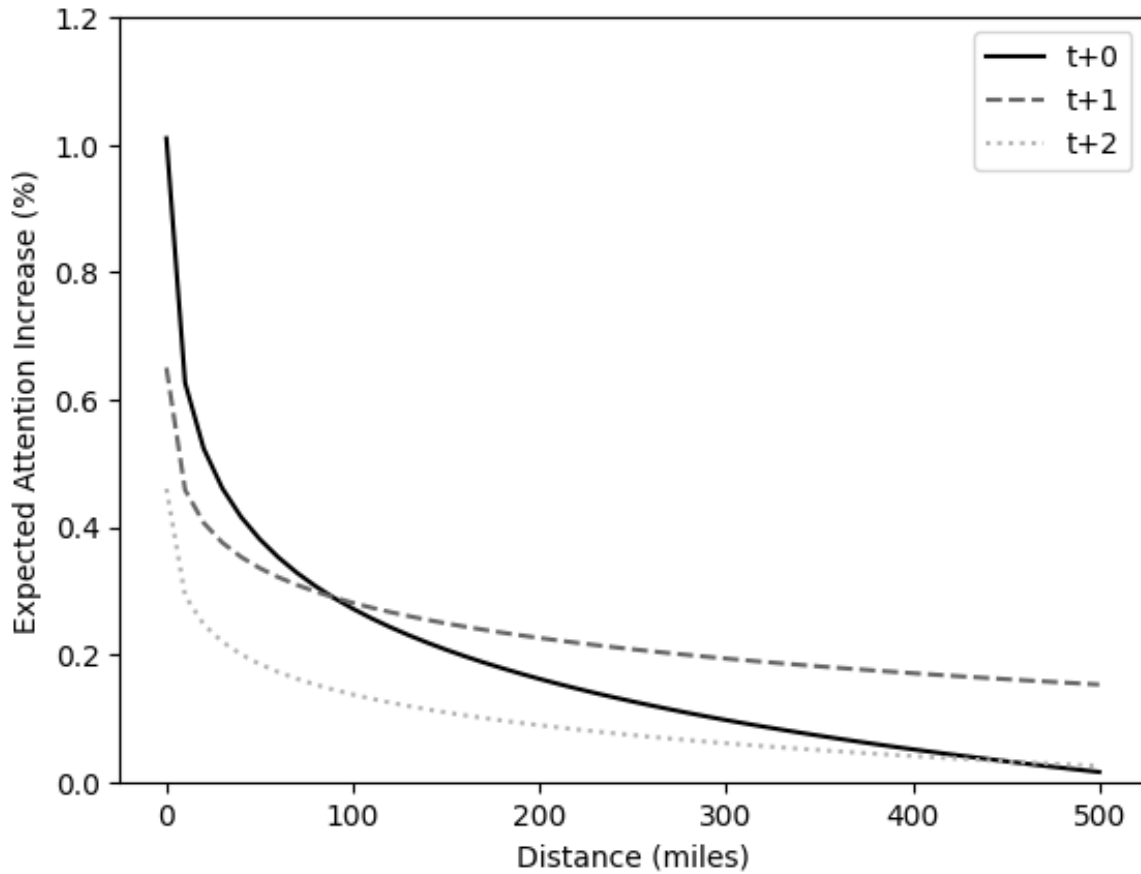


Figure 2.2: This figure presents how the attention towards firm i is expected to increase due to the abnormally increased attention of a firm j . It shows the relationship between the increase in attention and geographical distance between the two firms at times $t + 0$, $t + 1$ and $t + 2$. Here, $t + 0$ refers to the time when a firm j received an abnormal attention increase following our shock definition as outlined in Section 2. The expected attention increase is derived from the regression results of Table 2.3.

the effect across all firms, and that larger effects with economic relevance could be visible in finer segments. However, the existence of a statistical effect across the entire population, even if it is small, already shows that spillover effects exist in principle. This confirms our expectation that attention can spill over from firm j to firm i even if this effect does not yet have any economic implications from a broad perspective.

	Baseline	Including Interaction			
	Attention Increase $t+0$	Attention Increase $t+0$	Attention Increase $t+1$	Attention Increase $t+2$	Attention Increase $t+3$
$\log(1 + Distance)$	-0.0000 (0.0001)	0.0004** (0.0002)	0.0003* (0.0002)	0.0002 (0.0002)	0.0000 (0.0001)
Shock	-0.0035*** (0.0001)	0.0101** (0.0040)	0.0065* (0.0036)	0.0046* (0.0025)	0.0011 (0.0021)
$Distance \times Shock$		-0.0020*** (0.0006)	-0.0011** (0.0005)	-0.0007* (0.0004)	-0.0002 (0.0003)
Filing Released _{Peer}	0.1193*** (0.0031)	0.1193*** (0.0031)	0.1106*** (0.0030)	0.0994*** (0.0030)	0.0920*** (0.0030)
Size _{Peer}	-0.1337*** (0.0478)	-0.1337*** (0.0478)	-0.0892 (0.0544)	-0.1271** (0.0586)	-0.0875*** (0.0297)
MBR _{Peer}	-0.0003** (0.0001)	-0.0003** (0.0001)	-0.0002 (0.0003)	-0.0007 (0.0007)	-0.0001 (0.0002)
Loss _{Peer}	-0.0161 (0.0144)	-0.0162 (0.0144)	-0.0205 (0.0128)	-0.0483* (0.0270)	0.0073 (0.0153)
Leverage _{Peer}	0.0018 (0.0015)	0.0018 (0.0015)	-0.0010 (0.0018)	0.0009 (0.0014)	0.0010* (0.0006)
Cashflow _{Peer}	0.0164 (0.0124)	0.0164 (0.0124)	-0.0280*** (0.0089)	0.0072* (0.0037)	-0.0001 (0.0028)
Size _{Shocked}	0.0002*** (0.0000)	0.0002*** (0.0000)	0.0000*** (0.0000)	-0.0000** (0.0000)	-0.0000*** (0.0000)
MBR _{Shocked}	0.0001*** (0.0000)	0.0001*** (0.0000)	0.0000 (0.0000)	-0.0000* (0.0000)	-0.0000* (0.0000)
Loss _{Shocked}	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0000 (0.0000)	-0.0000** (0.0000)	-0.0000*** (0.0000)
Leverage _{Shocked}	0.0001 (0.0000)	0.0001 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
Cashflow _{Shocked}	0.0000 (0.0002)	0.0000 (0.0002)	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
R ²	0.1618	0.1618	0.1610	0.1659	0.1647
S.E. type	Clustered by: Firm and Time	Clustered by: Firm and Time	Clustered by: Firm and Time	Clustered by: Firm and Time	Clustered by: Firm and Time
Observations	123657740	123657740	123657740	123657740	123657740

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.3: This table shows OLS regression results that measure the relative increase in attention towards a peer firm i in comparison to its 90-day moving average. The first column shows a baseline specification that excludes the two-way interaction term. The subsequent columns show the regression results including the interaction of $\log(1 + Distance)$ and $Shock$ for $t + 0$, $t + 1$, $t + 2$ and $t + 3$. Here, $t + 0$ denotes the time when a firm j received an abnormal attention increase following our shock definition as outlined in Section 2. Clustered standard errors are displayed in parentheses below the coefficient estimates.

Building on the previous analysis, we want to further examine the structure of attention spillovers. Therefore, we analyze the regression results of our second model, utilizing discrete distance bins instead of continuous logarithmic distance. First, we apply a discretization of near-range distances. We introduce equidistant distance bins in 50-mile increments up to 250 miles. The rationale behind choosing a 50-mile step size is twofold. First, equidistant cutoffs are easy to compare and ensure uniform sensitivity across bins. Second, the cutoffs align with statistical quantiles in the data distribution. The 12.5% quantile equals 248 miles and is close to the upper end of our bins of 250 miles. Similarly, the other cutoffs approximate the 5, 7.5 and 10% quantiles⁹.

The results are shown in the first three columns of table 2.4. The overall effect of an abnormal attention increase at firm j on the attention increase at firm i is negative. The effect decreases over time and remains significant at a 5% significance level. Especially for distance bins up to 100 miles, positive coefficient estimates indicate that spillover effects are influenced by the proximity between firms. Considering the negative coefficient of the shock itself, and accounting for interaction terms, the net effect remains positive. A similar positive effect is observed for 200 to 250 miles. These findings suggest that spillover effects are non-linear and vary across different distance ranges. While the coefficients for the lower end of our distance bins are significant at $t + 0$, additional significant effects at $t + 1$ are observed for distances between 100 and 150 miles as well as 200 to 250 miles. This indicates that spillover effects may require more time to manifest as the distance between the firms increases.

Secondly, we repeat our analysis using long-range distance bins. The rationale for our discretization choice follows the same considerations as those outlined for the near-range distances. Here, we select 250-mile increments, with the upper limit of 1750 miles, which is close to the 75% quantile of 1790 miles. Except for the 1500-mile cutoff, each of our cutoffs corresponds in approximation to a quantile, increasing in 12.5% steps from the 12.5% to the 75% quantiles.

The results are presented in the last three columns of table 2.4. As in our previous analysis, the effect of the shock is generally negative. For distances between 0 and 250 miles, positive and significant effects are observed at $t+0$ and $t+1$. No significant positive effects are observed for distances over 250 miles. It is noteworthy that the results for distances above 500 miles show partially significant negative coefficients at $t + 1$ and $t + 2$. These results indicate that spillover effects may reverse with increasing distance, as investors pay more attention to firms in proximity to the shocked firm. The interpretation of our control variables remains consistent with our previous analysis for both near- and long-range distances.

⁹5% quantile is 65 miles, 7.5% quantile is 141 miles, 10% quantile is 194 miles

	Near Range Distance Bins			Long Range Distance Bins		
	Attention Increase $t+0$	Attention Increase $t+1$	Attention Increase $t+2$	Attention Increase $t+0$	Attention Increase $t+1$	Attention Increase $t+2$
$\log(1 + Distance)$	0.0003** (0.0002)	0.0004** (0.0002)	0.0002 (0.0001)	0.0003** (0.0001)	0.0003** (0.0002)	0.0001 (0.0002)
Shock	-0.0044*** (0.0004)	-0.0013*** (0.0003)	-0.0004** (0.0002)	-0.0037*** (0.0013)	0.0006 (0.0011)	0.0017** (0.0008)
$D_{0,50} \times Shock$	0.0093** (0.0039)	0.0055 (0.0034)	0.0046** (0.0022)			
$D_{50,100} \times Shock$	0.0113*** (0.0040)	0.0041 (0.0032)	0.0022 (0.0026)			
$D_{100,150} \times Shock$	0.0025 (0.0037)	0.0058* (0.0035)	-0.0002 (0.0029)			
$D_{150,200} \times Shock$	0.0048 (0.0034)	0.0028 (0.0031)	0.0048** (0.0024)			
$D_{200,250} \times Shock$	0.0082** (0.0032)	0.0086*** (0.0030)	-0.0008 (0.0023)			
$D_{0,250} \times Shock$				0.0070*** (0.0021)	0.0035* (0.0020)	0.0004 (0.0015)
$D_{250,500} \times Shock$				0.0016 (0.0019)	-0.0008 (0.0019)	-0.0004 (0.0017)
$D_{500,750} \times Shock$				-0.0005 (0.0022)	-0.0023 (0.0019)	-0.0032** (0.0016)
$D_{750,1000} \times Shock$				0.0006 (0.0025)	-0.0028 (0.0021)	-0.0042*** (0.0013)
$D_{1000,1250} \times Shock$				-0.0015 (0.0024)	-0.0043** (0.0020)	-0.0045*** (0.0016)
$D_{1250,1500} \times Shock$				-0.0051* (0.0029)	-0.0038 (0.0024)	-0.0029* (0.0016)
$D_{1500,1750} \times Shock$				-0.0037 (0.0026)	-0.0022 (0.0022)	-0.0028* (0.0017)
Filing Released $_{peer}$	0.1193*** (0.0031)	0.1106*** (0.0030)	0.0994*** (0.0030)	0.1193*** (0.0031)	0.1106*** (0.0030)	0.0994*** (0.0030)

Continued on next page

Table 2.4 – Continuation from previous page

	Near Range Distance Bins			Long Range Distance Bins		
	Attention Increase $t+0$	Attention Increase $t+1$	Attention Increase $t+2$	Attention Increase $t+0$	Attention Increase $t+1$	Attention Increase $t+2$
$Size_{Peer}$	-0.1337*** (0.0478)	-0.0892 (0.0544)	-0.1271** (0.0586)	-0.1337*** (0.0478)	-0.0891 (0.0544)	-0.1271** (0.0586)
MBR_{Peer}	-0.0161 (0.0144)	-0.0205 (0.0128)	-0.0483* (0.0270)	-0.0162 (0.0144)	-0.0205 (0.0128)	-0.0483* (0.0270)
$Loss_{Peer}$	-0.0041 (0.003)	-0.0042 (0.003)	-0.0042 (0.0029)	-0.0041 (0.003)	-0.0042 (0.003)	-0.0042 (0.0029)
$Leverage_{Peer}$	0.0018 (0.0015)	-0.0010 (0.0018)	0.0009 (0.0014)	0.0018 (0.0015)	-0.0010 (0.0018)	0.0009 (0.0014)
$Cashflow_{Peer}$	0.0164 (0.0124)	-0.0280*** (0.0089)	0.0072* (0.0037)	0.0164 (0.0124)	-0.0280*** (0.0089)	0.0073* (0.0037)
$Size_{Shocked}$	0.0002*** (0.0000)	0.0000*** (0.0000)	-0.0000** (0.0000)	0.0002*** (0.0000)	0.0000*** (0.0000)	-0.0000*** (0.0000)
$MBR_{Shocked}$	0.0000*** (0.0000)	-0.0000 (0.0000)	-0.0000*** (0.0000)	0.0000*** (0.0000)	-0.0000* (0.0000)	-0.0000*** (0.0000)
$Loss_{Shocked}$	0.0003*** (0.0001)	-0.0000 (0.0000)	-0.0000** (0.0000)	0.0004*** (0.0001)	-0.0000 (0.0000)	-0.0000 (0.0000)
$Leverage_{Shocked}$	0.0001** (0.0000)	0.0000** (0.0000)	0.0000** (0.0000)	0.0001* (0.0001)	0.0001** (0.0000)	0.0001*** (0.0000)
$Cashflow_{Shocked}$	0.0001 (0.0002)	0.0000 (0.0000)	0.0000 (0.0000)	0.0001 (0.0002)	0.0001 (0.0001)	0.0001** (0.0000)
R^2	0.1618	0.1610	0.1659	0.1618	0.1610	0.1659
S.E. type	Clustered by: Firm and Time	Clustered by: Firm and Time	Clustered by: Firm and Time	Clustered by: Firm and Time	Clustered by: Firm and Time	Clustered by: Firm and Time
Observations	123657740	123657740	123657740	123657740	123657740	123657740

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 2.4: This table shows OLS regression results that measure the relative increase in attention towards a peer firm i in comparison to its 90-day moving average. The first three columns show a discretization of distances in the near range up to 250 miles, while the last three columns show a discretization in the long range up to 1750 miles. For near and long range discretization, the results are estimated for $t + 0$, $t + 1$ and $t + 2$. Here, $t + 0$ denotes the time when a firm j received an abnormal attention increase following our shock definition as outlined in Section 2. Clustered standard errors are displayed in parentheses below the coefficient estimates.

So far, our empirical results indicate that a spatial attention spillover effect exists. The intensity is higher for firms in close proximity and decreases with increasing distance between the firms. The effect is non-linear and can persist over time for several days, but peaks at the instant of the occurrence of abnormal attention increase at another firm¹⁰. The overall effect varies but remains in the region of 1%. As already stated earlier, in our previous analysis, we have only considered the spatial relationship across the entire population of firms. Our results, therefore, only demonstrate that an effect exists on an average. Yet this leaves other potential relationships between firms unexamined. To better understand the economic relevance, we also consider product similarity between firms, as it may play a crucial role in determining the strength of spillover effects.

Figure 2.3 illustrates the results of our OLS regression by showing the total attention increase for a peer firm j when a firm i experiences an abnormal increase in attention. First, we observe that spillover effects are much more pronounced when product similarity is taken into account. Firms located in direct proximity to a shocked firm i experience up to a 91% increase in attention at $t+0$. This effect decreases with increasing distance, dropping to 41% at 1000 miles, and then gradually increases again, up to 60% at 1500 miles. Assuming firms i and j are in direct proximity and firm j experiences an abnormal attention increase at $t + 0$, the expected overall attention increase compared to firm i 's 90-day mean is 91%¹¹. Suppose firm i relocates its headquarters, thereby extending the distance to 1000 miles, while the product similarity remains unchanged. After the relocation, an abnormal attention increase at firm j only increases the attention towards firm i by 41 %. Therefore, the overall attention spillover is 50% lower when the distance between the two firms increases. Second, we observe that the effect also decreases over time. For $t+2$, we observe for firms in direct proximity still an effect of approximately 22%. Furthermore, we observe that, compared to $t+0$, the downward trend for $t+1$ and $t+2$ extends to greater distances before it begins to reverse.

In addition to the above described dynamics, proximity also shapes a peer firm's attention increase when their product similarity changes. Figure 2.4 shows the total spillover effect for different levels of product similarity at $t+0$. We observe that the effect flattens out as product similarity decreases. Vice versa, the effects are more pronounced when the similarity increases. For example, suppose firm i adjusts its strategy so that its product offerings become more similar to those of firm j , resulting in an increase from 0.5 to 0.75 in the product similarity score. If the two firms are in direct proximity, firm i 's relative attention increase is 23% more influenced by an

¹⁰Our pre-shock analysis in Appendix A.2 shows that no effects are observed before the shock occurs.

¹¹The total effect is calculated as the sum of the effects of all main variables, two-way and three-way interactions

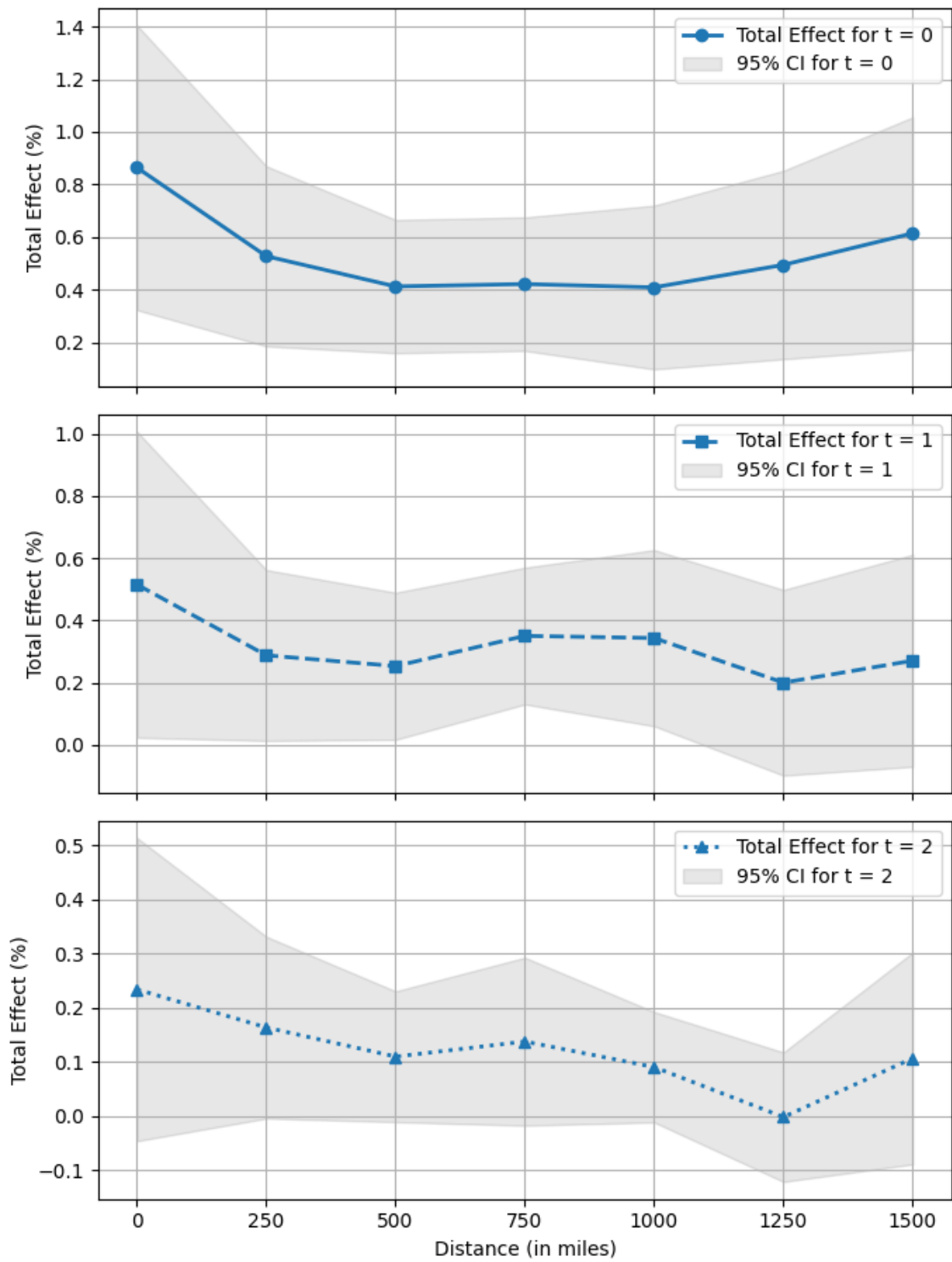


Figure 2.3: This figure shows the increase in attention for a peer firm j following an abnormal increase in attention experienced by firm i , measured across different distances over the periods $t+0$, $t+1$, and $t+2$. Here, $t+0$ refers to the initial date when firm i experienced an abnormal increase in attention. The total spillover effect is derived from the OLS regression coefficient estimates presented in Table A.1. Linear interpolations between the discrete distance bins have been used for visualization purposes. The 95% confidence intervals are shaded in grey.

abnormal attention increase of firm j than before the strategy adoption. If both firms are 1000 miles apart, the spillover effect is only 10% higher than before the strategy change.

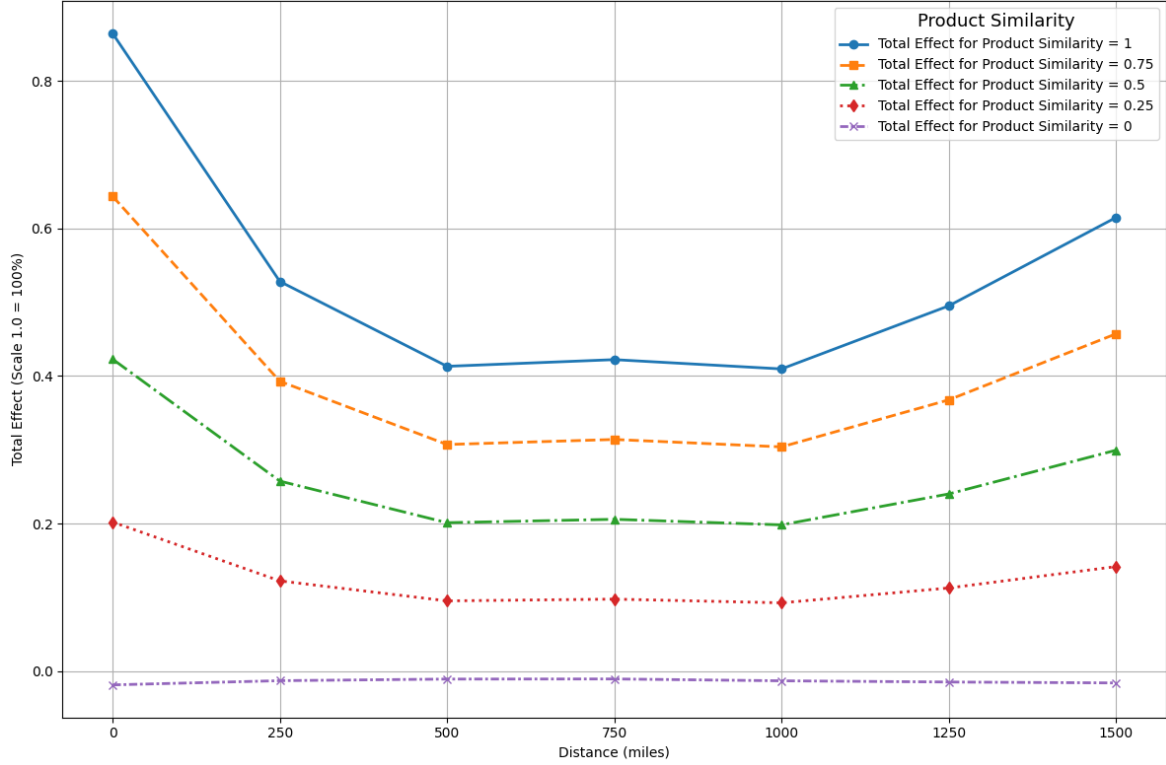


Figure 2.4: This figure shows the total spillover effect in relation to different levels of product similarity at $t+0$. The total spillover effect is derived from the OLS regression coefficient estimates presented in Table A.1. Linear interpolations between the discrete distance bins have been used for visualization purposes.

The detailed results of our OLS regression are presented in Table A.1 in the appendix. To better understand the dynamics, we examine below the individual contributions of the coefficient estimates to the overall effect. As in our previous analysis, the coefficient estimate of the logarithmic distance itself is significant but only makes a minor contribution to the total relative attention increase of firm i . The shock dummy coefficient suggests that firm i 's relative attention, when compared to its 90-day mean, decreases by 2.3% when firm j encounters an abnormal increase in attention. The estimates decrease over time but remain significant. Initially, the product similarity coefficient estimate does not contribute to the relative attention increase of firm i at time $t + 0$. In the subsequent days, this turns into a negative effect up to 5.4% at $t + 2$.

For the two-way interactions between our three main variables, we observe that there is a strong and significant effect on firm i 's relative attention explained by the interaction of an abnormal attention increase at firm j and the product similarity. This

finding suggests that there is a strong spillover effect in attention for firms with higher product similarity. As with the coefficient estimates for the shock variable, this effect decreases from 113% in $t + 0$ to 30% in $t + 2$, remaining significant. Furthermore, the interaction term between distance and shock shows a similar pattern as in our previous analysis.

The coefficient estimates of the three-way interactions show significant results across all distance ranges for $t + 0$. Furthermore, the estimated effects are decreasing over time. However, the estimates for $t + 1$ and $t + 2$ are not consistently below the 10% significance level. Considering the strong induced attention increase driven by the interaction of shock and product similarity, we now see that the three-way interaction seems to have a moderating effect on the total effect. For firms within a distance of 0 to 250 miles and with highly similar product descriptions, we observe a 22% reduction in the relative attention of firm i resulting from an abnormal attention increase of firm j . This effect increases up to a 73% reduction if the two firms are located between 1000 and 1250 miles apart. Subsequently, this effect decreases to a 46% reduction if the distance is between 1500 and 1750 miles.

When considering all coefficient estimates together, we finally observe pronounced spillover effects between treated and untreated firms as presented in Figure 2.3. Adding product similarity has increased the granularity of our data and enables us to observe effects in finer segments, which now also show significant economic relevance. The similarity between firms' products is reasonably the main driver of attention spillovers. Nevertheless, geography clearly shapes the intensity of the observed effects and also plays a crucial role in the transmission of information. While the standalone variables contribute little to the overall effect, the estimates for the two-way interactions show positive contributions. The main contribution comes from the interaction between shock and product similarity, whereas the three-way interactions moderate the overall effect again. The total spillover effect is larger for firms in close proximity and smaller with increasing distance. While the source of attention shocks may be driven by rational investor dynamics, the transmission across firms indicates potential market inefficiencies, particularly as investors seem to leverage and compare newly consumed information more locally. The spillover effect lasts for at least three days, in which peer firms experience a higher mean attention compared to the post-shock period. The magnitude of these effects, as well as the duration, could have further implications on stock market reactions, especially considering potential intraday movements, which provides further research opportunities.

In summary, our results clearly demonstrate that proximity in conjunction with an economic relationship between the firms plays a pivotal role in the transmission of

spillover effects on investor attention. Furthermore, we conduct several robustness checks to further substantiate our findings. The results are presented in the Appendix A.3. This includes testing alternative dependent variables and regression types as well as the main assumptions used to create our sample. We obtained similar results that give us further confidence in the robustness of our findings.

2.5 Conclusion

Our research contributes to the local information bias literature, building upon the foundational studies such as those by Bernile et al. (2015). Prior studies on investor attention have mainly focused on the geographical relationship between investors and firms, while the transmission of attention between firms has been largely unobserved. By analyzing EDGAR search traffic data alongside geographical data for firms, we offer a novel perspective on the dynamics of investor attention. Our findings confirm the existence of spillover effects on investor attention, facilitated by the proximity of firms. These effects underpin the home bias of investors, as proposed by Bernile et al. (2015). While Mengoli et al. (2023) and Dyer (2021) demonstrate that investor-to-firm-proximity influences the intensity of information acquisition, our findings extend this understanding. We show that local attention spikes also create a network effect, boosting nearby firms' attention. Considering only the continuous distance between firms reveals that firms in direct proximity receive up to a 1% higher increase in attention relative to firms that are farther away. By introducing discrete distance bins, we find that attention spillover effects are non-linear, with the estimated effects being relatively small but highly statistically significant for firms in the near range. Additionally, accounting for economic relationships among firms, by utilizing product similarity as a proxy, demonstrates a highly significant and pronounced increase in the spillover effect. Firms operating in closer product markets receive up to a 50% higher increase in attention relative to firms that are farther away. At the same time, increasing similarity between firms influences the attention of firms differently depending on their proximity to its peers. Our findings are consistent with the findings of Engelberg et al. (2018), who show that investors tend to acquire information about groups of firms within the same industry cluster and geographical area in order to reduce the cost of information processing. Additionally, we expand on Lee et al. (2015) by identifying geographical proximity between firms as another crucial factor influencing investor behavior. Since we are using EDGAR search traffic data, our findings suggest that co-search patterns may also be influenced by proximity between firms. The observation of non-linear attention spillover effects and significantly increased impacts from product

similarity indicates that attention does not diffuse evenly across the market. This uneven distribution may cause information asymmetries and local market inefficiencies. Furthermore, for firms operating in closely related product markets, our results show that attention also transfers over greater distances, although this effect diminishes with increasing distance. In conclusion, these observations offer a novel perspective on the findings of Chen (2023), who propose that a greater geographical dispersion of investor attention leads to more efficient market responses. Our results suggest that a greater geographical dispersion of investor attention alone may not always serve as a sufficient proxy for market efficiency. In addition, our findings are meaningful given that EDGAR is rather used by sophisticated investors as part of their information acquisition (Lee et al., 2015; Li and Sun, 2022; Gibbons et al., 2021). Understanding where sophisticated investors direct their attention not only may shed light on potential market movements but also could imply economic consequences for the entire market. As demonstrated by Ballinari et al. (2022), Yang et al. (2017), and Han et al. (2018), even a few days of increased investor attention, as observed in our study, can have a significant impact on price movements and volatility.

Our analysis offers further research opportunities into how proximity-powered attention spillovers affect stock price movements, the persistence of market inefficiencies due to local information bias as well as the propagation of attention among firms, and the arbitrage potentials created by firm proximity in conjunction with product similarity.

3 How Filing-Type Heterogeneity Shapes Investor Attention Spillovers Across Geographic and Product-Market Networks

3.1 Introduction

Investor attention plays a central role in how financial markets process information and update prices. A key source of firm-level information for investors are corporate filings, which they use to update beliefs and guide investment decisions. However, financial markets are characterized by limited investor attention, which constrains information processing and may lead to selective or delayed incorporation of news. Consequently, a growing literature has emerged on how and where investors allocate their limited attention.

Andrei et al. (2023) show that higher economic uncertainty increases investors' demand for firm-level information, while French and Poterba (1991) document the home bias phenomenon, highlighting how geographic proximity influences information acquisition. Huberman (2001) similarly finds that investors concentrate on familiar, local investments, often neglecting optimal diversification. Limited information-processing capacity leads investors to group similar firms together (Engelberg et al., 2018; Lee et al., 2015), consistent with category-based learning (Peng and Xiong, 2006). Drake et al. (2017) show that such attention allocation results in synchronized market movements within related groups of firms. Geographic frictions also shape attention. Investors pay more attention to nearby firms (Bernile et al., 2015; Dyer, 2021), while insider activity disclosures reduce information asymmetry (Wang, 2019). Mengoli et al. (2024) document that local firms attract disproportionately more attention, especially around news events or periods of high volatility. Most recently, Lehmann and Posch (2025) show that attention to one firm's filings spills over to product-market peers or geographically close firms, indicating systematic attention-based information spillovers.

While this literature highlights the directional flow of investor attention, it largely treats corporate filings as homogeneous. In practice, however, SEC filings differ substantially in content, timing, salience, and predictability. Motivated by this, we investigate whether spillover effects vary systematically across three major SEC filing types: sched-

uled financial reports (10-K and 10-Q), insider trading disclosures (Forms 3, 4, and 5), and event-driven 8-K filings. We also explore how product-market and geographic relatedness moderate these spillovers.

Importantly, filing type should not be interpreted as a primitive source of spillover heterogeneity, but rather as a summary characteristic that captures systematic differences in the informational environment surrounding disclosures. Different filing types vary not only in content and timing, but also in their typical information quality, degree of surprise, filing frequency, and the composition of investors who monitor them. For example, insider filings are infrequent and closely scrutinized by sophisticated investors, while periodic filings are highly predictable and routinely processed as benchmarking documents. Event-driven filings such as 8-Ks differ further in scope and may attract attention from a broader investor base when they convey information with industry-wide relevance. Accordingly, our analysis does not attribute spillover heterogeneity mechanically to filing labels, but uses filing type as a structured way to capture these underlying differences in informational salience, predictability, and audience.

Our hypotheses are grounded in four well-established insights from the literature on investor attention and information diffusion:

- **Salience and Surprise:** Investors are more likely to pay attention to disclosures that are unexpected or perceived as containing sensitive, value-relevant information (Drake et al., 2017; Brochet, 2010; Wang, 2019).
- **Predictability:** Periodic filings such as 10-Ks and 10-Qs trigger less pronounced attention surges, as their timing is anticipated and investors prepare accordingly (Drake et al., 2017; Engelberg et al., 2018; Lee et al., 2015).
- **Attention Narrowness:** Given limited attention, investors tend to focus on familiar or nearby firms. This is consistent with home bias (French and Poterba, 1991), geographic familiarity (Huberman, 2001; Bernile et al., 2015; Dyer, 2021), and peer-based learning across geographic or product-market categories (Peng and Xiong, 2006; Lee et al., 2015; Mengoli et al., 2024).
- **Spillover Potential:** When one firm receives heightened attention, it can spill over to economically related or geographically close peers. Evidence shows that investor attention co-moves across firm networks (Drake et al., 2017), industry clusters (Engelberg et al., 2018), and along dimensions of geographic proximity and product market relatedness (Lehmann and Posch, 2025).

These principles motivate our core research question: Do different types of corporate filings produce distinct attention spillovers, and how are these spillovers shaped by firm similarity and spatial proximity?

We begin with regular disclosures such as annual (10-K) and quarterly (10-Q) reports. These are scheduled, standardized, and widely anticipated events that investors use to benchmark firm performance. They are foundational to investor decision-making, and prior research documents significant market responses to these filings (Griffin, 2003). Because of their timing-wise predictability, attention surges are expected to be less driven by surprise and more by comparative analysis. In addition, prior work shows that investors benchmark firms within multidimensional clusters when interpreting such disclosures (Engelberg et al., 2018; Lee et al., 2015; Dyer, 2021), suggesting that attention spillovers are likely to concentrate among economically or geographically similar firms.

Hypothesis 1a: Regular disclosures of annual (10-K) and quarterly (10-Q) reports generate stronger spillovers for product-market similar firms.

Hypothesis 1b: Geographic proximity amplifies attention spillovers following regular disclosures.

Insider trading disclosures (Forms 3, 4, 5) differ notably. Insider trades are typically viewed as signals of private information, and their informativeness has been well documented in the literature (Brochet, 2010; Lakonishok and Lee, 2001; Mansi et al., 2025). Although some transactions may be prearranged, the exact timing of filings is not known ex ante. Research shows that insider trades convey meaningful information and attract heightened scrutiny (Lakonishok and Lee, 2001; Brochet, 2010; Wang, 2019; Mansi et al., 2025). These characteristics suggest that insider filings should generate sharper attention shifts that propagate through product-market and geographic linkages.

Hypothesis 2: Spillover effects are more intense for unexpected, sensitive disclosures (insider filings) than for routine scheduled filings.

Finally, 8-K filings disclose significant corporate events, including mergers, leadership changes, and earnings updates, and must be filed within four business days of the event. These disclosures vary in surprise and scope but often carry broader industry implications. Event-driven filings have been shown to trigger market-wide institutional attention reallocations (Ben-Rephael et al., 2022). Unlike insider filings, which primarily convey firm-specific signals, and periodic filings, which largely serve as benchmarking documents, many 8-K disclosures relate to events with relevance beyond the focal firm. As a result, attention triggered by 8-K filings may diffuse beyond narrowly defined local peer groups and geographic markets, generating spillover patterns that are broader and less geographically concentrated.

Hypothesis 3: Significant event disclosures (8-K) trigger attention shifts that extend beyond local markets.

Because Form 8-K filings cover a wide range of event types, we further refine our analysis of attention spillovers by examining the heterogeneity across the SEC’s top-level 8-K item categories¹. This item-level analysis allows us to assess whether spillover patterns differ systematically across event types and offers additional insight into how the nature of disclosed information influences the scope and diffusion of investor attention. In doing so, we deepen our understanding of filing-level heterogeneity in attention spillover dynamics.

Together, our hypotheses provide a testable framework for understanding how information spreads across firms and markets, contributing to broader debates on information efficiency and investor behavior.

Prior work, including Lehmann and Posch (2025), documents that attention to one firm can spill over to economically related or geographically close peers, but relies on aggregate EDGAR activity and therefore treats all filings as a homogeneous information source. We advance this literature by explicitly accounting for filing-type heterogeneity. By decomposing EDGAR searches into attention directed to regular filings (10-K/10-Q), insider filings (Forms 3,4 and 5), and event-driven filings (8-K), we show that the scope, magnitude, and spatial reach of attention spillovers differ systematically across disclosure types. This filing-level perspective reveals attention networks that are invisible in aggregate data and helps explain why some disclosures generate localized spillovers while others trigger broader reallocations of investor attention. Our study thus complements the findings of Lehmann and Posch (2025). While that paper documents the existence of proximity-based attention spillovers, we focus on which types of information generate localized versus widespread spillovers and how disclosure characteristics shape the diffusion of attention across firms and markets.

The remainder of our paper is organized as follows. In Section 2, we describe the materials used and discuss the methods chosen. Our empirical results are presented in Section 3, while Section 4 concludes.

3.2 Data and methods

3.2.1 Data

Our data preparation largely follows the methodology of Lehmann and Posch (2025), with some modifications. Consistent with their approach, we focus on U.S. stock listed

¹See Table B.2 for a list of 8-K filing top-level items.

firms and use the SEC’s EDGAR log files from 2007 to 2017 as a proxy for institutional investor attention. A central innovation of this study relative to Lehmann and Posch (2025) is that, instead of relying solely on the total number of daily firm-level searches, we construct filing-type-specific attention measures that allow us to analyze spillovers separately for different filing types. In contrast to the total number of daily firm-level searches, we further disaggregate attention by filing type. While the EDGAR log files contain granular information on user search behavior, they do not directly report the filing type. Instead, each record includes an accession number that uniquely identifies the requested filing. To classify these filings, we rely on the SEC’s submission history, which provides a comprehensive list of all filings submitted by each filer, including their accession numbers and corresponding form types. Using this information, we disaggregate total attention into three filing-specific attention measures: regular filings (Forms 10-K and 10-Q), insider filings (Forms 3, 4, and 5, which reflect insider trading activity and changes in ownership), and 8-K filings. This filing-level classification is essential for examining heterogeneity in spillover dynamics across disclosure types. We then compute daily attention measures for each filing group, at the firm-date level.

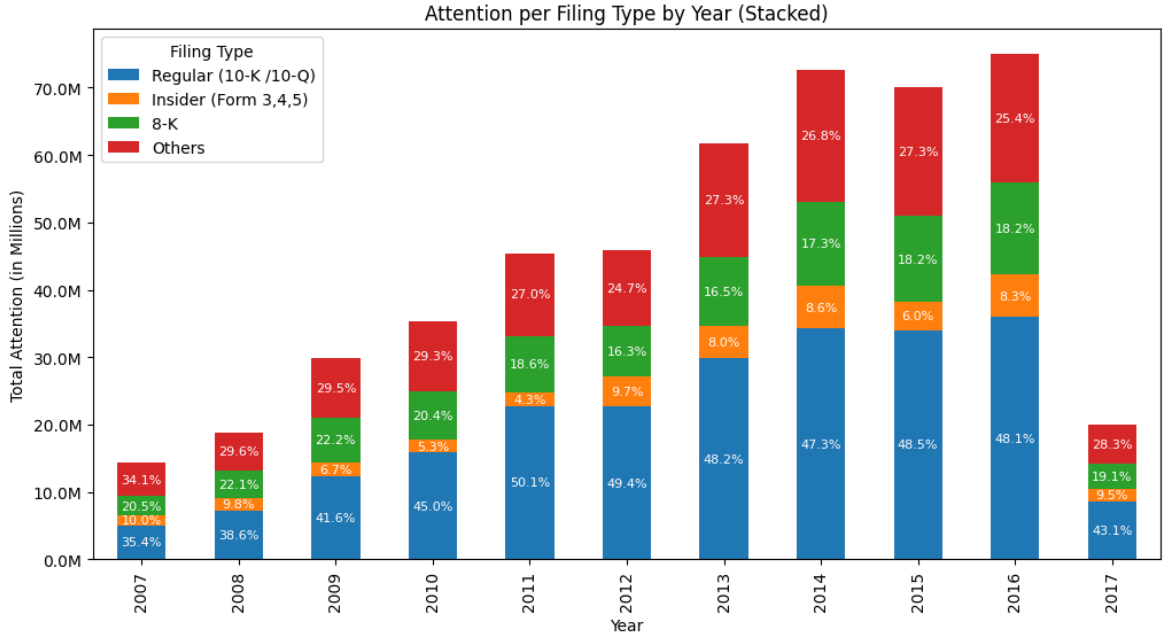


Figure 3.1: Distribution of investor access to EDGAR after data cleaning based on all EDGAR log files for January 1, 2007 to March 31, 2017.

Figure 3.1 shows the annual composition of total EDGAR-based investor attention across all firms considered, broken down by filing type. First, we observe a strong upward trend in EDGAR activity over our research horizon. While total attention amounted to broadly 14 million queries in 2007, this number increased to over 74 million by 2016. Due to data limitations in data availability, our sample covers only the first

quarter of 2017. However, with almost 20 million searches in that quarter alone, the upward trend appears to persist. Second, the majority of investor attention is directed toward regular filings, namely annual and quarterly reports. In 2007, 35.4% of total attention was allocated to regular filings. This share rose to approximately 50% in subsequent years. Insider filings consistently account for around 20% of total attention, while 8-K filings, though less frequently submitted, capture up to 10%. In conclusion, the three filing categories examined in this study account for roughly 75% of all EDGAR search activity of the considered firms. The remaining 25% of attention is dispersed across a variety of other filing types.

Subsequently, using both total and filing-specific attention measures, we identify firms experiencing abnormal increases in investor attention. For each filing type k , we compute a firm-specific attention Z-score based on filing-type-specific daily EDGAR requests $x_{i,t}^k$ and a rolling 90-day firm baseline with mean $\mu_{i,t}$ and standard deviation $\sigma_{i,t}$, defined as

$$Z_{i,t}^k = (x_{i,t}^k - \mu_{i,t}) / \sigma_{i,t}$$

Here, $\mu_{i,t}$ and $\sigma_{i,t}$ are computed from the firm's total EDGAR attention over the preceding 90 days, so that $Z_{i,t}^k$ captures filing-type attention spikes relative to the firm's overall attention baseline. We consider a firm i shocked if its daily attention toward a filing type $x_{k,i,t}$ exceeds three standard deviations $\sigma_{i,t}$ from their total 90-day average attention $\mu_{i,t}$. Because the Z-score is computed relative to each firm's total historical attention, the shock definition identifies days on which attention to a specific filing type pushes overall investor attention far above its baseline level. Similar as Lehmann and Posch (2025), we additionally require that the firm receives at least 250 total EDGAR requests on the respective date to ensure that shocks represent economically meaningful levels of investor attention. Table 3.1 reports summary statistics for all firm-date observations that meet these criteria. We identify 12,273 shocks related to regular filings (Forms 10-K and 10-Q), 1,909 for insider filings (Forms 3, 4, and 5), and 2,459 for 8-K filings.

Metric	Filing Type	Mean	Std	Min	25%	50%	75%	Max	Count
Filing Specific Attention	Regular	501.1	1,267.3	90.0	255.0	315.0	449.0	38,198.0	12,273
	Insider	509.8	1,188.0	101.0	241.0	315.0	507.0	46,195.0	1,909
	8-K	606.9	2,679.7	88.0	219.5	293.0	470.0	87,937.0	2,459
Total Attention	Regular	585.1	1,303.7	251.0	293.0	364.0	520.0	38,341.0	12,273
	Insider	713.2	1,446.6	251.0	306.0	405.0	658.0	53,201.0	1,909
	8-K	782.0	2,739.9	251.0	314.0	418.0	655.5	88,373.0	2,459
Z-Score	Regular	4.8	1.5	3.0	3.6	4.4	5.8	9.4	12,273

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Table 3.1 – continued from previous page

Metric	Filing Type	Mean	Std	Min	25%	50%	75%	Max	Count
	Insider	5.2	1.7	3.0	3.7	4.8	6.5	9.2	1,909
	8-K	4.6	1.5	3.0	3.5	4.1	5.4	9.3	2,459

Table 3.1: Descriptive statistics for firms experiencing attention shocks, defined by Z-score > 3 and minimum total attention ≥ 250 .

On average, 8-K filings receive the highest levels of attention. Notably, 8-K filings also exhibit the highest standard deviation in attention, consistent with their role in disclosing unexpected and often material events. Regular and insider filings show similar average attention levels, but insider filings stand out by generating the highest Z-scores, indicating larger relative spikes in attention compared to each firm’s historical baseline. Interestingly, despite attracting the most total and filing-specific attention, 8-K filings display slightly lower Z-scores than insider or even regular filings. This reflects the inherently higher baseline volatility in attention to 8-Ks, which makes even large jumps appear less extreme when standardized. In contrast, insider filings tend to be less frequent and more stable in attention, so spikes are more pronounced in relative terms. Overall, these statistics support that our approach effectively isolates economically meaningful attention shocks and that the magnitude and nature of these shocks vary systematically across filing types. This variation provides empirical support for our hypothesis that attention spillover dynamics depend on the type of corporate filing.

To further examine heterogeneity in attention spillovers following Form 8-K filings, we decompose 8-K-based attention shocks by the specific top-level items reported in the filings that trigger abnormal attention. Form 8-K is primarily used to disclose material corporate events, most of which are predefined by the SEC. Items 1 through 6 predominantly cover clearly defined, event-driven disclosures, such as material definitive agreements, acquisitions or dispositions of assets, and changes in control. While each of these items contains multiple sub-items referring to distinct events, we focus exclusively on the top-level item categories to ensure consistency and sufficient statistical power. By contrast, Items 7 and 8 are more discretionary in nature. Item 7.01 (Regulation FD Disclosure) is intended to ensure compliance with fair-disclosure requirements and is commonly used to furnish investor presentations, earnings call materials, or other narrative updates that may not correspond to a single discrete event. Item 8.01 (Other Events) serves as a residual category for material information that does not fall under Items 1 through 6 but that management nevertheless considers relevant for investors. By design, we do not account for Item 9, which is reserved for financial statements and exhibits. Item 9 disclosures typically accompany or reference information reported elsewhere and therefore reflect documentation associated with existing events rather

than independent news. Analysing Item 9 would primarily capture correlations with other item categories rather than distinct informational shocks.

For each previously identified 8-K attention shock, we identify the top-level item categories contained in the filings that receive abnormal attention on the shock date². A shock is assigned to a specific item category when filings containing that item account for more than 50% of total 8-K attention on that day. This procedure allows us to classify 8-K shocks by event type and to examine heterogeneity in attention spillover patterns across different categories of corporate disclosures.

The remainder of our data preparation also closely follows the methodology of Lehmann and Posch (2025). For each identified shock, we construct a control sample using observations from the same firm outside the immediate event window. Specifically, we randomly select four control dates per shock. Two occurring 20 to 35 days before, and two occurring 20 to 35 days after the shock. This symmetric window mitigates bias from anticipatory or lingering attention effects while maintaining temporal proximity.

After identifying shock and control dates, we construct firm-pair observations by matching each shocked or control record with all other U.S. stock-listed firms on the same date. This yields a comprehensive panel of firm-pair-day combinations. Finally, we enrich our sample with spatial distance measures based on firms' headquarter geolocations, and product-market similarity scores from the TNIC dataset. In addition, we incorporate firm-level accounting controls from Compustat and generate a binary variable indicating whether the peer firm released any filing within the previous 10 days.

Our final sample consists of approximately 185 million observations, covering all shocked and control firm-pairs across all filing types. Table 3.2 presents the descriptive statistics for the key variables.

	mean	std	min	25%	50%	75%	max	count
Distance	1155.08	788.20	0.00	529.99	999.94	1717.40	5147.91	185,635,603
$\log(1 + \text{Distance})$	6.66	1.16	0.00	6.27	6.91	7.45	8.55	185,635,603
ProductSimilarity	0.02	0.03	0.00	0.00	0.00	0.02	0.94	185,635,603
Attention	41.93	155.96	0.00	7.00	19.00	43.00	88373.00	185,635,603
Filing Released	0.59	0.49	0.00	0.00	1.00	1.00	1.00	185,635,603
Size _{Shocked}	8.31	1.97	-1.16	7.13	8.45	9.61	14.74	185,635,603
MBR _{Shocked}	1.73	2.13	0.00	0.83	1.25	2.01	116.39	185,635,603
Loss _{Shocked}	0.16	0.36	0.00	0.00	0.00	0.00	1.00	185,635,603
Leverage _{Shocked}	0.28	0.25	0.00	0.10	0.24	0.39	4.75	185,635,603
Cashflow _{Shocked}	0.04	0.13	-6.76	0.01	0.04	0.09	0.86	185,635,603
Size _{Peer}	6.83	2.06	-1.43	5.46	6.83	8.18	14.76	185,635,603

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²The current 8-K item structure was introduced on August 23, 2011. Earlier items were manually mapped to the new categories, as the existing items were only reordered and supplemented with additional items rather than completely replaced.

Table 3.2 – Continuation from previous page

	mean	std	min	25%	50%	75%	max	count
MBR_{Peer}	1.80	18.66	0.00	0.73	1.13	1.91	2650.12	185,635,603
$Loss_{Peer}$	0.24	0.43	0.00	0.00	0.00	0.00	1.00	185,635,603
$Leverage_{Peer}$	0.23	0.26	0.00	0.03	0.17	0.36	39.59	185,635,603
$Cashflow_{Peer}$	0.01	0.18	-24.51	-0.00	0.03	0.07	3.36	185,635,603

Table 3.2: Descriptive statistics for the sample of U.S. stock listed firms. Attention metrics are based on EDGAR log file entries between January 1, 2007, and March 31, 2017. Firm-level accounting variables are obtained from Compustat. The *Filing Released* dummy is based on submission dates from the SEC’s EDGAR filing history. Product similarity is derived from the TNIC dataset published by Hoberg and Phillips (2010, 2016).

Notably, our descriptive sample statistics are influenced by the research design, which includes four control dates for each shock date. The attention variable reported here reflects the number of EDGAR requests directed toward peer firms. The overall mean of 41.9 arises from averaging across both periods with potential elevated attention and a much larger number of control observations, during which no spillovers are expected.

Given that our sample includes all U.S. stock-listed firms³, the firm-pair distances range from near-zero to over 5,000 miles. The average distance between firm headquarters is approximately 1,155 miles, with considerable dispersion. Product-market similarity also spans a wide range, though the mean is reasonably relatively low due to the prevalence of unrelated firm-pair combinations. Control variables based on accounting data exhibit expected variation in firm size, market-to-book ratios, profitability, and leverage. All variables are not winsorized in order to preserve the full distribution of economic characteristics, including extreme but meaningful observations.

3.2.2 Methods

We conduct two analyses, both estimated using Poisson Pseudo-Maximum Likelihood (PPML). Our empirical setting follows a difference-in-differences (DiD) framework, in which we compare peer-firm attention on shock days to attention on selected control days for the same firm. By exploiting within-firm variation around attention shocks, this design isolates how disclosures by one firm are associated with short-run investor attention shifts toward its peers. PPML is well suited for this setting, as the dependent variable is a non-negative count of EDGAR requests that frequently includes zero observations, is strongly right-skewed, and exhibits substantial heteroskedasticity. Furthermore, PPML handles high-dimensional fixed effects efficiently (Correia et al.,

³Including firms in Alaska

2020), which is essential for our large firm-pair panel. As we are applying a DiD setting, this implies the assumption that, absent the shock, attention toward treated and control firm pairs would have evolved in parallel. To assess this assumption, we conduct a placebo pseudo-shock analysis in which the attention shock is replaced by a pseudo shock that is unrelated to actual abnormal attention events. The placebo test shows no systematic spillover effects, supporting the validity of our methodological specification (see Appendix Table B.1).

Our first regression analysis examines, for each filing group k , the effect of product-market similarity on attention spillovers and is defined as follows:

$$\begin{aligned} \text{Attention}_{i,t_0} = & \beta_1 \times \text{Shock}_{k,j,t_0} + \beta_2 \times \text{Product Similarity}_{i,j} \\ & + \beta_3 \times \text{Shock}_{k,j,t_0} \times \text{Product Similarity}_{i,j} \\ & + \sum_{l=0}^N \beta_{4+l} \times \text{Controls}_{l,t_0} + v_i + u_{t_0} + x_{i,t_0} + \epsilon_{i,t_0}. \end{aligned}$$

Here, $\text{Attention}_{i,t_0}$ denotes the total EDGAR attention directed toward peer firm i at time t_0 . The variable Shock_{k,j,t_0} is a dummy equal to one if firm j experiences an abnormal attention increase for filing type k at time t_0 . $\text{Product Similarity}_{i,j}$ measures the product-market similarity between firms i and j using the TNIC dataset (Hoberg and Phillips, 2010, 2016). Lastly, we include the respective interaction terms and control variables. As we aim to isolate spillover effects for each filing type, we include additional controls for co-occurring shocks. In some cases, a firm may experience abnormal attention in more than one filing category on the same day. If unaddressed, such co-shocks could confound the estimated effect of the focal filing type. To account for this, we add co-shock dummy variables for each other filing category, which equal one if the shock firm i simultaneously receives abnormal attention in another filing type. This ensures that the estimated spillover effect is attributable to the filing type under consideration rather than to overlapping attention shocks.

In line with Lehmann and Posch (2025), we adopt a comprehensive fixed-effects framework to ensure that our estimates of attention spillovers are not driven by persistent firm characteristics or market-wide information shocks. We include firm fixed effects v_i , which capture all time-invariant characteristics of peer firms i , such as industry affiliation, average size, typical disclosure practices, or their baseline level of investor attention. Daily time fixed effects u_{t_0} absorb shocks that affect all firms at t_0 , including macroeconomic announcements, sector-wide news releases, or regulatory events that could influence overall search activity. Furthermore, we include interacted firm x month-year fixed effects x_{i,t_0} to capture smoothly evolving, firm-specific conditions that change over time but not day-to-day. This level of granularity preserves short-run variation in

peer-firm attention around shock and control days, allowing us to identify attention spillovers. Although we control for fixed effects, we acknowledge that omitted variables may still influence both proximity and co-movement in attention. Standard errors are clustered multi-way by peer firm and date.

In our second analysis, we extend the previous regression equation by including distance bins $D_{i,j}^{l,l+1}$, which approximate the statistical quantiles of pairwise firm distances, following Lehmann and Posch (2025). This approach allows us to capture potential non-linearities in how geographic proximity moderates attention spillovers. By interacting these distance bins with our shock dummy and product-market similarity, we further assess whether geographic frictions amplify the diffusion of attention across economically related firms. All other variable notations, controls and fixed effects remain unaltered.

$$\begin{aligned}
Attention_{i,t_0} = & \beta_1 \times \log(1 + Distance_{i,j,t_0}) + \beta_2 \times Shock_{k,j,t_0} \\
& + \beta_3 \times ProductSimilarity_{i,j} \\
& + \beta_4 \times Shock_{k,j,t_0} \times ProductSimilarity_{i,j} \\
& + \sum_{k=1}^M \beta_{5,k} \times D_{i,j}^{k,k+1} \times Shock_{k,j,t_0} \\
& + \sum_{l=1}^M \beta_{6,l} \times D_{i,j}^{l,l+1} \times ProductSimilarity_{i,j} \\
& + \sum_{l=1}^M \beta_{7,l} \times D_{i,j}^{l,l+1} \times Shock_{j,t} \times ProductSimilarity_{i,j} \\
& + \sum_{l=0}^N \beta_{8,l} \times Controls_{l,t_0} + v_i + u_{t_0} + x_{i,t_0} + \epsilon_{i,t_0}
\end{aligned}$$

3.3 Results

In this section, we present the empirical findings from our two main analyses. We begin by examining investor attention spillovers that operate through product-market linkages. We then extend the framework by incorporating geographic proximity as an additional channel through which attention may diffuse across firms. In the discussion that follows, we interpret the PPML coefficients as percentage effects, computed as $e^\beta - 1$, while the tables report the coefficients in their original form. Table 3.3 reports the results of our PPML regressions analyzing investor attention spillovers driven solely by product-market similarity.

Across all filing types, the coefficient on the shock indicator is negative and statistically

significant. This pattern indicates that when one firm experiences an abnormal surge in attention, investor attention directed toward other firms temporarily declines. This finding is consistent with the notion of limited investor attention. Turning to product-market similarity, we observe substantial heterogeneity across filing types. For 8-K filings, the coefficient on product similarity is statistically insignificant, suggesting that similarity alone does not systematically shape baseline attention patterns for these event-driven disclosures. In contrast, product similarity exhibits a negative and statistically significant association with attention for both regular and insider filings. The corresponding estimates imply reductions of approximately 1.8% for regular filings and 12.6% for insider filings. This negative baseline association is consistent with attention substitution among close substitutes. In the absence of abnormal attention increases, investors concentrate on one representative firm within a product cluster rather than spreading searches evenly across all similar firms.

Allowing for interaction effects between attention shocks and product-market similarity reveals significant and positive spillover effects. The two-way interaction term $Shock \times Product\ Similarity$ is positive and highly significant for regular and insider filings, but insignificant for 8-K filings.

	Regular Filings	Insider Filings	8-K Filings
Main Term			
<i>Shock</i>	-0.0039*** (0.0004)	-0.0108*** (0.0028)	-0.0035*** (0.0009)
<i>ProductSimilarity</i>	-0.0189*** (0.0068)	-0.1353** (0.0620)	0.0327 (0.0239)
Two-way Interaction			
<i>Shock $\times$ ProductSimilarity</i>	0.0866*** (0.0211)	1.1214*** (0.3221)	0.0074 (0.0412)
Controls			
<i>log(1 + Distance)</i>	0.0004* (0.0002)	-0.0006 (0.0006)	0.0000 (0.0004)
<i>Co-Shock Regular</i>		-0.0073** (0.0035)	-0.0013 (0.0010)
<i>Co-Shock Insider</i>	-0.0108*** (0.0028)		-0.0175* (0.0093)
<i>Co-Shock 8-K</i>	-0.0019** (0.0008)	-0.0256** (0.0123)	
<i>Filing Released_{Peer}</i>	0.0954*** (0.0030)	0.0960*** (0.0039)	0.0978*** (0.0031)
<i>Size_{Peer}</i>	-0.0805** (0.0330)	-0.1485*** (0.0409)	-0.0989*** (0.0312)
<i>MBR_{Peer}</i>	0.0000 (0.0001)	-0.0003 (0.0003)	0.0000 (0.0002)
<i>Loss_{Peer}</i>	-0.0025 (0.0097)	-0.0077 (0.0130)	-0.0047 (0.0111)
<i>Leverage_{Peer}</i>	-0.0577 (0.0357)	-0.0285 (0.0318)	-0.0210 (0.0411)
<i>Cashflow_{Peer}</i>	0.0294 (0.0326)	0.0112 (0.0366)	0.0168 (0.0395)
<i>Size_{Shocked}</i>	-0.0000 (0.0000)	-0.0004*** (0.0002)	-0.0003** (0.0001)
<i>MBR_{Shocked}</i>	-0.0000 (0.0000)	-0.0001 (0.0001)	-0.0001 (0.0001)
<i>Loss_{Shocked}</i>	-0.0002** (0.0001)	-0.0013* (0.0008)	-0.0006 (0.0006)
<i>Leverage_{Shocked}</i>	0.0001 (0.0002)	0.0014** (0.0006)	0.0008** (0.0004)
<i>Cashflow_{Shocked}</i>	-0.0004 (0.0003)	0.0015 (0.0016)	-0.0000 (0.0005)

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Table 3.3 – Continuation from previous page

	Regular Filings	Insider Filings	8-K Filings
Daily Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Firm $\times$ Month-Year Fixed Effects	Yes	Yes	Yes
S.E. type	Clustered by: Firm and Time		
Observations	146,863,106	17,906,802	21,760,805
<i>Note:</i>			*p<0.1; **p<0.05; ***p<0.01
Format of coefficient cell: Coefficient (Std. Error)			

Table 3.3: This table shows PPML regression results analyzing the spillover effects of investor attention between firms for regular, insider, and 8-K filings, focusing on interactions with product similarity. Coefficients are shown with clustered standard errors in parentheses.

For insider filings, the two-way interaction term implies an economically large increase in peer attention of approximately 206%. Accounting for the negative baseline effect associated with the shock variable, the total spillover effect amounts to roughly 203%. This magnitude highlights the strong informational content investors attribute to insider trading activity, which appears to convey signals that are highly relevant not only for the focal firm but also for economically similar peers. Investors seemingly interpret insider transactions as informative about broader product-market conditions.

For regular filings, the two-way interaction coefficient estimation is positive but substantially smaller. The estimated coefficient implies an increase in attention of roughly 9%. Accounting jointly for the main effect of the shock variable, the overall spillover effect amounts to approximately 8.6%. Compared to insider filings, this more muted response is consistent with the view that much of the information contained in regular disclosures is anticipated by the market, limiting its incremental informational value.

In contrast, we find no statistically significant product-similarity-based spillovers. The absence of product-market spillovers for these filings likely reflects the heterogeneous and firm-specific nature of 8-K events, as well as the broad relevance of many such disclosures, which may not be confined to narrowly defined product markets.

Overall, these findings provide strong support for Hypotheses 1a and 2. Regular disclosures are capable of generating attention spillovers among product-market peers, but spillovers are substantially stronger for insider filings.

We next extend the baseline specification by incorporating geographic proximity as an additional dimension of investor attention spillovers. Table 3.4 reports the results, where attention shocks are interacted with distance bins capturing firm-pair headquarters

distance, alongside product-market similarity.

Consistent with the first analysis, the coefficient on the shock variable remains negative and statistically significant across all filing types, reinforcing the presence of attention crowding effects. Moreover, the interaction between shock and product similarity continues to be the primary driver of spillovers for regular and insider filings, underscoring the robustness of product-market linkages.

	Regular Filings	Insider Filings	8-K Filings
Main Term			
$\log(1 + Distance)$	0.0009*** (0.0003)	-0.0009 (0.0006)	0.0004 (0.0005)
<i>Shock</i>	-0.0085*** (0.0020)	-0.0319*** (0.0081)	-0.0048** (0.0024)
<i>ProductSimilarity</i>	-0.0303** (0.0151)	-0.1865* (0.0952)	0.0661* (0.0349)
Two-way Interaction			
<i>Shock</i> $\times$ <i>ProductSimilarity</i>	0.0927** (0.0365)	1.4576*** (0.3745)	-0.1202* (0.0718)
$D_{0,250} \times Shock$	0.0078** (0.0034)	0.0022 (0.0047)	0.0042 (0.0034)
$D_{250,500} \times Shock$	0.0067** (0.0027)	0.0030 (0.0058)	0.0014 (0.0035)
$D_{500,750} \times Shock$	0.0058** (0.0027)	0.0084 (0.0056)	0.0021 (0.0037)
$D_{750,1000} \times Shock$	0.0063** (0.0030)	0.0147* (0.0084)	0.0054 (0.0040)
$D_{1000,1250} \times Shock$	0.0059* (0.0031)	0.0141 (0.0101)	0.0014 (0.0043)
$D_{1250,1500} \times Shock$	0.0038* (0.0023)	0.0113 (0.0086)	-0.0020 (0.0037)
$D_{1500,1750} \times Shock$	0.0065*** (0.0021)	0.0152** (0.0074)	-0.0039 (0.0037)
$D_{0,250} \times ProductSimilarity$	0.0389** (0.0159)	0.0283 (0.0606)	-0.0019 (0.0356)
$D_{250,500} \times ProductSimilarity$	-0.0035 (0.0176)	0.0556 (0.0510)	-0.0169 (0.0418)
$D_{500,750} \times ProductSimilarity$	0.0171 (0.0218)	0.0790 (0.0671)	-0.0566 (0.0443)
$D_{750,1000} \times ProductSimilarity$	0.0169 (0.0196)	0.1072 (0.0742)	-0.0957** (0.0411)
$D_{1000,1250} \times ProductSimilarity$	0.0207 (0.0234)	0.1440** (0.0720)	-0.1080*** (0.0373)
$D_{1250,1500} \times ProductSimilarity$	-0.0308 (0.0249)	0.1376* (0.0729)	-0.0389 (0.0466)
$D_{1500,1750} \times ProductSimilarity$	0.0322 (0.0221)	0.0558 (0.0691)	-0.0806 (0.0591)
Three-way Interaction			
$D_{0,250} \times Shock \times ProductSimilarity$	-0.0350 (0.0369)	-0.2783** (0.1383)	0.0563 (0.0843)
$D_{250,500} \times Shock \times ProductSimilarity$	0.0144 (0.0468)	-0.4745*** (0.1367)	0.1383* (0.0818)
$D_{500,750} \times Shock \times ProductSimilarity$	-0.0102 (0.0428)	-0.5793*** (0.1640)	0.2356** (0.0962)
$D_{750,1000} \times Shock \times ProductSimilarity$	-0.0161 (0.0496)	-0.5769*** (0.1940)	0.1573* (0.0930)
$D_{1000,1250} \times Shock \times ProductSimilarity$	-0.0227 (0.0532)	-0.8352*** (0.1942)	0.2470*** (0.0867)
$D_{1250,1500} \times Shock \times ProductSimilarity$	0.0505 (0.0694)	-0.5965*** (0.1628)	0.2825*** (0.1006)
$D_{1500,1750} \times Shock \times ProductSimilarity$	-0.0538 (0.0509)	-0.3968* (0.2110)	0.3100** (0.1250)
Daily Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Firm $\times$ Month-Year Fixed Effects	Yes	Yes	Yes
S.E. type	Clustered by: Firm and Time		
Observations	146,863,106	17,906,802	21,760,805
<i>Note:</i> *p<0.1; **p<0.05; ***p<0.01			

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Table 3.4 – Continuation from previous page

Regular Filings	Insider Filings	8-K Filings
Format of coefficient cell: Coefficient (Std. Error)		

Table 3.4: This table shows PPML regression results analyzing the spillover effects of investor attention between firms for regular, insider, and 8-K filings. Key variables include distance and product similarity, with interaction terms to capture their combined effects. Control variables are included but not shown as they are stable compared to Table 3.3. Coefficients are shown with clustered standard errors in parentheses.

For regular filings, the dominant contributor to spillover effects is the interaction between attention shocks and product-market similarity. The corresponding estimate implies an increase in peer attention of approximately 9.7%. Interaction terms between shocks and distance are generally positive but economically small, with the strongest effects observed for firms located within 0 to 250 miles of each other. The three-way interaction terms are statistically insignificant, indicating that geographic proximity plays a limited moderating role beyond its direct interaction with the shock. Aggregating the statistically significant shock-related terms, the spillover effect for nearby firms (0 to 250 miles) amounts to approximately 9.6%, declining only slightly to around 9.1% at larger distances. Consistent with Hypothesis 1a, these results indicate that regular disclosures generate significant attention spillovers among product-market peers. In contrast, we find only weak evidence in support of Hypothesis 1b. Geographic proximity plays at most a limited moderating role, consistent with the view that information contained in regular disclosures is largely anticipated and processed through economic rather than spatial linkages.

For insider filings, spillover effects are economically and statistically pronounced. The two-way interaction between *Shock* and *Product Similarity* implies an increase in peer attention of approximately 329%, underscoring the exceptional informational relevance of insider activity. However, this effect is significantly moderated by geographic distance. The three-way interaction terms are negative and statistically significant across all distance bins, with the magnitude of moderation increasing in distance. Consequently, firms located closer to the shocked firm experience substantially larger spillovers than more distant peers. The total spillover effect ranges from approximately 216% for firms within 250 miles to 83% for firms located 1,000 to 1,250 miles away. These results further corroborate Hypothesis 2 and demonstrate that the intensity of attention spillovers from insider activity diminishes with geographic distance.

For 8-K filings, product similarity exhibits a positive and statistically significant main effect, corresponding to an increase in attention of approximately 6.8%. While the shock coefficient is weakly negative and significant, its interaction with distance is

insignificant, indicating that geographic proximity plays a limited role. The interaction between distance and product similarity reveals a moderating pattern: as distance increases, the effect of product similarity on attention declines, although statistical significance is confined to distances between 750 and 1,250 miles. In contrast, the three-way interaction terms are positive and statistically significant at distances above 250 miles, albeit without a monotonic pattern. Notably, the total spillover effect peaks at approximately 19.8% for distances between 1,500 and 1,750 miles.

One possible interpretation of this non-monotonic pattern is that geographically distant but economically similar firms may benefit disproportionately from firm-specific 8-K information. Unlike local peers, which often share overlapping investor bases and information environments, distant industry peers may rely more heavily on public disclosures to update beliefs about sector-wide conditions. In this setting, firm-specific events disclosed via 8-K filings may serve as salient signals for investors monitoring the broader industry, even when firms are geographically separated. As a result, attention spillovers need not decline monotonically with distance when disclosures convey information with industry-wide relevance. This finding suggests that for major corporate events disclosed via 8-K filings, investors do not restrict their attention to geographically proximate firms, thereby providing support for Hypothesis 3.

As 8-K filings can be triggered by a wide range of heterogeneous events, we further analyze attention spillover dynamics separately by the top-level items reported in the filings that trigger abnormal attention. Appendix Table B.3 presents the full PPML estimates, including distance bins and product-market similarity interactions. For Items 1–5, we do not observe any pronounced pattern in attention spillovers. Item 4 is estimated but, due to the very limited number of observations and lack of variation, the corresponding subsample is fully collinear and does not yield meaningful coefficient estimates. Results for Item 4 are therefore not reported. Likewise, Item 6 does not appear in our sample because none of the identified shock dates exhibit abnormal attention to filings containing Item 6.

In contrast, 8-K filings that contain Item 7 or Item 8 disclosures exhibit a distinct, non-monotonic relationship between geographic distance and attention spillovers. We observe that both items exhibit pronounced and statistically significant estimates. Item 7.01 (Regulation FD Disclosure) is typically furnished rather than filed, meaning that the information is submitted to the SEC for public dissemination but is not formally incorporated into the issuer’s periodic reports and is subject to a different liability regime than filed information. Item 8.01 (Other Events) can include both furnished and filed information, but in practice serves as a flexible channel for voluntary or supplemental disclosures that do not fit into Items 1 through 6.

Figure 3.2 shows the total attention spillover effect from a shocked focal firm to its peers.

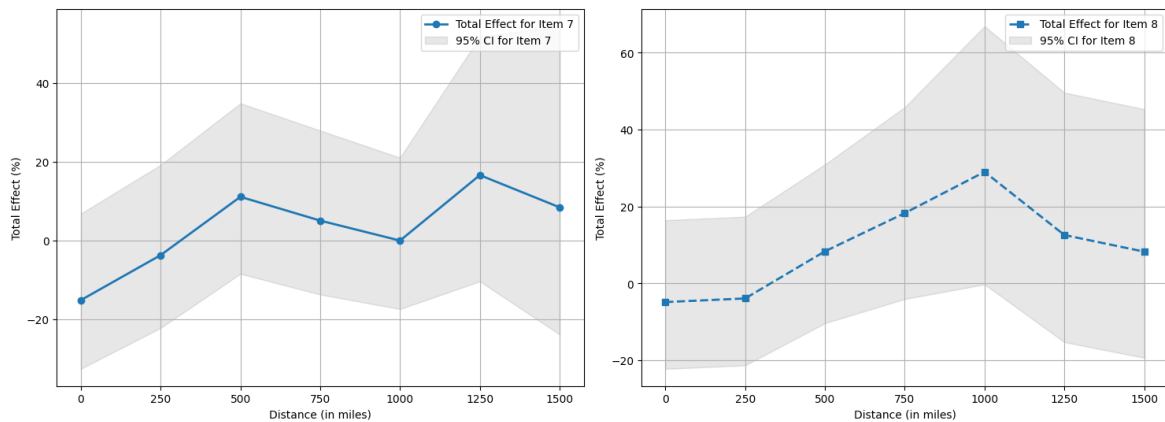


Figure 3.2: This figure shows the estimated total effect of an 8-K attention spillover on peer-firm attention across geographic distance bins, shown separately for Item 7 (Regulation FD) and Item 8 (Other Events). The total effect in each bin is computed as the sum of the shock main effect and the shock interactions with product similarity and distance-bins. Shaded areas denote 95% confidence intervals. Effects are reported in percentage.

For Item 7, the total spillover effect is slightly negative for the closest peers but turns clearly positive at intermediate distances (around 500–750 miles) and peaks again at longer distances, whereas Item 8 shows steadily increasing spillovers up to roughly 1,000 miles before declining, while remaining positive thereafter. This pattern is economically intuitive. Very local peers are already heavily monitored and often serve as close substitutes, so an attention shock to the focal firm partly crowds out incremental attention to its nearest neighbors. By contrast, more distant but product-market-similar firms are in an information shadow and benefit most when a salient 8-K event provides new, industry-relevant signals that investors can transfer to them. Item-7 disclosures typically convey broad guidance, strategic updates, or market-wide assessments, which helps explain why their spillovers are strongest for distant but similar firms that investors rarely analyze in depth. Item 8 (Other Events) comprise a more heterogeneous set of firm-specific events whose spillovers also strengthen with distance but follow a smoother, less pronounced pattern. Taken together, these results show that only the event-driven, discretionary 8-K items generate wide, distance-sensitive reallocations of investor attention across the firm network, whereas the more routine or tightly defined Items 1–6 predominantly affect the disclosing firm itself and do not produce comparable cross-firm attention waves. These item-level results for 8-K filings sharpen Hypothesis 3. Our results show that only 8-K items with broad, event-driven and largely discretionary content (Items 7 and 8) generate the wide, non-localized spillovers we predict, with

attention shifting to distant but economically similar peers, whereas items 1–5 do not display comparable distance-sensitive propagation and thus do not underpin the hypothesized market-wide reallocation of investor attention.

Overall, the results reveal substantial heterogeneity in attention spillovers across filing types. Spillovers are strongest for insider filings and more moderate for regular disclosures. Insider filings exhibit pronounced localization along product-market and geographic dimensions. In contrast, attention spillovers associated with 8-K filings that contain discretionary, signaling-type items such as Items 7 and 8 are not localized, suggesting that investors respond to these disclosures more broadly rather than concentrating their attention on economically or geographically proximate peers. These patterns underscore that the diffusion of investor attention depends critically on the nature of the underlying disclosure.

This is further supported by our robustness check presented in Table B.1, which reports the results of the placebo pseudo-shock analysis. Across both analyses, pseudo-shock coefficients and their interaction terms are economically small and largely statistically insignificant, with no consistent pattern across filing types. The absence of robust placebo effects supports the parallel trends assumption underlying our difference-in-differences framework and suggests that the spillover effects documented in the previously presented analysis are unlikely to be driven by pre-existing trends.

3.4 Conclusion

This paper examines how investor attention spills over across firms following corporate filings, emphasizing the role of product-market similarity, geographic proximity, and the heterogeneity of filing types. Using EDGAR search data from 2007 to 2017, we show that attention spillovers are neither uniform nor mechanical but depend systematically on the nature of the underlying disclosure. Our results indicate that insider filings generate the strongest attention spillovers. When investors focus on insider trading disclosures, attention shifts sharply toward firms that are both economically similar and geographically proximate. These spillovers are economically large, increasing peer-firm attention by more than 200%, and attenuate with distance. This pattern suggests that investors interpret insider activity as a highly informative signal about firm fundamentals and product-market conditions, prompting targeted search behavior among close substitutes.

In contrast, spillovers associated with regular disclosures, such as 10-K and 10-Q filings, are more moderate. Investor attention following these filings reallocates primarily toward product-market peers, with geographic proximity playing a secondary

role. The smaller magnitude of these effects is consistent with the view that much of the information contained in periodic filings is anticipated by the market, limiting its incremental informational value and, consequently, the strength of attention spillovers.

Finally, we find that attention spillovers originating from 8-K filings exhibit a distinct pattern. Spillovers associated with these event-driven disclosures are not geographically localized and often extend to firms that are far away. Our item-level analysis shows that this non-localized pattern is driven almost entirely by discretionary, signaling type items such as Item 7 (Regulation FD) and Item 8 (Other Events), which generate sizable spillovers to distant but economically similar peers. In contrast, more routine items (such as Items 1-6), which report predefined and mandatory triggering events, do not display comparable patterns. These results suggest that major, interpretative 8-K events attract broad investor attention that is not confined to local markets, whereas standardized 8-K items have only limited effects on peer-firm attention

Taken together, our findings contribute to the literature on investor attention and information diffusion by demonstrating that the reach and structure of attention spillovers depend jointly on economic relatedness, geographic frictions, and the type of information released. Rather than operating through a single channel, investor attention networks adapt to the informational content and scope of disclosures: attention concentrates among nearby and similar firms when information is precise and informative, but diffuses more broadly when disclosures signal major or wide-ranging events. These patterns help shed light on observed co-movement in attention, trading activity, and volatility among related firms and underscore the importance of accounting for filing-type heterogeneity when studying information spillovers in financial markets.

4 How Investor Attention Conditions Market Reactions to Female Board Appointments

4.1 Introduction

Although awareness of diversity and gender equality has increased over recent decades, women remain substantially underrepresented on U.S. corporate boards. Among Fortune 500 firms, the share of female directors has grown by less than one percentage point per year on average since the mid-90s, reaching only 30.4% as recently as 2022 (Pew Research Center, 2023). Thus, a growing literature studies the determinants for this inequality. Hillman et al. (2007) show that women's representation on boards is systematically tied to a firm's size and industry, but also, notably, to whether the firm's directors already sit on other boards alongside women. At the group level, token dynamics imply that when women remain a small minority, their integration and influence can be constrained in male-dominated settings (Kanter, 1977). Policy interventions can enforce female board representation but may also be priced by markets. Thus, the Norwegian quota is associated with a significant stock price reduction at the law's announcement and subsequent negative valuation effects (Ahern and Dittmar, 2012). At the same time, Adams and Ferreira (2009) shows that voluntary appointments of female directors are associated with meaningful changes in board governance and firm performance. Consistent with this, Carter et al. (2003) find that board diversity is positively related to firm value among Fortune 1000 firms, suggesting that investors can also recognize the governance benefits of female representation. Moreover, evidence on new appointments suggests that female directors often enter boards with strong observable human capital profiles, consistent with high selection thresholds (Singh et al., 2008). Understanding how investors respond to board appointments is therefore not only a question of equality, but also an empirical question about how markets process information about leadership and governance changes.

In this context, a growing strand of research evaluates stock market reactions to female leadership appointments, with mixed findings. Early event-study evidence documents negative abnormal returns around female CEO appointment announcements (Lee and James, 2007). Related evidence for board composition changes similarly suggests

that investors can perceive incoming female directors as a more negative short-term information shock than comparable male appointments (Sanford and Tremblay-Boire, 2019). In contrast, other studies find that appointing women into senior leadership roles is associated with positive price responses on average, though the effect depends heavily on the industry’s existing gender composition and can even turn negative in male-dominated industries (Cook and Glass, 2011). More recent work highlights that visibility and attention can condition these reactions. For instance, Smith et al. (2021) shows that female CEO appointments that attract limited public attention can receive more favorable market responses, whereas high-attention appointments may elicit comparatively more negative reactions. Beyond return reactions, investors may also penalize board gender diversity by interpreting diversity as a signal about lower shareholder value commitment, leading to decreases in market value following increases in board diversity (Solal and Snellman, 2019). Taken together, the literature suggests that market responses to female appointments are not uniform and depend on context, salience, and interpretation.

A common theoretical explanation for negative reactions draws on role congruity theory (Eagly and Karau, 2002), which posits that female leaders may be perceived as less congruent with stereotypical leadership expectations, thereby lowering investor confidence. Importantly, however, the broader social environment can also shape firms’ propensity to appoint women. For example, Giannetti and Wang (2023) document that increases in public attention toward gender equality translate into more women being appointed to boards. This effect is largely confined to firms whose culture already favored gender equality beforehand. This suggests that both appointment decisions and investors’ interpretations may be shaped by time-varying attention and social salience.

Despite extensive evidence on announcement effects, the conditional factors through which gender translates into differential market outcomes remain comparatively underexplored. Most prior work focuses on the determinants of board diversity or on its long-run governance and performance consequences, rather than on the information-processing dynamics that shape short-run investor reactions at the moment of appointment. A natural candidate for such an information-processing channel is investor attention, which we take as our main focus. Because attention is scarce and investors cannot fully process all information at all times, limited-attention models predict slower and weaker incorporation of information into prices when events receive less attention (Hirshleifer and Teoh, 2003; Peng and Xiong, 2006). Empirical evidence similarly links inattention to delayed market adjustment and underreaction around information events (Dellavigna and Pollet, 2009; Hirshleifer et al., 2011). Importantly, attention is not homogeneous across investor types. Institutional investor attention facilitates

permanent price adjustment, whereas retail attention proxies are more consistent with temporary price pressure (Ben-Rephael et al., 2017; Da et al., 2011). If female and male board appointments attract systematically different attention, such differences could help explain heterogeneous price and volume responses.

This paper investigates whether the gender of newly appointed board directors affects investor attention and whether attention operates as a conditional factor linking gender to stock-market reactions. We analyze a comprehensive sample of board appointments by U.S. stock listed firms from 2007-2017, combining BoardEx data with SEC EDGAR log files and Compustat equity data. Prior work often relies on media coverage and broad public salience measures (Lee and James, 2007; Smith et al., 2021). While informative, these measures capture heterogeneous audiences and do not directly measure the targeted acquisition of regulatory information. By contrast, EDGAR log files provide a direct record of user demand for SEC filings and have been widely used to proxy information acquisition and attention to corporate disclosures (Drake et al., 2015; Ryans, 2017). Because EDGAR traffic reflects deliberate access to regulatory filings, it offers a particularly targeted lens on information acquisition around governance disclosure events.

To our knowledge, this is the first study to link EDGAR-based attention around board appointment announcements directly to the gender of the newly appointed director and to study attention as a moderator of gendered market reactions. We find that appointment announcements involving female directors generate a significantly smaller attention spike than male appointments on average, with heterogeneity across appointment types. The attention gap is concentrated in supervisory positions but reverses for executive and CEO appointments. In our market-reaction analysis, we show that higher attention mitigates the return penalty associated with female appointments while simultaneously amplifying the gender gap in abnormal trading volume. This asymmetric pattern is consistent with the idea that attention changes how investors process female appointments, supporting more cautious trading behavior even when price reactions become less negative. Although our sample ends in 2017, the slow pace of progress in female board representation indicates that the patterns we study are likely to remain relevant also in the current environment (Pew Research Center, 2023).

Our paper contributes to the literature in three ways. First, we provide direct evidence on gender differences in EDGAR-based attention around board appointment announcements, offering a filing-based measure of targeted information acquisition. Second, while much of the prior event-study literature focuses on CEO successions, we document heterogeneity across appointment roles and firm size. Third, we show that investor attention is not a uniform amplifier of market reactions. Instead, it has an

asymmetric moderating role. High attention reduces the return penalty associated with female appointments but increases the relative trading-volume gap. This distinction is important because it separates price adjustment from trading intensity. The same variation in attention that is associated with more favorable price incorporation is also associated with different trading responses across male and female appointments.

The remainder of the paper is structured as follows. Section 2 describes the data and sample construction. Section 3 outlines the empirical methodology. Section 4 presents the main results. Section 5 reports robustness checks, and Section 6 concludes.

4.2 Data

Our data construction proceeds in three main steps, assembling (i) board announcement data, (ii) a proxy for investor attention, and (iii) measures of stock market reactions in returns and trading volumes.

The backbone of our analysis is an announcement-level dataset of board appointments constructed from BoardEx, accessed via WRDS. BoardEx consolidates publicly available information and provides rich data on firms' board compositions, announcements, remuneration, and detailed director profiles, including employment and education histories, gender, age, and many other individual characteristics for both executive and non-executive directors.

We construct our sample by integrating BoardEx announcement records, employment histories, director individual details, and stock-listing information. We begin by restricting the universe to publicly listed U.S. firms whose exchange-listing periods overlap with our sample window from January, 1, 2007 to March, 31, 2017¹. Stock-listing information is obtained for the two major U.S. exchanges, NASDAQ and NYSE, using the SEC-API.io interface, which provides listing start and end dates together with firms' CIK codes and other identifiers.

BoardEx provides an announcement table containing, among other fields, the firm CIK code, announcement date, director name and ID, role title, effective date, and a free-text description of the announcement. After removing non-stock-listed firms, we further exclude committee-level announcements and require a valid effective date. We then enrich announcement records with information from the employment-history data, which contain more detailed information on role start dates and indicators for executive and board positions, and can be used to recover directors' prior board experience. Using director identifiers, firm identifiers, and role titles, we link each announcement to the

¹This sample window is chosen based on the representativeness and availability of our attention data source.

corresponding position in the employment history and exclude all announcements that do not correspond to board positions.

BoardEx does not provide fully structured information on the nature of the announcement itself. In particular, the dataset does not include an announcement type variable indicating whether a director is joining or leaving the board. Since the announcement content is only available as free text, we classify announcement types using regular-expression filters applied to the textual descriptions. First, we categorize announcements as joins, leaves, or transitions depending on whether the text indicates entry into a role, exit from a role, or both. Second, we identify specific leadership appointments by applying additional role-specific regular expressions that also consider the role field. Like the announcement description, the role field is non-standardized free text, meaning that the same position can appear fully spelled out, abbreviated, or combined with other roles the director holds. After extracting the announcement type and standardized role flags for key board positions, we clean the sample to retain only genuine new appointments. Specifically, we exclude all leave events and ensure that the remaining announcements are not related to the continuation of an incumbent. For example, a CEO event is classified as a new CEO appointment only if both the role field and the announcement text clearly indicate a genuine new appointment, and not merely that the incumbent CEO is assuming an additional role or remaining in office. This approach reduces false positives from disclosures that mention an executive title without implying a new appointment.

Finally, we enrich the dataset with director-level characteristics. From the BoardEx individual details table, we merge in gender and date of birth and use these to calculate age and time to retirement at the appointment date. Based on complete prior employment histories, we compute cumulative board experience, firm-specific board experience, and prior tenure within the appointing firm, carefully merging overlapping appointment intervals to avoid double-counting. The resulting dataset is a richly structured event-level panel of board appointments that underpins our analysis.

To proxy for investor attention toward the announcing firm, we use the SEC’s EDGAR log files, which record all user requests to EDGAR on a daily basis from 2003 to 2017 and again from 2020 onward². Prior work argues that EDGAR traffic is largely driven by professional, information-seeking users and is more closely tied to informed trading than broader proxies such as Google search volume, which capture a much larger retail and noise component (e.g. Da et al., 2011; Ben-Rephael et al., 2017; Li and Sun, 2022). We therefore deliberately focus on this institutionally tilted attention channel, which is particularly relevant for corporate governance events where large, sophisticated

²From 2020 onward the data granularity of the log files have significantly been reduced.

investors are the marginal price setters (e.g. Li and Sun, 2022). To exploit the richer information content of the historical log files, we restrict our attention data to the 2007–2017 period, for which the logs provide high-granularity information on each visit, including timestamps, pseudo-anonymized IP addresses, the requested resource, the corresponding firm identifier, the accession number of the filing, the response status, and further technical details. Although the granular EDGAR log files are already available from 2003 onward, we begin our sample in 2007. We do so to focus on the period in which EDGAR appears to have become a more actively used and economically meaningful information channel for investors, as suggested by the traffic data and consistent with evidence that investors rely on EDGAR filings for information acquisition (Loughran and McDonald, 2017). To capture genuine search behavior rather than mechanical or automated traffic, we preprocess the raw logs by applying the built-in bot flags and by removing crawler traffic, restricting to successful content requests, excluding IPs with more than 500 requests per day, and excluding IP–minute combinations associated with implausibly broad filing or firm activity, following the cleaning procedures of Ryans (2017); Köchling et al. (2021); Lee et al. (2015).

Because board appointments must be disclosed within four business days via an 8-K filing on EDGAR, each appointment event in our sample should have a corresponding EDGAR filing. However, BoardEx consolidates information from various public sources, and its announcement dates are not always the true public disclosure dates; in some instances, the effective date, a press release date, or another reference date is recorded instead. Since we rely on EDGAR log activity as our attention proxy and 8-K filings constitute mandatory regulatory disclosures, we treat the 8-K filing date as the most reliable proxy for the public announcement date, under the assumption that firms file in a timely manner and that the filing date closely coincides with the true announcement date within the permissible four-day window.

To assign the correct filing date to each observation, we leverage the SEC submission history, a publicly available dataset containing the complete filing history for all EDGAR registrants, including an item field that identifies the disclosed items. Board appointments are primarily reported under Item 5.02 - Departure of Directors or Principal Officers; Election of Directors; Appointment of Principal Officers. In some cases, appointments are also reflected in shareholder meeting voting outcomes filed under Item 5.07 - Submission of Matters to a Vote of Security Holders, or, more rarely, in definitive proxy statements (Form DEF 14A). Therefore, we match BoardEx records to SEC filing history in a sequential procedure. We first search for 8-K filings with Items 5.02 or 5.07 and match BoardEx observations to filing report dates using the BoardEx announcement date and, if needed, the effective date, allowing for a tolerance

window of seven calendar days³. Remaining unmatched observations are then compared to DEF 14A filings using a tighter one-day tolerance. For matched observations, we use the associated EDGAR filing date as the event date. We then use the date on which the filing have officially be disclosed on EDGAR as the announcement date⁴.

With the corrected announcement dates in hand, we compute attention measures for each board announcement using the cleaned EDGAR log data. Specifically, for each announcing firm we aggregate the total number of EDGAR filing requests in the five calendar days before and after the announcement date. In addition, we construct a 8-K-filing-level attention measure that captures the number of requests directed specifically at the 8-K filing containing the board appointment, which we use as a control variable in our regressions. Additionally, we use the filing history to construct a dummy variable, *FilingReleased*, which equals one if the firm released any other SEC filing (besides the 8-K containing the board announcement) in the ten days prior to the announcement. This variable allows us to distinguish attention related to appointment announcements from cases where the firm is likely already in the spotlight for other disclosures.

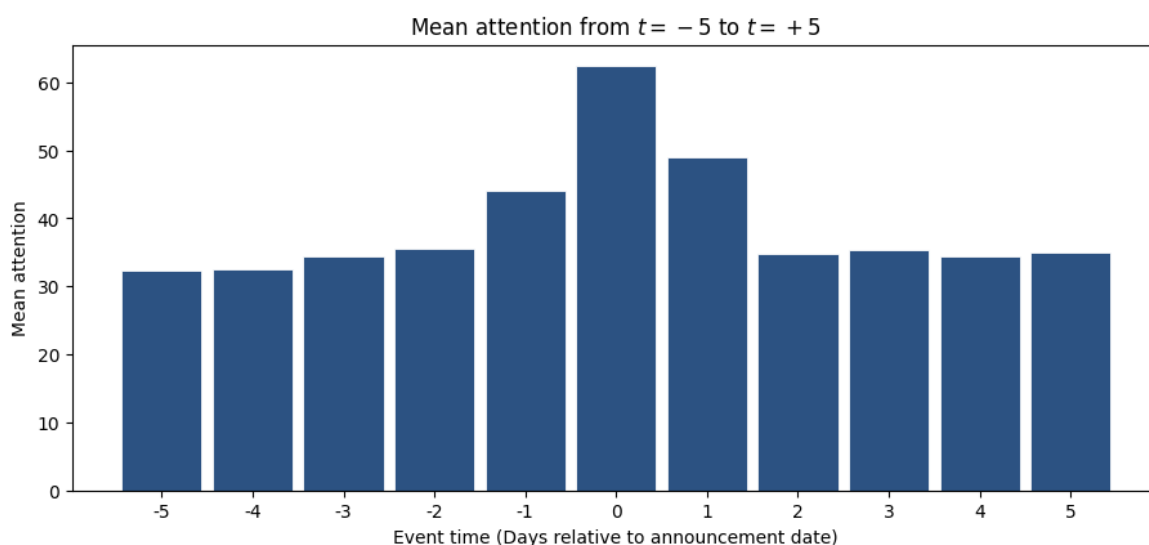


Figure 4.1: This figure shows the average number of EDGAR requests for the appointing firm from five calendar days before to five calendar days after the board appointment is released on EDGAR.

Combining the announcement-level and attention data yields a final sample of 17,544 board appointment events. Figure 4.1 plots the average unconditional attention profile

³Due to weekends, public holidays, and occasional reporting frictions, we allow for a seven rather than a four-day tolerance window.

⁴We validated the filing-matching procedure by manually inspecting a subset of announcements. In these spot checks, the matched SEC filings corresponded to the underlying appointment events, and we found no indication of systematic matching errors.

over the five calendar days surrounding the announcement date. Attention begins to rise on day $t - 1$, peaks sharply on the filing day $t = 0$, and reverts to its baseline level by about $t + 2$. This pattern suggests that some information acquisition starts shortly before the filing date, consistent with modest anticipation or information leakage, while the bulk of attention is concentrated on and immediately around the 8-K release itself, after which the announcement appears to be quickly absorbed.

As a final step, we obtain equity data to enable the second stage of our analysis, which examines market reactions to board announcements. We use Compustat as our source for daily closing prices, trading volume (number of common shares traded), and common shares outstanding. From these variables, we construct daily stock returns and turnover (trading volume scaled by shares outstanding). To mitigate the influence of data errors, illiquid trading days, and extreme outliers that could disproportionately affect our regressions, we winsorize both returns and turnover at the 0.1% level in each tail.

Next, we compute abnormal trading volume as excess turnover relative to the expected (mean) turnover⁵. To obtain abnormal returns, we follow Fama and French (1993) and merge our equity dataset with the Fama–French three factors, accessed via WRDS. Using the three-factor model, we then estimate abnormal returns over a five-day window around the board appointment announcement date. Cumulative abnormal returns (CAR) and trading volumes (CAV) are subsequently calculated, with CAR expressed in basis points and CAV scaled by 10^4 .

Subsequently, we merge the equity information into the announcement–attention dataset for the specified event window around the board appointment announcement. While in the first part of our analysis attention is measured in calendar days relative to the announcement date, in the second stage equity variables are defined over trading days. Consequently, when we use attention measures for the second analysis stage, we aggregate all EDGAR requests that fall into the corresponding calendar period up to the end of each event window based on trading days. This ensures that attention and equity reactions are aligned in time despite the different underlying day conventions. For 16,886 observations, equity data are available. For fewer than 1% of events, equity information is incomplete; we exclude these observations from the second-stage analysis when equity data are missing. Lastly, we augment the sample with Compustat’s GICS sector classification (*gsector*) to capture firms’ industry affiliation, which we later use to define industry fixed effects and to study heterogeneity across sectors.

Table 4.1 reports descriptive statistics for all key variables in our sample of 17,544

⁵The exact estimation windows and model specification for abnormal returns and abnormal trading volume are described in detail in Section 4.3.

board appointment events.

Variable	Name	Mean	Std. dev.	Min.	p_{25}	p_{50}	p_{75}	Max.	Count
Female appointee	female	0.12	0.33	0.00	0.00	0.00	0.00	1.00	17544
Attention count (pre-window sum)	attention_pre	178.8	469.2	0.0	37.0	80.0	171.0	17377.0	17544
Attention count (post-window sum)	attention_post	251.1	607.8	0.0	62.0	125.0	253.0	27234.0	17544
Attention count (full window sum)	attention_window	429.9	1036.5	1.0	105.0	210.0	430.2	42235.0	17544
Attention count (filing day only)	attention_t0	62.4	153.7	0.0	14.0	30.0	62.0	6263.0	17544
Age (years)	Age	56.06	8.51	23.00	50.00	56.00	62.00	90.00	17544
Tenure in firm (years)	TimeInCo	3.21	6.75	0.00	0.00	0.00	3.07	66.00	17544
Time to retirement	TimeRetirement	10.25	8.05	0.00	3.50	9.26	15.64	43.45	17544
Board experience, total (years)	BoardExpTot	7.03	7.80	0.00	0.46	4.62	10.64	57.66	17544
Board experience, company (years)	BoardExpCo	1.77	4.79	0.00	0.00	0.00	0.17	56.00	17544
Other filing re- leased	filing_released	0.68	0.47	0.00	0.00	1.00	1.00	1.00	17544
Executive director	executive	0.27	0.45	0.00	0.00	0.00	1.00	1.00	17544
Supervisor/non- exec.	supervisor	0.73	0.45	0.00	0.00	1.00	1.00	1.00	17544
CEO role	ceo	0.16	0.36	0.00	0.00	0.00	0.00	1.00	17544
C-suite/chief role	chief	0.19	0.39	0.00	0.00	0.00	0.00	1.00	17544
Log market capital- ization	log_marketcap	20.398	2.099	11.863	18.930	20.350	21.827	26.895	17544
CAR(1,5)	CAR_1_p5_w	13.81	707.22	-2301.14	-283.51	-3.98	277.68	2873.86	16886
CAV(1,5)	CAV_1_p5_w	44.36	366.49	-885.41	-98.13	-9.89	100.99	1929.17	16886
CAR(0,5)	CAR_0_p5_w	23.44	819.05	-2698.15	-319.99	1.12	325.77	3398.48	16886
CAV(0,5)	CAV_0_p5_w	65.10	443.47	-1040.46	-111.64	-8.38	132.22	2317.99	16886

Table 4.1: This table reports descriptive statistics for our sample of board appointment events. Attention, (abnormal) returns and trading volumes are shown for aggregated and selected event windows only, as results for more granular and alternative time periods are quantitatively similar.

Female appointees account for 12% of all board appointments in our sample, reflecting the well-documented underrepresentation of women on U.S. corporate boards. The typical appointee is 56 years old, has accumulated approximately seven years of total board experience across firms, and brings less than two years of company-specific board tenure. About 16% of appointments are to a CEO role, while executive directors represent roughly one quarter of all appointees. Supervisory directors account for the remaining

three quarters, consistent with the predominance of independent oversight roles on U.S. boards. EDGAR attention is on average higher in the post-event window than in the pre-event window, suggesting that the filing meaningfully raises investor information acquisition. The CAR and CAV measures exhibit substantial cross-sectional dispersion, underscoring the heterogeneity in market reactions across board appointments.

4.3 Methods

Our empirical analysis proceeds in two stages that jointly assess whether investor attention operates in a moderating role behind differential market reactions to board appointments by gender. In the first stage, we implement a difference-in-differences style setup to examine whether investor attention around board appointment announcements differs systematically by the gender of the appointee. In the second stage, we study whether market reactions to these appointments vary with investor attention. Together, the two stages allow us to trace the chain from announcement to attention to market reaction.

4.3.1 Stage 1: Attention Analysis

In our attention analysis, we adopt a difference-in-differences style framework by interacting an announcement dummy variable with the female-appointee indicator in a panel setting with firm and time fixed effects (Meyer, 1995; Bertrand et al., 2004). Figure 4.1 shows that EDGAR attention peaks on the filing day and drops back toward its baseline level shortly thereafter. We nevertheless define the announcement dummy $Announcement_{i,t}$ to equal 1 for $t \in [0, 2]$ and 0 otherwise. This choice reflects the notion that investors may access and process 8-K filings with a delay of one or two days and provides a small buffer for timing frictions in EDGAR usage and filing timestamps. By focusing on the window from $t=0$ to $t=2$, we thus capture the immediate post-announcement reaction of investors. As a robustness check, Section 4.5 confirms via the event-study analysis that the estimated gender differential is concentrated at $t = 0$ and remains qualitatively identical when the announcement window is restricted to $t = 0$ only (Figure 4.7, Panel B). The female indicator $Female_i$ equals 1 if the appointee is female and 0 otherwise.

Formally, we estimate the following regression equation:

$$\begin{aligned} \text{Attention}_{i,t} = & \beta_0 + \beta_1 \text{Announcement}_{i,t} + \beta_2 \text{Female}_i \\ & + \beta_3 (\text{Announcement}_{i,t} \times \text{Female}_i) \\ & + \gamma' X_{i,t} + \delta_i + \lambda_t + \varepsilon_{i,t}. \end{aligned}$$

where $\text{Attention}_{i,t}$ denotes the EDGAR-based attention measure for firm i on day t , $X_{i,t}$ is a vector of control variables as outlined before, δ_i are firm fixed effects, and λ_t are weekly fixed effects. The interaction coefficient β_3 captures the differential change in attention for female versus male appointments in the immediate post-announcement window and is our main parameter of interest.

We estimate our model with firm and week fixed effects to control for confounding heterogeneity while preserving the short-run variation around announcements that identifies our effects. Prior work, such as Hillman et al. (2007), shows that the likelihood of appointing female directors is systematically related to firm characteristics and existing board networks, which are highly persistent over time. Our choice of firm fixed effects absorbs these time-invariant traits, so our coefficients are identified from within-firm variation in appointment events rather than cross-sectional differences. Additionally, weekly time fixed effects capture common shocks in a given week such as macroeconomic news, regulatory changes, market sentiment, and seasonal reporting patterns, that may jointly affect attention and market reactions. We deliberately avoid more granular time fixed effects because they would remove almost all of the short-horizon variation in attention around the filing date. Standard errors are clustered at the firm level throughout to allow for arbitrary serial correlation and heteroskedasticity within firms over time.

Since our dependent variable $\text{Attention}_{i,t}$ is a non-negative count variable, we use a Poisson pseudo-maximum likelihood (PPML) estimator. Following Silva and Tenreiro (2006), PPML is well suited for such data because it naturally accommodates zero outcomes, does not require a log transformation, and remains consistent under fairly general forms of heteroskedasticity. Cohn et al. (2022) further show that PPML performs robustly in empirical settings with overdispersed count data and many fixed effects, which closely matches the structure of our analysis setting.

As a second step, we explore whether attention dynamics are heterogeneous across appointments. Specifically, we examine whether these dynamics vary with the role in focus and with the market capitalization of the appointing firm. To do so, we extend our baseline specification by including role dummies or the log of market capitalization as heterogeneity variables and their interactions with the announcement and female

indicators. This leads to the following regression equation:

$$\begin{aligned}
Attention_{i,t} = & \beta_0 + \beta_1 Announcement_{i,t} + \beta_2 Female_i + \beta_3 HeterogeneityVariable_{i,t} \\
& + \beta_4 (Announcement_{i,t} \times Female_i) \\
& + \beta_5 (Announcement_{i,t} \times HeterogeneityVariable_{i,t}) \\
& + \beta_6 (Female_i \times HeterogeneityVariable_i) \\
& + \beta_7 (Announcement_{i,t} \times Female_i \times HeterogeneityVariable_{i,t}) \\
& + \gamma' X_{i,t} + \delta_i + \lambda_t + \varepsilon_{i,t}
\end{aligned}$$

where :

$$HeterogeneityVariable \in \{Role, Log(MarketCap)\}$$

Variable definitions, the fixed-effects structure, controls, and the PPML estimator are identical to those used in our baseline attention specification.

Finally, as we are working with a difference-in-differences design at this stage of our analysis, we implicitly assume that, in the absence of an announcement, firms appointing female directors would exhibit similar attention dynamics to those appointing male directors. To assess and support this parallel-trends assumption, we perform in section 4.5 a robustness check in the form of a dynamic event-study of attention around the filing date, which traces the evolution of investor attention before and after the event. In particular, we compare pre-announcement attention paths for female and male appointments and show that effects are negligible, while differences emerge in the immediate post-announcement window.

4.3.2 Stage 2: Stock Market Reaction Analysis

In the second stage of our analysis, we shift our focus from attention to market reactions, adopting a standard event-study approach (Fama et al., 1969; MacKinlay, 1997). We examine two related but distinct measures of market reaction: cumulative abnormal returns (CAR) and cumulative abnormal trading volumes (CAV). CAR captures the net price effect of the announcement, reflecting investors' aggregated revision of firm value (e.g. Fama et al., 1969). In contrast, CAV measures the associated trading response, which reflects the degree of information processing and heterogeneity in investors' beliefs triggered by the event (e.g. Beaver, 1968; Kandel and Pearson, 1995).

First, we compute daily abnormal returns using the Fama–French three-factor model Fama and French (1993) over a 252-day estimation window, excluding the 10 days

immediately preceding the event.⁶ We require that at least 60% of daily return observations be available within the estimation window for a return to be included.

$$\begin{aligned}
 R_{i,t} &= \frac{prccd_{i,t} - prccd_{i,t-1}}{prccd_{i,t-1}}, \\
 E(R_{i,t}) &= R_t^f + \hat{\alpha}_i + \hat{\beta}_i(R_t^m - R_t^f) + \hat{s}_i SMB_t + \hat{h}_i HML_t, \\
 AR_{i,t} &= R_{i,t} - E(R_{i,t}), \\
 CAR_i(\tau_1, \tau_2) &= \sum_{t=\tau_1}^{\tau_2} AR_{i,t}
 \end{aligned}$$

Daily returns $R_{i,t}$ are calculated from end-of-day closing prices $prccd$ obtained from Compustat. R_t^f , R_t^m , SMB_t , and HML_t denote the Fama–French factors, and the parameters $\hat{\alpha}_i$, $\hat{\beta}_i$, $\hat{s}_i$, and $\hat{h}_i$ are estimated using firm-specific OLS regressions over the estimation window. Abnormal returns $AR_{i,t}$ are defined as the difference between realized and expected returns, and cumulative abnormal returns $CAR_i(\tau_1, \tau_2)$ are obtained by summing daily abnormal returns over the respective event window. To mitigate the influence of outliers, $CAR_i(\tau_1, \tau_2)$ is winsorized at the 1st and 99th percentiles.

Following the event-study literature on abnormal trading volume (Bamber, 1986; Campbell and Wasley, 1996), we define abnormal volume as the deviation of daily turnover from its firm-specific average level in the estimation window.

$$\begin{aligned}
 Turnover_{i,t} &= \frac{cshtrd_{i,t}}{cshoc_{i,t}}, \\
 E(Turnover_{i,t}) &= \frac{1}{T_i} \sum_{s \in \mathcal{E}_i} Turnover_{i,s}, \\
 AV_{i,t} &= Turnover_{i,t} - E(Turnover_{i,t}), \\
 CAV_i(\tau_1, \tau_2) &= \sum_{t=\tau_1}^{\tau_2} AV_{i,t}
 \end{aligned}$$

We apply the same estimation window as used for the abnormal return calculations. Daily turnover is computed as the ratio of the number of common shares traded

⁶Thus, the estimation period corresponds to the interval $[-262, -11]$ relative to the announcement day.

$cshtd_{i,t}$ to the number of common shares outstanding $csnoc_{i,t}$. The expected turnover $E(Turnover_{i,t})$ is given by the firm-specific average turnover over the estimation window. Abnormal trading volume $AV_{i,t}$ is then defined as the difference between daily and expected turnover, and cumulative abnormal trading volume $CAV_i(\tau_1, \tau_2)$ is obtained by summing daily abnormal volumes over the respective event window. Analogously, $CAV_i(\tau_1, \tau_2)$ is winsorized at the 1st and 99th percentiles to limit the influence of extreme observations.

With cumulative abnormal returns and turnover in hand, we first examine whether CAR and CAV differ unconditionally between firms appointing female and male directors, and whether CAR and CAV associated with female appointing firms differ statistically from zero in the post-announcement period. To this end, we use a standard one-sample t-test (Student, 1908) and a two-sample Welch t-test (Welch, 1947).

Subsequently, we examine conditional differences related to gender and various attention measures by adopting a standard event-study approach in the spirit of Fama et al. (1969); MacKinlay (1997), and estimate cross-sectional OLS regressions with quarter and industry fixed effects.

Thus, we estimate the following regression equation:

$$\begin{aligned}
Y_i &= \alpha_0 + \alpha_1 Female_i + \alpha_2 AttentionVar_i \\
&\quad + \alpha_3 (Female_i \times AttentionVar_i) + \theta' X_i + \mu_q + \eta_s + \varepsilon_i
\end{aligned}$$

where :

$$\begin{aligned}
Y_i &\in \{CAR_i(\tau_1, \tau_2), CAV_i(\tau_1, \tau_2)\} \\
AttentionVar &\in \{\ln(pre-event\ attention), \ln(attention\ on\ t = 0), \\
&\quad \ln(post-event\ attention(1,5))\}
\end{aligned}$$

Here, the dependent variable Y_i denotes either cumulative abnormal returns or cumulative abnormal trading volumes for firm i . As estimator, we rely on OLS for this cross-sectional specification because the dependent variables are continuous measures, and our interest lies in linear differential effects and interactions rather than in modeling higher moments. $Female_i$ and X_i are defined as in the previous analyses. $AttentionVar_i$ refers to three different log-transformed attention measures. Using multiple alternative log attention measures in separate specifications enables us to examine the role of attention more thoroughly. By comparing coefficient estimates across pre-event, event-day, and post-event attention specifications, we can assess at which stage of the information-processing timeline investor attention is most strongly associated with the gender-related differences in price and volume reactions. The interaction terms $Female_i \times AttentionVar_i$ capture whether attention moderates the

difference in market reactions to female versus male appointments. μ_q are quarter fixed effects that absorb common time shocks and market conditions within a calendar quarter, and η_s are industry fixed effects that control for systematic differences across industries. As in previous analyses, we cluster standard errors at the firm level.

Within our cross-sectional design, attention is likely endogenous to firm and event characteristics. We therefore acknowledge that our interaction estimates capture conditional associations rather than causal effects of attention. Establishing formal causal mediation is left for future research.

4.4 Empirical Results

4.4.1 Stage 1: Attention to Board Announcements

First, we assess whether board appointment announcements affect investor attention and whether announcements involving female appointees generate statistically different attention patterns than those involving male appointees. To this end, we estimate our baseline PPML regression, with coefficient estimates reported in Table 4.2 and the corresponding attention effects are visualized in Figure 4.2.

	Attention	Attention	Attention
Female		-0.0169 (0.0214)	-0.0127 (0.0213)
Announcement	0.3188*** (0.0187)	0.3352*** (0.0180)	0.3056*** (0.0174)
Announcement × Female		-0.1033*** (0.0366)	-0.0872** (0.0367)
TimeInCo	0.0029** (0.0012)	0.0025** (0.0012)	0.0021* (0.0011)
TimeRetirement	0.0031 (0.0034)	0.0030 (0.0034)	0.0026 (0.0033)
Age	0.0008 (0.0033)	0.0007 (0.0033)	0.0007 (0.0033)
BoardExpTot	0.0035*** (0.0008)	0.0033*** (0.0007)	0.0032*** (0.0007)
BoardExpCo	0.0006 (0.0017)	0.0008 (0.0017)	0.0004 (0.0017)
Filing Released	0.1757*** (0.0158)	0.1751*** (0.0158)	0.1653*** (0.0158)
8K-Attention			0.0007*** (0.0002)
Firm Fixed Effects	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes
Observations	192984	192984	192984
S.E. type	by: Firm	by: Firm	by: Firm
<i>Note:</i> *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$ Format of coefficient cell: Coefficient (Std. Error)			

Table 4.2: This table reports PPML regression estimates of the impact of gender in board appointment announcements on investor attention. Coefficients are shown with clustered standard errors in parentheses.

The first column of Table 4.2 presents the specification without gender variables. The coefficient on the announcement indicator is positive and highly statistically significant, indicating that board appointment announcements generate an increase in investor attention. Applying the standard PPML transformation $(\exp(\beta) - 1) \times 100$, the coefficient estimate implies that a board appointment announcement is associated with approximately a 37% increase in EDGAR-based investor attention on average. Across all three specifications, the announcement coefficient remains positive, economically large, and statistically significant, confirming that board appointment filings systematically and robustly attract investor attention.

In the second and third columns, we introduce the female indicator variable and its interaction with the announcement indicator. The coefficient on *Female* is small in magnitude and statistically indistinguishable from zero across both specifications, indicating that firms appointing female versus male directors do not differ systematically in baseline attention levels outside the announcement window. By contrast, the interaction term *Announcement* $\times$ *Female* is negative and statistically significant in both columns. This finding indicates that the increase in investor attention following board appointment announcements is smaller for female appointees than for male appointees. In the third column, we additionally control for filing-specific 8-K attention to account for the possibility that differences in attention are driven purely by variation in the consumption of the announcement filing itself. The interaction term remains negative and statistically significant, implying an 8.3% lower attention response for announcements involving female appointees. Taken together, these observations suggest that the attenuated attention response to female appointments is not solely driven by differences in filing readership, but may reflect broader differences in attention allocation.

Figure 4.2 complements the results by visualizing the attention profiles separately for male and female appointments, as well as the gender gap. The figure reveals that both male and female appointments generate a visible spike in attention. However, the attention response for male appointments is visibly larger in magnitude than for female appointments.

In the following, we further analyze whether the attention effects are heterogeneous across roles and firm size. We begin by examining role heterogeneity. Table 4.3 reports PPML regression estimates for appointments to executive, supervisory, CEO, and broader chief roles. While the executive and supervisory categories are distinct, chief roles form a subset of executives, and CEO appointments are a subset of both executives and chief positions.

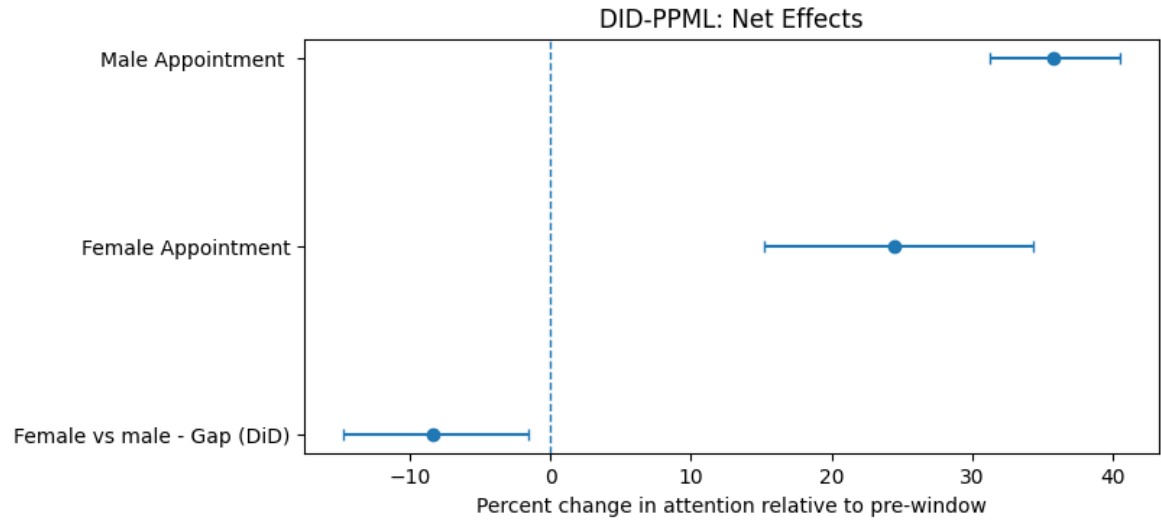


Figure 4.2: This figure visualizes the net attention effects of male and female board appointments, as well as the resulting gender gap, based on the regression results reported in Table 4.2.

	Attention	Attention	Attention	Attention
Female	-0.0054 (0.0220)	-0.0629 (0.0520)	-0.0081 (0.0216)	-0.0080 (0.0219)
Announcement	0.2712*** (0.0220)	0.3834*** (0.0203)	0.2868*** (0.0200)	0.2867*** (0.0203)
Announcement $\times$ Female	-0.0866** (0.0365)	0.1040 (0.0745)	-0.0923** (0.0361)	-0.0964*** (0.0364)
TimeInCo	-0.0012 (0.0014)	-0.0012 (0.0014)	-0.0001 (0.0012)	-0.0002 (0.0013)
TimeRetirement	0.0031 (0.0033)	0.0031 (0.0033)	0.0017 (0.0034)	0.0021 (0.0034)
Age	0.0019 (0.0032)	0.0019 (0.0032)	0.0004 (0.0033)	0.0007 (0.0033)
BoardExpTot	0.0035*** (0.0007)	0.0035*** (0.0007)	0.0036*** (0.0007)	0.0036*** (0.0007)
BoardExpCo	0.0028 (0.0018)	0.0028 (0.0018)	0.0028 (0.0017)	0.0029 (0.0017)
Filing Released	0.1693*** (0.0157)	0.1693*** (0.0157)	0.1698*** (0.0157)	0.1694*** (0.0157)
8K-Attention	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)
Executive	0.0420* (0.0216)			
Announcement $\times$ Executive	0.1122*** (0.0285)			
Female $\times$ Executive	-0.0575 (0.0539)			
Announcement $\times$ Female $\times$ Executive	0.1906** (0.0814)			
Supervisor		-0.0420* (0.0216)		
Announcement $\times$ Supervisor		-0.1122*** (0.0285)		
Female $\times$ Supervisor		0.0575 (0.0539)		
Announcement $\times$ Female $\times$ Supervisor		-0.1906** (0.0814)		
CEO			0.0583** (0.0227)	
Announcement $\times$ CEO			0.1042*** (0.0331)	
Female $\times$ CEO			-0.0458 (0.0538)	

Continued on next page

Table 4.3 – Continuation from previous page

	Attention	Attention	Attention	Attention
Announcement $\times$ Female $\times$ CEO			0.2578*** (0.0868)	
Chief				0.0508** (0.0216)
Announcement $\times$ Chief				0.0900*** (0.0301)
Female $\times$ Chief				-0.0412 (0.0552)
Announcement $\times$ Female $\times$ Chief				0.2478*** (0.0824)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes
Observations	192984	192984	192984	192984
S.E. type	by: Firm	by: Firm	by: Firm	by: Firm
<i>Note:</i>			*p<0.1; **p<0.05; ***p<0.01	
			Format of coefficient cell: Coefficient (Std. Error)	

Table 4.3: This table reports PPML regression estimates of how the impact of board appointment announcements on investor attention varies by role and gender. Coefficients are shown with clustered standard errors in parentheses.

The role-heterogeneity analysis shows that the gender gap in attention is not uniform across appointment types. For supervisory appointments, the interaction term *Announcement $\times$ Female $\times$ Supervisor* is negative and statistically significant, indicating that announcements of female supervisors attract less investor attention than comparable announcements of male supervisors. Quantitatively, the coefficient of about -0.19 implies that, relative to male supervisory appointments, announcements of female supervisory appointments receive roughly $[\exp(-0.1906) - 1] \times 100 \approx -17\%$ less attention.

By contrast, the triple interaction terms for Executive, CEOs and Chief roles are positive and statistically significant. For executive appointments, the estimate of about 0.19 implies that announcements of female executives receive approximately 21% more attention than those of male executives. The effect is even stronger for CEO and chief roles. The coefficients around 0.25 translate into an increase in attention of about 28% for announcements of female CEOs.

Substantively, these estimates imply that the negative average female-attention gap documented in the baseline specification is concentrated in less prominent supervisory appointments, whereas female appointments to executive and especially CEO positions generate comparatively stronger investor attention. This pattern is consistent with the notion that the salience and decision-making authority of the position shape how strongly investors react to the gender of the appointee. Gender differences in attention

are muted or even reversed when the appointment concerns senior roles with greater visibility and influence.

Figure 4.3 visualizes this role-specific heterogeneity in the gender gap by plotting the female–male difference in the announcement effect on investor attention for all appointments and separately per role. This representation complements the regression results and highlights how the direction and magnitude of the gender gap vary systematically across positions.

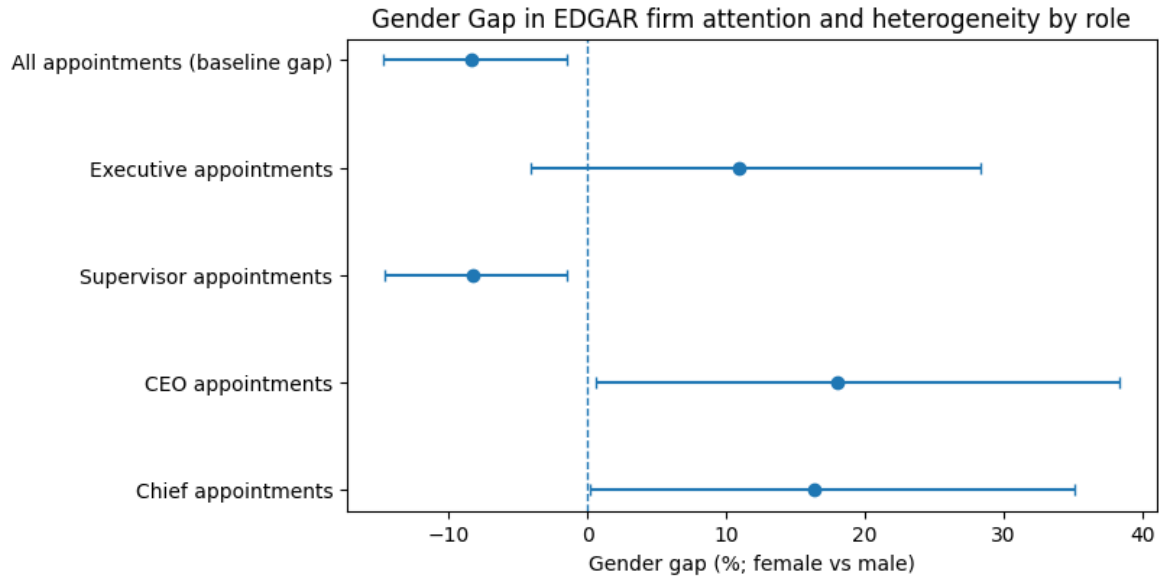


Figure 4.3: Gender gap in attention at the appointment announcement, by role. The figure plots the female–male difference in the announcement effect on investor attention based on the regression results in Table 4.3. For each role category, the estimate is the linear combination of $Announcement \times Female$ and the corresponding triple interaction term $Announcement \times Female \times Role$. The main effect of $Female$ is not included, as it cancels when comparing announcement versus no-announcement by gender.

As a second heterogeneity analysis, we examine whether the gender gap in investor attention varies with firm size, measured by log-transformed market capitalization. Figure 4.4 visualizes the estimated gender gap as a function of market capitalization by role, and the full regression results are reported in Table C.1 in the Appendix. The results indicate that firm size further conditions the gender–attention relationship.

The coefficient estimates show that the triple interaction $Announcement \times Female \times \log(\text{MarketCap})$ is small and statistically insignificant, providing little evidence of a uniform size gradient in the gender gap across all appointments. Once the analysis is disaggregated by role, however, stronger heterogeneity emerges. For executive, CEO, and chief appointments, the four-way interaction $Announcement \times Female \times Role \times \log(\text{MarketCap})$ is positive and highly significant, implying that the relative attention

advantage of female appointments increases with firm size in these senior roles. For supervisory appointments, the corresponding interaction is negative and significant, pointing to the opposite pattern.

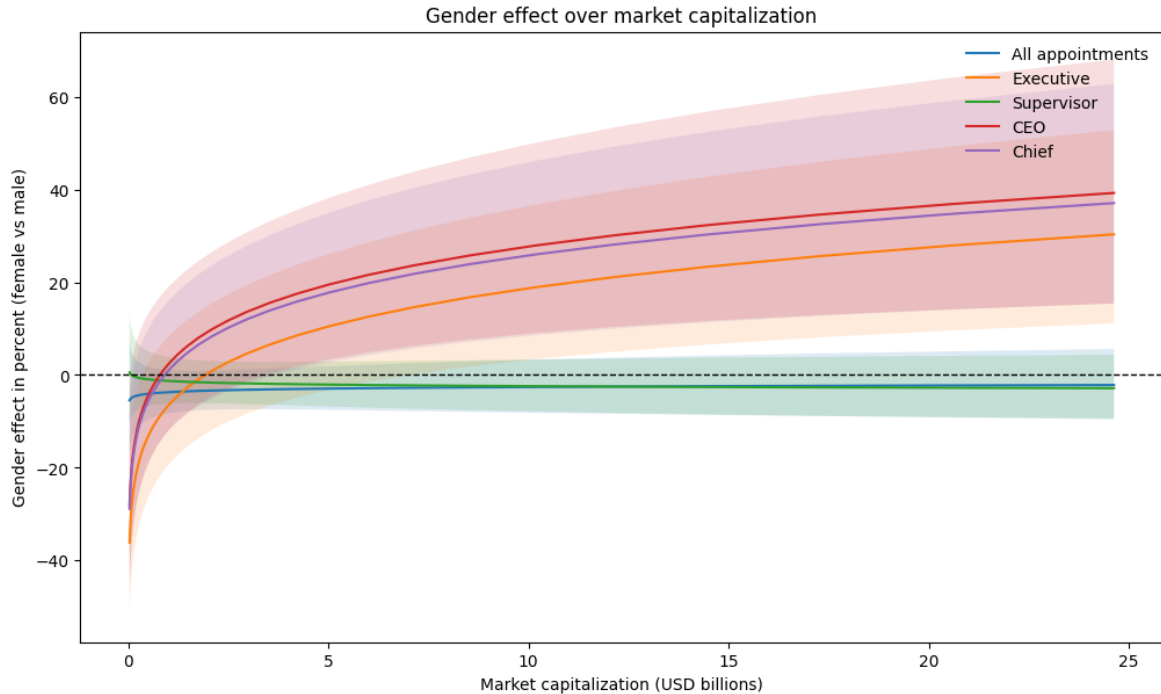


Figure 4.4: Gender gap in attention at the appointment announcement, by role and Market Capitalization. The figure plots the female–male difference in the announcement effect on investor attention based on the regression results in Table C.1.

Taken together, Figure 4.4 suggests that the attention penalty associated with female appointments is concentrated among smaller firms and attenuates. On the other side, it reverses into an advantage as market capitalization increases, but only when the appointment concerns a senior executive position. For supervisory appointments, larger firm size does not similarly benefit the attention response to female appointees. Overall, the market-capitalization heterogeneity results indicate that investor attention to female board appointments varies with both the prominence of the role and the visibility of the appointing firm. These heterogeneity results also bear on the interpretation of the baseline estimates. Given that the median firm in our sample is relatively small and attracts comparatively modest EDGAR-based attention (Table 4.1), the average gender gap in attention documented before is likely more representative of smaller, less closely scrutinized firms. For larger, high-attention firms, the direction and magnitude of the effect depend on the role of the announced appointee.

4.4.2 Stage 2: Price and Trading Reactions (Abnormal Returns and Volumes)

The first stage of our analysis documents systematic differences in investor attention following board appointments by gender. We now turn to the question of whether these differences matter for financial markets. In particular, we examine whether variation in investor attention translates into differential stock price and trading volume reactions, thereby shedding light on the role of attention as a moderating factor linking the gender of appointees to market outcomes.

As a first step, we present unconditional evidence on market reactions to board appointments. First, we compare how price and trading volumes evolve around the announcement date before introducing regression-based analyses. Figure 4.5 visualizes abnormal return and volume patterns around board announcements by gender.

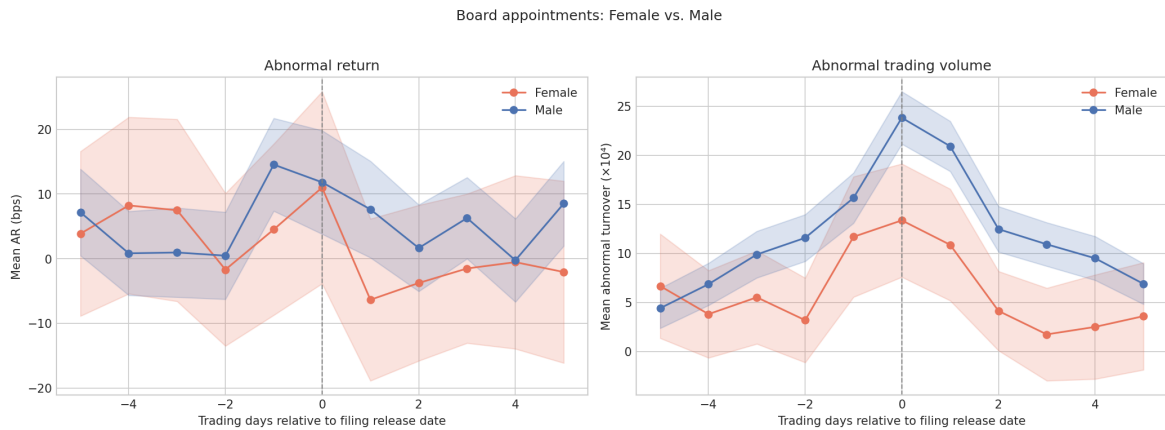


Figure 4.5: This figure plots average daily abnormal returns (left panel) and abnormal turnover (right panel) around board appointment announcements in the event window $t \in [-5, 5]$, with 95% confidence intervals.

The figure shows that price reactions are broadly similar across gender on the announcement day, with abnormal returns of comparable magnitude and no clear systematic pre-trends. However, post-announcement dynamics differ. Abnormal returns for female appointments decline on the day following the filing, while remaining positive for male appointments, although these differences are estimated imprecisely. In contrast, abnormal trading volumes exhibits more pronounced gender differences. Abnormal turnover increases sharply around the filing date for both groups but is consistently larger and more persistent for male appointments. This pattern suggests that while price responses are relatively similar and noisy, market participation reacts more strongly to male than to female board appointments in these unconditional comparisons.

To further assess the observed differences, we conduct two-sample Welch tests as well as one-sample t-tests for cumulative abnormal returns (CAR) and cumulative abnormal

volume (CAV). Table 4.4 reports the corresponding results.

Two-sample Welch (female vs. male)									One-sample (female vs. 0)	
Window	$\bar{x}_F$	std_F	N_F	$\bar{x}_M$	std_M	N_M	t-Statistic	p-Value	t-Statistic	p-Value
CAR(1,5)	-11.39	571.58	2089	17.37	724.28	14797	-2.076	0.038	-0.910	0.363
CAV(1,5)	17.67	331.16	2089	48.13	371.06	14797	-3.874	<0.001	2.439	0.015

Table 4.4: This table shows two-sample Welch tests comparing CAR(1,5) and CAV(1,5) between female and male board appointments, as well as one-sample t-tests evaluating whether the mean effects for female appointments differ from zero.

The two-sample Welch test accounts for unequal variances between female and male appointments and compares average cumulative abnormal returns and trading volumes across gender. The results indicate statistically significant differences between female and male appointments for both CAR and CAV, as reflected in the reported p-values.

The one-sample t-tests evaluate whether the average effects for female appointments differ from zero. While cumulative abnormal returns for female appointments are not statistically distinguishable from zero, cumulative abnormal trading activity is positive and statistically significant.

While the preceding analysis provides evidence of unconditional gender differences in market reactions, it does not establish whether these differences are related to variation in investor attention. We therefore estimate our regression model that jointly account for gender and attention measures. By interacting the female indicator with attention, we directly test whether investor attention amplifies or attenuates gender differences in cumulative abnormal returns and trading volumes.

We test three distinct attention measures. First, pre-event attention is defined as the sum of all requests back to the second trading day before the announcement. Second, we measure attention on the announcement day itself ($t = 0$). Third, post-event attention is defined as the total sum of attention in the event window from $t = 1$ to $t = 5$. Table 4.5 presents the OLS regression results. For reference when interpreting the results, the dependent variable CAR is measured in basis points. For CAV, the dependent variable is measured in units of 10^{-4} of the shares-outstanding-scaled turnover, so coefficients are directly interpretable as deviations in daily turnover fractions.

	CAR(1,5)				CAV(1,5)			
	Baseline (female indicator only)	Female × ln(pre-event attention)	Female × ln(attention on t0)	Female × ln(post-event attention (1,5))	Baseline (female indicator only)	Female × ln(pre-event attention)	Female × ln(attention on t0)	Female × ln(post-event attention (1,5))
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	-23.42* (14.41)	-106.25** (47.48)	-92.49** (44.27)	-111.75* (63.31)	-27.53*** (7.97)	22.72 (23.97)	12.34 (23.17)	41.66 (33.35)
Attention Var.		-6.99 (7.95)	2.54 (8.38)	-3.92 (9.15)		36.62*** (6.37)	53.87*** (5.49)	65.44*** (6.05)
Female × Attention Var.		20.64* (10.64)	19.63* (11.66)	17.55 (12.04)		-13.31** (6.19)	-11.95* (6.91)	-14.90** (7.04)
TimeInCo	0.78 (1.10)	0.86 (1.11)	0.74 (1.11)	0.82 (1.11)	-0.32 (0.53)	-0.77 (0.54)	-0.94* (0.54)	-1.05* (0.54)
TimeRetirement	6.63* (3.55)	6.55* (3.54)	6.74* (3.54)	6.62* (3.55)	-1.63 (1.67)	-0.96 (1.66)	-0.69 (1.64)	-0.42 (1.64)
Age	5.85* (3.33)	5.76* (3.32)	5.94* (3.32)	5.83* (3.33)	-1.38 (1.61)	-0.79 (1.60)	-0.45 (1.58)	-0.23 (1.58)
BoardExpTot	1.49* (0.85)	1.56* (0.87)	1.42* (0.87)	1.52* (0.87)	0.58 (0.41)	0.06 (0.41)	-0.21 (0.40)	-0.41 (0.40)
BoardExpCo	-2.84 (1.80)	-2.96* (1.82)	-2.76 (1.81)	-2.90 (1.82)	0.76 (0.80)	1.54* (0.81)	1.70** (0.80)	2.08*** (0.80)
Filing Released	-9.05 (15.47)	-6.29 (15.99)	-10.47 (15.58)	-8.39 (15.69)	4.72 (7.81)	-15.41* (8.32)	-12.46* (7.61)	-12.86* (7.67)
Industry (gsector)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16850	16850	16850	16850	16850	16850	16850	16850
S.E. type	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm

Note: *p<0.1; **p<0.05; *** p<0.01
Format of coefficient cell: Coefficient (Std. Error)

Table 4.5: This table reports OLS regression estimates of cumulative abnormal returns (CAR(1,5)) and cumulative abnormal trading volume (CAV(1,5)) around board appointment announcements. The baseline specifications include only the female indicator, while subsequent columns introduce interactions between the female indicator and measures of investor attention (pre-event, announcement-day, and post-event attention). All specifications include industry and quarter fixed effects. Coefficients are shown with clustered standard errors in parentheses.

In the baseline specifications, the coefficient on the female indicator is negative for both CAR and CAV, indicating that female appointments are associated with weaker market reactions relative to male appointments. This pattern is consistent with the unconditional evidence documented above.

Introducing interactions between the female indicator and investor attention reveals a more nuanced pattern. For cumulative abnormal returns, the coefficient on the female indicator remains negative and statistically significant, while the interaction terms are positive and statistically significant when attention is measured before as well as on the announcement day. This indicates that higher investor attention attenuates the negative return penalty associated with female appointments. In other words, when attention is high, the gap in price reactions between female and male appointments becomes smaller or may even reverse. The effect is strongest for pre-event attention and somewhat weaker for announcement-day attention. For the post-event window, the coefficient estimate remains positive but loses statistical significance⁷, suggesting that most of the attention-driven adjustment in returns occurs in the immediate pre-announcement and announcement periods.

In contrast, the results for trading volume show the opposite pattern. While investor attention is strongly positively associated with abnormal trading volume, the interaction terms between the female indicator and attention are negative and statistically significant across the different attention measures. This implies that the positive effect of attention on trading activity is systematically weaker for female appointments than for male appointments.

A potential interpretation for the opposite pattern in CAV is that attention changes who trades and how they trade, rather than just how prices adjust. However, prior research shows that abnormal trading volumes are closely tied to disagreement and heterogeneous beliefs among investors (Cookson and Niessner, 2020), suggesting that the lower CAV for female appointments reflects a more homogeneous investor response at the margin.

In this context, our negative interaction terms indicate that, conditional on a given attention level, female appointees attract a different investor clientele or behavior than male appointments. Attention to male appointments may disproportionately attract short-term or speculative traders who react more aggressively, generating larger volume responses (Barber and Odean, 2001). By contrast, attention to female appointments may involve relatively more long-horizon or governance-oriented investors who monitor information but adjust positions more gradually. This pattern is consistent with a more

⁷The interaction term $Female \times \ln(post - eventattention(1, 5))$ in the CAR specification yields a coefficient of 17.55 (s.e. 12.04, $p = 0.15$), which falls below conventional significance thresholds.

cautious, wait-and-see response, resulting in fewer immediate and aggressive position adjustments.

Figure 4.6 visualizes the total gender effect as a function of investor attention. The figure shows that different attention measures primarily shift the level of the effect, with stronger impacts observed for pre-event attention and attention on the filing day. Consistent with the regression results, cumulative abnormal returns increase with attention, while cumulative abnormal trading volume exhibits the opposite pattern.

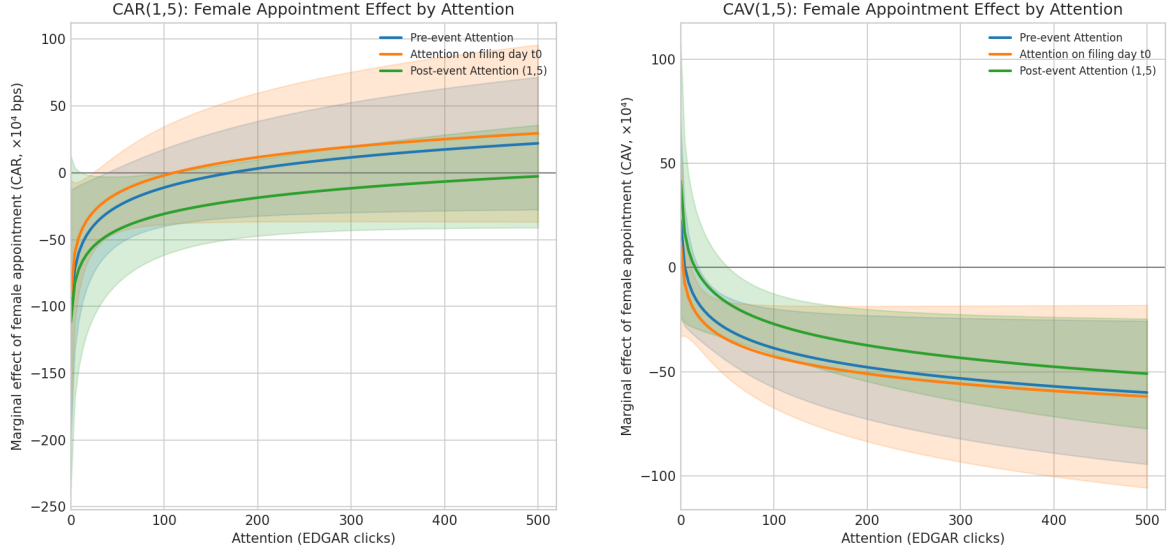


Figure 4.6: This figure shows the total female appointment effect on CAR and CAV by attention.

Taken together, these findings suggest that investor attention plays a differential role across market outcomes. For prices, attention mitigates gender differences in market reactions, whereas for trading activity, it amplifies the relative gap by generating stronger volume responses to male appointments. These results provide evidence that attention operates as an important conditioning factor that shapes how gender differences in board appointments are reflected in prices and trading activity.

To assess whether our findings vary across appointment types, we additionally estimate role-specific specifications for supervisory, executive, and CEO appointments. The results, reported in Table C.2 in the Appendix, are broadly consistent with the main findings in Table 4.5 and with the role heterogeneity documented in Stage 1. The triple interaction terms $Female \times Role \times Attention$ are largely statistically insignificant across role categories, with the exception of CEO appointments, where the interaction with CAR is negative and significant. Overall, we find no robust evidence that the attention-moderation pattern on CAR and CAV is uniformly role-specific, and interpret the results as broadly general with at most suggestive heterogeneity for senior executive

roles.

4.5 Robustness Checks

In the following section, we present a series of robustness checks for both stages of our analysis. First, we assess the robustness of the Stage 1 results on the impact of board appointment announcements on EDGAR-based investor attention. Second, we examine the robustness of the Stage 2 results on stock market reactions and the moderating role of EDGAR-based investor attention.

4.5.1 Stage 1 - Robustness

Event-Study-Analysis First, to validate our DID approach, we perform an event-study as a robustness check. We replace the announcement indicator with a set of event-time bins and estimate a PPML specification in which these bins are interacted with the female indicator. As before, we include the same controls as well as firm and time fixed effects. The omitted reference category is a *pre_far* bin, which covers days -5 and -4 relative to the filing date. Because attention may already starts to rise shortly before the announcement, we treat days -3 to -1 as an anticipation period and group them into an *anticip* bin. The announcement day $t = 0$ and the subsequent days $t = 1$ and $t = 2$ are modeled separately. Finally, we combine days $t = 3$ to $t = 5$ into a *post_long* bin. This design allows us to trace the timing of the gender differential around the announcement and to assess whether the main DID result is driven by pre-existing differences.

The estimates show that the gender gap emerges at the announcement itself, with the strongest negative effect on day 0, while the coefficients in the anticipation bin are small and statistically insignificant, indicating no meaningful pre-trend before the filing. Figure 4.7 visualizes the results and compares them with the DID estimates. It shows that the event-study pattern closely mirrors the DID evidence, with a negative effect for female appointments concentrated in the immediate event window and no comparable effect beforehand. Overall, the event-study supports our main analysis by confirming that the estimated gender gap is tied to the announcement period rather than to differential trends already present in advance.

Outlier Analysis Second, we assess the sensitivity of our results to extreme observations by excluding the ten firms with the highest average attention and re-estimating the baseline and role-heterogeneity specifications. This test is motivated by the concern that a small number of unusually visible firms may disproportionately affect our estimates.

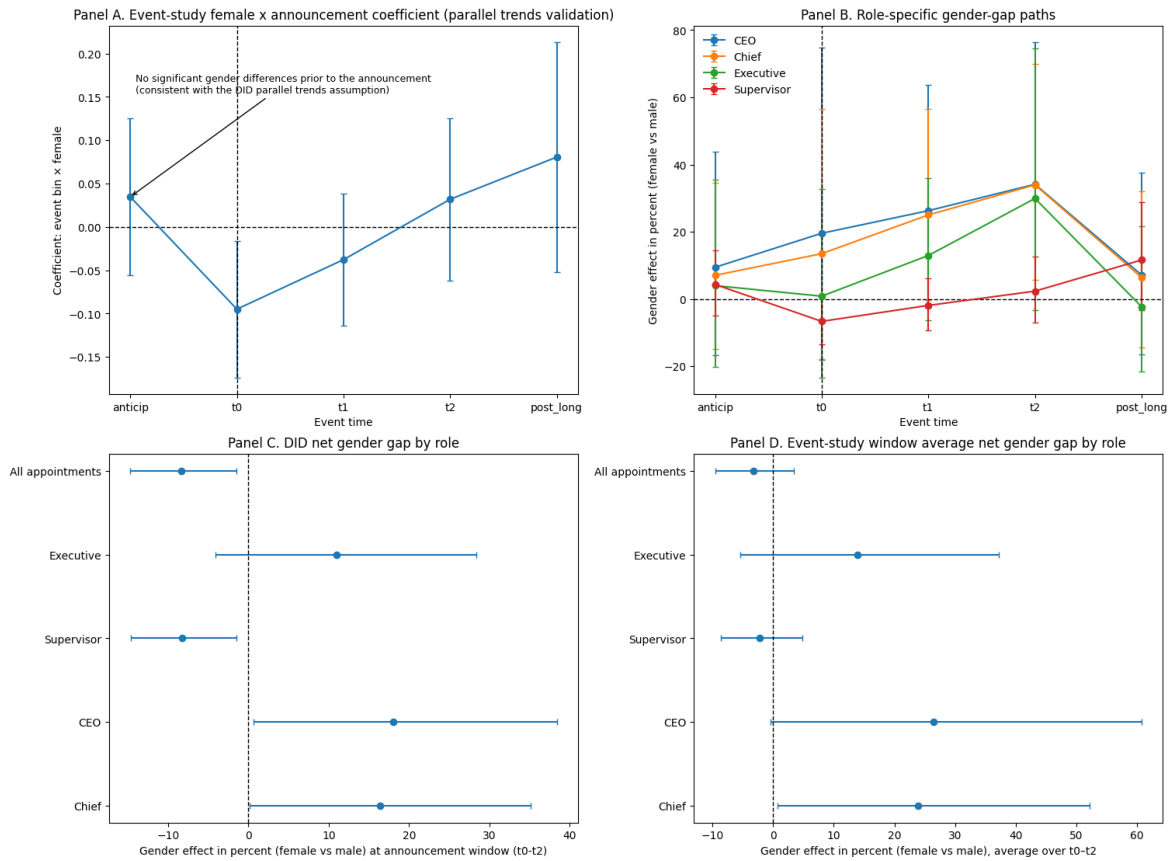


Figure 4.7: This figure presents the results of the event-study analysis (Panels A and B) and compares the estimated gender gap between the DID specification (Panel C) and the event-study estimates (Panel D). In Panel D, the event-study effects are averaged over days $t = 0$ to $t = 2$ to ensure comparability with the DID model, in which the announcement indicator also spans the same window.

We therefore remove all appointment announcements belonging to these ten firms from the sample, rather than dropping only ten individual events. Eliminating all observations for the most heavily scrutinized firms provides a stringent test of whether the estimated gender differential reflects a broad-based pattern rather than the behavior of a few highly salient firms.

Table 4.6 reports the results. The main coefficient of interest remains negative and statistically significant, with a very similar magnitude. In particular, the *Announcement* $\times$ *Female* interaction continues to be estimated at roughly -0.051 to -0.060 in the trimmed specifications, compared with about -0.087 in the full model. The three-way interactions also display patterns that are qualitatively similar to the baseline, although some of them lose statistical power, as expected when removing high-attention events. However, the signs and magnitudes remain comparable and are mostly still statistically significant. This stability indicates that the results are not driven by a handful of extreme-attention-firms, but instead reflect a robust pattern present across the wider sample.

Confounding Factors Third, we assess the sensitivity of our results to a more demanding fixed-effects structure by augmenting the baseline and role-heterogeneity specifications with Firm $\times$ Week fixed effects, in addition to firm and week fixed effects. This specification absorbs all firm-specific, week-varying shocks—such as idiosyncratic news clusters or disclosure patterns, to ensure that identification comes purely from within-firm, within-week variation between announcement and non-announcement days.

Table 4.7 shows that the *Announcement* $\times$ *Female* interaction remains negative and statistically significant, with magnitudes very close to the baseline estimates. The role-heterogeneity patterns are also preserved. The triple interactions for Executive, CEO, and Chief appointments remain positive and significant, while the corresponding coefficient for Supervisory roles is negative and significant. Some effects (e.g. CEO and Chief) even gain precision. Overall, these interacted fixed-effects results confirm that the documented gender differential in attention is robust to a substantially more saturated fixed-effects structure and is not driven by firm-week-level confounders.

	Attention	Attention	Attention	Attention	Attention	Attention
Female		-0.0079 (0.0161)	-0.0070 (0.0165)	0.0089 (0.0528)	-0.0036 (0.0163)	-0.0069 (0.0164)
Announcement	0.3407*** (0.0128)	0.3039*** (0.0149)	0.2852*** (0.0167)	0.3448*** (0.0211)	0.2917*** (0.0153)	0.2924*** (0.0152)
Announcement $\times$ Female		-0.0513** (0.0225)	-0.0546** (0.0235)	0.0621 (0.0826)	-0.0552** (0.0231)	-0.0598*** (0.0231)
Executive			0.0419** (0.0201)			
Announcement $\times$ Executive			0.0595*** (0.0228)			
Female $\times$ Executive			0.0159 (0.0533)			
Announcement $\times$ Female $\times$ Executive			0.1167 (0.0866)			
Supervisor				-0.0419** (0.0201)		
Announcement $\times$ Supervisor				-0.0595*** (0.0228)		
Female $\times$ Supervisor				-0.0159 (0.0533)		
Announcement $\times$ Female $\times$ Supervisor				-0.1167 (0.0866)		
CEO					0.0569*** (0.0212)	
Announcement $\times$ CEO					0.0671** (0.0280)	
Female $\times$ CEO					-0.0187 (0.0595)	
Announcement $\times$ Female $\times$ CEO					0.1482* (0.0816)	
Chief						0.0497** (0.0202)
Announcement $\times$ Chief						0.0538** (0.0248)
Female $\times$ Chief						0.0239 (0.0551)
Announcement $\times$ Female $\times$ Chief						0.1517** (0.0764)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	192115	192115	192115	192115	192115	192115
S.E. type	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm
<i>Note:</i>				*p<0.1; **p<0.05; ***p<0.01		
				Format of coefficient cell: Coefficient (Std. Error)		

Table 4.6: This table reports PPML regression estimates of the impact of board appointment announcements on investor attention after excluding all announcements from the ten firms with the highest average attention on announcement days. The specifications mirror the baseline and role-heterogeneity models. Control variables are included but omitted from the table for brevity. Coefficients are shown with clustered standard errors in parentheses.

	Attention	Attention	Attention	Attention	Attention
Announcement	0.3062*** (0.0175)	0.2721*** (0.0220)	0.3836*** (0.0205)	0.2872*** (0.0200)	0.2872*** (0.0204)
Female	0.0306*** (0.0118)	0.0284** (0.0116)	-0.0291 (0.0307)	0.0315*** (0.0115)	0.0326*** (0.0116)
Announcement × female	-0.0880** (0.0366)	-0.0873** (0.0364)	0.1011 (0.0744)	-0.0924** (0.0361)	-0.0966*** (0.0363)
Executive		-0.0401*** (0.0104)			
Announcement × Executive		0.1115*** (0.0285)			
Female × Executive		-0.0575* (0.0323)			
Announcement × Female × Executive		0.1883** (0.0812)			
Supervisor			0.0401*** (0.0104)		
Announcement × Supervisor			-0.1115*** (0.0285)		
Female × Supervisor			0.0575* (0.0323)		
Announcement × Female × Supervisor			-0.1883** (0.0812)		
CEO				-0.0386*** (0.0121)	
Announcement × CEO				0.1054*** (0.0331)	
Female × CEO				-0.0922*** (0.0350)	
Announcement × Female × CEO				0.2524*** (0.0865)	
Chief					-0.0331*** (0.0108)
Announcement × Chief					0.0907*** (0.0300)
Female × Chief					-0.0863*** (0.0327)
Announcement × Female × Chief					0.2437*** (0.0820)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm × Week Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	192984	192984	192984	192984	192984
S.E. type	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm
<i>Note:</i>				*p<0.1; **p<0.05; ***p<0.01	
Format of coefficient cell: Coefficient (Std. Error)					

Table 4.7: This table reports PPML regression estimates with interacted fixed effects mirroring the baseline and role-heterogeneity models. Control variables are included but omitted from the table for brevity. Coefficients are shown with clustered standard errors in parentheses.

4.5.2 Stage 2 - Robustness

Placebo Test We conduct a placebo test using a pre-announcement window before the filing date of the board appointment. Specifically, we estimate CAR and CAV regression over the interval $[-10, -5]$ relative to the announcement. The results in Table 4.8 show that the coefficient on the female indicator is small in magnitude and statistically insignificant for both CAR and CAV. This indicates that, in the absence of an announcement, firms that later appoint female directors do not exhibit systematically different pre-event return or trading patterns than firms appointing male directors. The placebo evidence therefore supports the identifying assumption that the documented gender differences around the announcement are not driven by pre-existing trends in prices or volumes.

	Placebo $CAR(-10, -5)$	Placebo $CAV(-10, -5)$
Female	-4.37 (16.29)	-3.78 (9.33)
Industry Fixed Effects	Yes	Yes
Quarter Fixed Effects	Yes	Yes
Observations	16739	16739
S.E. type	by: Firm	by: Firm
<i>Note:</i> *p<0.1; **p<0.05; ***p<0.01 Format of coefficient cell: Coefficient (Std. Error)		

Table 4.8: This table reports placebo test results examining whether firms that appoint female directors already behave differently in terms of returns and trading volumes prior to the announcement. The dependent variables are CAR and CAV over the pre-announcement window $[-10, -5]$. Control variables are included but omitted from the table for brevity. Coefficients are shown with clustered standard errors in parentheses.

Low-Overlap-Sample As abnormal returns and volumes are sensitive to the choice of estimation window, we examine whether our findings might be biased by overlapping board appointment events. Specifically, if a firm announces multiple directors within a relatively short period, the estimation window used to construct abnormal returns and volumes for a given event may already be affected by earlier appointments. We therefore identify, for each firm and announcement, whether there is a prior board appointment announcement in the preceding 120 days, which applies to roughly 19% of all observations. To obtain a cleaner benchmark, we remove these records from the sample and re-estimate our regressions on this subsample.

Table 4.9 reports the corresponding results. Overall, the estimates support the robustness of our main analysis. The main female coefficient on CAR becomes even more negative and statistically significant, while the female effect on CAV largely

vanishes. The attention coefficients remain strongly positive and highly significant for trading volumes, but small and insignificant for returns. The interaction term *Female* $\times$ *Attention* continues to be positive for CAR and negative for CAV, with similar or even stronger levels of statistical significance than in the full sample. These patterns indicate that the attenuating role of attention for the return penalty and its amplifying role for the volume gap between female and male appointments are not driven by contamination from earlier board events in the estimation window, but instead persist in a cleaner subsample of isolated appointment announcements. Overall, our results suggest that the reduction in noise from excluding contaminated announcements more than offsets the loss in sample size and translates into higher statistical power.

Alternative Windows As a further robustness test, we re-estimate the baseline regression using alternative, shorter event windows. For both CAR and CAV, we re-estimate our models over the windows $[0, 3]$ and $[1, 3]$. The results, reported in Table 4.10, closely mirror our baseline findings. The attention coefficients remain economically large and highly significant for abnormal turnover, while they are partially small and statistically insignificant for CAR.

Moving to shorter event windows, and in particular including the filing day in the $[0, 3]$ specification, introduces additional short-term noise in returns and reduces statistical power. As a result, some coefficients lose statistical significance. By contrast, the $[1, 3]$ window, which excludes the filing day and focuses on the immediate post-announcement adjustment, yields results for CAR that are more comparable to the baseline specification, suggesting that the main price effects materialize shortly after the announcement and are robust to alternative window definitions. Most importantly, the interaction term *Female* $\times$ *Attention* continues to be positive for CAR and negative for CAV across these alternative windows, often with similar levels of statistical significance. This pattern implies that higher attention mitigates the return penalty associated with female appointments but amplifies the gender gap in trading responses, and it does so robustly across different event-window definitions.

	CAR(1, 5)			CAV(1, 5)		
	ln(pre-event atten- tion)	ln(attention on filing day t0)	ln(post-event atten- tion)	ln(pre-event atten- tion)	ln(attention on filing day t0)	ln(post-event atten- tion)
Female	-117.34** (53.80)	-129.51*** (49.01)	-165.04** (69.87)	7.76 (26.12)	-4.14 (25.09)	34.79 (34.91)
Attention Var.	-5.53 (8.70)	2.33 (9.03)	-3.16 (9.75)	30.49*** (6.81)	48.57*** (5.73)	60.60*** (6.50)
Female $\times$ Attention Var.	23.01* (12.09)	29.88** (12.89)	27.83** (13.33)	-11.63* (6.66)	-9.67 (7.47)	-15.16** (7.35)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13553	13553	13553	13553	13553	13553
S.E. type	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm
<i>Note:</i>				*p<0.1; **p<0.05; ***p<0.01		
				Format of coefficient cell: Coefficient (Std. Error)		

Table 4.9: This table reports regression results for the Low-Overlap-Subsample that excludes all announcements with another board appointment by the same firm in the preceding 120 trading days. The specifications mirror the main analysis described in Section 4.3.2. Control variables are included but omitted from the table for brevity. Coefficients are shown with clustered standard errors in parentheses.

Window	Variable	CAR			CAV		
		ln(pre-event atten- tion)	ln(attention on fil- ing day t0)	ln(post-event at- tention)	ln(pre-event atten- tion)	ln(attention on fil- ing day t0)	ln(post-event at- tention)
(0, 3)	Female	-19.81 (42.89)	-60.64 (40.86)	-29.60 (58.25)	30.41 (22.03)	24.09 (22.08)	51.35* (31.47)
	Attention Var.	-3.50 (7.42)	4.59 (7.73)	5.64 (8.46)	40.35*** (5.34)	63.43*** (5.02)	74.22*** (5.63)
	Female $\times$ Attention Var.	2.54 (9.92)	14.39 (11.18)	3.91 (11.66)	-14.80** (5.78)	-14.82** (6.79)	-16.79** (6.92)
(1, 3)	Female	-73.34** (36.84)	-64.43* (35.27)	-64.78 (45.54)	7.58 (15.63)	5.36 (15.87)	16.14 (20.10)
	Attention Var.	-3.43 (5.69)	-0.29 (6.05)	-1.27 (6.37)	25.19*** (3.93)	38.09*** (3.68)	45.37*** (3.81)
	Female $\times$ Attention Var.	15.69* (8.58)	15.44* (9.65)	12.00 (9.87)	-6.92* (4.14)	-7.05 (4.82)	-8.39* (4.80)
Industry Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes
Observations		16850	16850	16850	16850	16850	16850
S.E. type		by: Firm	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm

Continued on next page

Table 4.10 – Continuation from previous page							
Window	Variable	CAR			CAV		
		ln(pre-event atten- tion)	ln(attention on fil- ing day t0)	ln(post-event at- tention)	ln(pre-event atten- tion)	ln(attention on fil- ing day t0)	ln(post-event at- tention)
Note:				*p<0.1; **p<0.05; ***p<0.01			
				Format of coefficient cell: Coefficient (Std. Error)			

Table 4.10: This table reports regression estimates of CAR and CAV for alternative event windows [0, 3] and [1, 3]. Control variables are included but omitted from the table for brevity. Coefficients are shown with clustered standard errors in parentheses.

4.6 Conclusion

This paper investigates whether gender matters for investor attention around board appointment announcements and whether attention operates as a moderator linking the gender of appointees to stock market reactions in the immediate post-announcement period. Using a large sample of U.S. board appointments and EDGAR-based attention data, we document a central asymmetry in how attention shapes market reactions to female board appointments. Attention appears to reduce gender differences in price reactions, but it increases gender differences in trading activity.

First, female board appointments generate approximately 8% less EDGAR-based investor attention than comparable male appointments on average. These gender effects are not uniform but depend on firm size and appointment role. The gap is concentrated in supervisory roles, where female appointments attract substantially less attention, but reverses for executive positions. In particular, for more senior roles such as CEO appointments, we observe comparatively stronger investor attention responses relative to male appointments. Heterogeneity by firm size further conditions this pattern, with the attention advantage for female executives being more pronounced at larger firms, which are likely more visible to investors.

Second, the gender gap in attention is economically meaningfully associated with market reactions. Higher investor attention mitigates the return penalty associated with female appointments while simultaneously amplifying the gender gap in abnormal trading volume. This asymmetric pattern suggests that attention is related not only to price adjustment, but also to which types of investors engage and how aggressively they trade. We find significant interaction effects between attention and gender, consistent with a clientele-based interpretation. One possible interpretation is that attention to male appointments disproportionately attracts short-term or speculative traders, whereas attention to female appointments may involve relatively more governance-oriented investors who adjust their positions more gradually. However, our data do not directly identify trader types, and we therefore treat this as a plausible but unverified interpretation.

Taken together, our findings suggest that gender differences in investor attention are a systematic feature of board appointment announcements and that such differences correlate meaningfully with variation in post-announcement market reactions. These patterns are consistent with differential information processing being an important factor that correlates with how gender differences in board appointments are reflected in financial markets.

Our paper makes three main contributions to the literature. First, we provide the first direct evidence linking EDGAR-based investor attention to the gender of newly

appointed board directors. Prior work on gender and market reactions has largely relied on media coverage or broad public salience measures (Lee and James, 2007; Smith et al., 2021). By contrast, EDGAR log files capture the deliberate acquisition of regulatory information by predominantly institutional and informed investors, offering a more targeted lens on information processing around governance disclosure events. Our approach thus complements and extends the existing event-study literature on female leadership appointments by introducing an EDGAR-based attention measure that is directly tied to the investor audience most likely to set prices.

Second, while much of the prior literature focuses on average announcement effects or CEO successions in isolation (Lee and James, 2007; Sanford and Tremblay-Boire, 2019), we document systematic heterogeneity across appointment roles and firm sizes. This reveals that the commonly reported negative average effect masks a rich underlying structure. The attention penalty for female directors is concentrated in supervisory appointments, while female executive and CEO appointments attract a premium in investor attention that is more pronounced at larger, high-visibility firms. This role- and size-conditional pattern helps reconcile seemingly contradictory findings in the prior literature and suggests that context is a first-order determinant of how markets respond to the gender of board appointees.

Finally, by showing that attention operates as an asymmetric moderator, we connect the gender and boards literature to the broader limited-attention literature (Hirshleifer and Teoh, 2003; Peng and Xiong, 2006; Dellavigna and Pollet, 2009). Our findings suggest that attention is not simply a scaling factor for announcement effects but shapes the nature of investor reactions in fundamentally different ways for prices and trading activity.

These results carry important implications for corporate governance practice and disclosure policy. For appointments in less salient roles at smaller firms, the attention gap for female directors may amplify market penalties, an effect that firms could potentially mitigate through more proactive disclosure strategies. More broadly, our findings imply that market reactions to governance changes depend not only on the content of the announcement but also on the composition of the investor audience it reaches. Within our cross-sectional design, attention is likely endogenous to firm and event characteristics, and our interaction estimates capture conditional associations rather than causal effects. Establishing formal causal identification remains an important avenue for future research.

A Appendix for Chapter 2

A.1 Empirical Results including Product Similarity

	Attention Increase $t+0$	Attention Increase $t+1$	Attention Increase $t+2$
Main Term			
$\log(1 + Distance)$	0.0003* (0.0002)	0.0002 (0.0002)	0.0001 (0.0002)
Shock	-0.0233*** (0.0054)	-0.0088* (0.0048)	-0.0025 (0.0026)
Product Similarity	-0.0084 (0.0174)	-0.0378** (0.0174)	-0.0545*** (0.0143)
Two-way Interaction			
$Shock \times ProductSimilarity$	1.1323*** (0.3647)	0.6305* (0.3249)	0.3035* (0.1730)
$D_{0,250} \times Shock$	0.0044** (0.0021)	-0.0008 (0.0020)	-0.0013 (0.0017)
$D_{250,500} \times Shock$	0.0101*** (0.0030)	0.0028 (0.0027)	0.0006 (0.0019)
$D_{500,750} \times Shock$	0.0123*** (0.0032)	0.0019 (0.0028)	-0.0017 (0.0020)
$D_{750,1000} \times Shock$	0.0125*** (0.0034)	0.0005 (0.0030)	-0.0024 (0.0020)
$D_{1000,1250} \times Shock$	0.0100*** (0.0033)	-0.0015 (0.0028)	-0.0022 (0.0024)
$D_{1250,1500} \times Shock$	0.0083*** (0.0029)	0.0026 (0.0026)	0.0006 (0.0023)
$D_{1500,1750} \times Shock$	0.0071** (0.0028)	0.0028 (0.0028)	-0.0023 (0.0022)
$D_{0,250} \times ProductSimilarity$	-0.0173 (0.0184)	-0.0119 (0.0179)	0.0180 (0.0162)
$D_{250,500} \times ProductSimilarity$	-0.0272 (0.0209)	0.0315 (0.0205)	0.0160 (0.0173)
$D_{500,750} \times ProductSimilarity$	0.0157 (0.0230)	0.0407* (0.0235)	0.0464** (0.0200)
$D_{750,1000} \times ProductSimilarity$	-0.0009 (0.0248)	0.0301 (0.0250)	0.0435** (0.0221)
$D_{1000,1250} \times ProductSimilarity$	0.0289 (0.0287)	0.0621** (0.0271)	0.0492** (0.0230)
$D_{1250,1500} \times ProductSimilarity$	0.0082 (0.0256)	0.0513* (0.0280)	0.0130 (0.0236)
$D_{1500,1750} \times ProductSimilarity$	-0.0237 (0.0272)	0.0493* (0.0292)	0.0295 (0.0265)
Three-way Interaction			
$D_{0,250} \times Shock \times ProductSimilarity$	-0.2239** (0.1045)	-0.0563 (0.0872)	-0.0290 (0.0603)
$D_{250,500} \times Shock \times ProductSimilarity$	-0.5557*** (0.2032)	-0.3306* (0.1894)	-0.0991 (0.1003)
$D_{500,750} \times Shock \times ProductSimilarity$	-0.7158*** (0.2538)	-0.3735* (0.2152)	-0.1811 (0.1276)
$D_{750,1000} \times Shock \times ProductSimilarity$	-0.6903*** (0.2646)	-0.2644 (0.2367)	-0.1497 (0.1232)

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Table A.1 – Continuation from previous page

	Attention Increase $t+0$	Attention Increase $t+1$	Attention Increase $t+2$
$D_{1000,1250} \times Shock \times ProductSimilarity$	-0.7302*** (0.2389)	-0.3012 (0.2071)	-0.2029 (0.1550)
$D_{1250,1500} \times Shock \times ProductSimilarity$	-0.6225*** (0.2373)	-0.4379** (0.1897)	-0.2617* (0.1367)
$D_{1500,1750} \times Shock \times ProductSimilarity$	-0.4696** (0.2000)	-0.3654** (0.1782)	-0.1667 (0.1057)
Controls			
$FilingReleased_{Peer}$	0.1491*** (0.0043)	0.1356*** (0.0044)	0.1192*** (0.0042)
$Size_{Peer}$	-0.1343*** (0.0509)	-0.0552* (0.0327)	-0.1542** (0.0651)
MBR_{Peer}	-0.0003* (0.0002)	-0.0001* (0.0001)	-0.0003* (0.0002)
$Loss_{Peer}$	-0.0025 (0.0122)	-0.0140 (0.0141)	-0.0461* (0.0240)
$Leverage_{Peer}$	0.0250 (0.0429)	0.0437 (0.0486)	0.0384 (0.0447)
$Cashflow_{Peer}$	0.0824** (0.0388)	0.0432 (0.0363)	0.0560 (0.0653)
$Size_{Shocked}$	0.0001* (0.0001)	-0.0001 (0.0000)	-0.0000 (0.0000)
$MBR_{Shocked}$	0.0001*** (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
$Loss_{Shocked}$	0.0001 (0.0001)	-0.0002** (0.0001)	-0.0001 (0.0001)
$Leverage_{Shocked}$	0.0011*** (0.0002)	0.0003* (0.0002)	0.0001 (0.0001)
$Cashflow_{Shocked}$	0.0029*** (0.0011)	0.0012 (0.0009)	-0.0001 (0.0005)
R^2	0.1654	0.1640	0.1692
S.E. type	Clustered by: Firm and Time		
Observations	101593632	101593632	101593632
Note: *p<0.1; **p<0.05; ***p<0.01 Format of coefficient cell: Coefficient (Std. Error) [p-value]			

Table A.1: This table shows OLS regression results that measure the relative increase in attention towards a peer firm i in comparison to its 90-day moving average under consideration of the distance and product similarity of peer and shocked firms. The results are estimated for $t + 0$, $t + 1$ and $t + 2$. Here, $t + 0$ denotes the time when a firm j received an abnormal attention increase following our shock definition as outlined in Section 2. Clustered standard errors are displayed in parentheses.

A.2 Pre-Shock Analysis

In the following, we present a pre-shock analysis accompanying the regression results in section 2.4. The objective is to verify that observed spillover effects are not already present before the occurrence of an abnormal attention increase at a firm j . Therefore, we conduct four regressions where the dependent variable measures the attention increase of a firm i compared to its 90-day moving average five days prior of an abnormal attention increase at a firm j occurs. All other regression specifications remain unchanged compared to our main analyses. If the parallel trend assumption holds, no remarkable

significant effects should be present between the treated and untreated firm pairs before the event date $t + 0$. Table A.2 shows the results of the corresponding OLS regressions.

The first column shows the pre-shock results for the corresponding results in table 2.3. Neither the coefficient estimates of the main variables nor the interaction term is significant at $t - 5$. The second and third columns show the results accompanying table 2.4. The third column refers to the near-distance analysis and shows no significant estimates at all. For our long-distance analysis in the fourth column, we observe minor significant effects for the shock dummy variable as well as a few two-way interactions. The total effects range between -0.0032 and -0.0007. Although we observe some effects here, the effects are close to zero and substantially lower than in our previous analysis. We therefore deem these results negligible. The last column shows the results of our analysis that incorporates product similarity as presented in table A.1. In our main analysis, we observe pronounced spillover effects. Our pre-shock analysis shows except for one estimate no significant effects. Only for the two-way interaction between our shock dummy and the 1250 to 1500 miles distance bin, we observe a significant coefficient estimate. This observation could be driven by random variation or local specific behavior in a subset of the data. In addition, the coefficient estimate is close to zero and just below the 10% significance level. However, given that this is just a single case, we deem this observation negligible and not sufficient to question the overall validity of the parallel trend assumption.

In summary, although the long-distance analysis shows minimal significant effects, all other pre-shock analyses consistently support the parallel trend assumption, reinforcing the robustness of our Difference-in-Difference approach.

	Table 2.3	Table 2.4	Table A.1
$\log(1 + Distance)$	0.0001 (0.0001)	0.0002 (0.0001)	0.0001 (0.0001)
Shock	-0.0008 (0.0023)	-0.0001 (0.0002)	0.0021*** (0.0008)
Product Similarity			0.0045 (0.0151)
$\log(1 + Distance) \times Shock$	0.0001 (0.0003)		
$Shock \times ProductSimilarity$			0.1511 (0.1698)
$D_{0,50} \times Shock$		0.0029 (0.0021)	
$D_{50,100} \times Shock$		-0.0023 (0.0027)	
$D_{100,150} \times Shock$		-0.0012 (0.0026)	
$D_{150,200} \times Shock$		-0.0015 (0.0022)	
$D_{200,250} \times Shock$		0.0030 (0.0022)	
$D_{0,250} \times Shock$		-0.0014 (0.0013)	-0.0021 (0.0016)
$D_{250,500} \times Shock$		-0.0023 (0.0014)	-0.0018 (0.0020)
$D_{500,750} \times Shock$		-0.0030** (0.0013)	-0.0026 (0.0021)

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Table A.2 – Continuation from previous page

	Table 2.3	Table 2.4	Table A.1
$D_{750,1000} \times Shock$		-0.0028* (0.0015)	-0.0010 (0.0020)
$D_{1000,1250} \times Shock$		-0.0034** (0.0016)	-0.0014 (0.0021)
$D_{1250,1500} \times Shock$		-0.0053*** (0.0020)	-0.0037* (0.0019)
$D_{1500,1750} \times Shock$		-0.0008 (0.0019)	0.0018 (0.0022)
$D_{0,250} \times ProductSimilarity$			-0.0205 (0.0167)
$D_{250,500} \times ProductSimilarity$			0.0114 (0.0194)
$D_{500,750} \times ProductSimilarity$			-0.0045 (0.0194)
$D_{750,1000} \times ProductSimilarity$			0.0022 (0.0213)
$D_{1000,1250} \times ProductSimilarity$			0.0007 (0.0231)
$D_{1250,1500} \times ProductSimilarity$			-0.0126 (0.0258)
$D_{1500,1750} \times ProductSimilarity$			0.0128 (0.0276)
$D_{0,250} \times Shock \times ProductSimilarity$			-0.0110 (0.0581)
$D_{250,500} \times Shock \times ProductSimilarity$			-0.0371 (0.1106)
$D_{500,750} \times Shock \times ProductSimilarity$			-0.0210 (0.1303)
$D_{750,1000} \times Shock \times ProductSimilarity$			-0.0951 (0.1099)
$D_{1000,1250} \times Shock \times ProductSimilarity$			-0.1341 (0.1276)
$D_{1250,1500} \times Shock \times ProductSimilarity$			-0.0526 (0.1200)
$D_{1500,1750} \times Shock \times ProductSimilarity$			-0.0854 (0.0734)
R^2	0.1661	0.1661	0.1661
Observations	123650183	123650183	123650183
Note: *p<0.1; **p<0.05; ***p<0.01 Format of coefficient cell: Coefficient (Std. Error)			

Table A.2: This table shows a pre-shock analysis accompanying our regression results as presented in section 2.4. The dependent variable is the relative attention increase towards a peer firm i in comparison to its 90-day moving average measured five days before the initial occurrence of an abnormal attention increase at firm j . Control variables are included, but have been omitted for the sake of readability. Fixed effects are included. Standard errors are clustered and presented in parentheses behind the coefficient estimates.

A.3 Robustness

To substantiate our findings, our robustness checks are twofold. First, we perform robustness checks in regard to our methodological assumptions by varying both the dependent variable and the types of regressions used. Second, we challenge the primary assumptions made in constructing the sample.

In our previous analysis we have used the relative attention increase of firm i compared to its 90-day moving average as dependent variable and performed an OLS regression.

To demonstrate the methodological robustness of our findings, we perform the following checks. Firstly, we run two additional regressions to verify our results that spillover effects in investor attention generally exist. Therefore, we are using the first regression specification using the continuous logarithmic distance measure and replacing our dependent variable with an attention increase dummy variable that is 1 if the attention of firm i is larger than its 90-day moving average attention. Since we now have a binary variable, we use a logistic instead of OLS regression. Furthermore, we run a second regression using the absolute attention of firm i using a Poisson regression. The first two columns of table A.3 show the results of our regressions.

For the Logit model, we observe a positive and significant coefficient of our shock dummy. The odds ratio shows that with a 92% probability the attention of firm i increases due to an abnormal increase in attention of firm j . The interaction shows a significant and negative coefficient of -0.014. This indicates that with increasing distance the likelihood of an spillover effect is reduced by 1.4%. Similar as in our initial analysis, the interaction term is moderating the effect of the shock dummy. This confirms our finding that spillover effects in attention are depending on proximity between firms. A comparable result is also shown by the Poisson estimations using the absolute attention of firm i as dependent variable. The coefficient estimation of the shock dummy is not significant, while the estimated effect of the interaction term is significant and comparable to the effect of OLS regression. Both logistic and Poisson regression strongly confirm the robustness of our analysis.

Secondly, we conduct Logit and Poisson regressions to further demonstrate the robustness of our results with regard to the importance of considering economic relationships when examining spatial spillover effects in investor attention. The corresponding results are shown in the last two columns of table A.3. For both models we observe the same characteristics as in our previous analysis. The effect of the interaction between shock and product similarity is dominating and moderated by the three-way interaction terms. Firms in direct proximity experience a higher spillover effect compared to firms with larger distances. The estimated effect of the two-way interaction between shock and distance is rising for firms within a range up to 1000 miles and decreasing afterwards. The Poisson regression reveals an even higher spillover effect on the attention of firm i . Examining the odds ratio of the interaction between shock and product similarity, we observe a 205% increase in attention compared to a 113% increase following the OLS regression. The same holds for the interpretation of the three-way interaction terms. For example, the odds ratio of the three-way interaction for ranges between 0 and 250 miles shows an effect of -132% compared to -28% in the OLS regression. Notably, while the OLS regression estimated the relative attention increase compared to the 90-day

moving average, the Poisson regression uses the absolute attention of firm i as the dependent variable. However, both logistic and Poisson regression confirm our previous findings.

In order to test our sample design, we run four regressions with adjusted samples. Table A.4 shows the regression results. The first two models assess the assumptions regarding the time window used to select control group data. In our previous analysis, we selected records that are a minimum of 20 and a maximum of 35 days before or after each shock. For the first model, we adjusted the range to a minimum of 10 and a maximum of 20 days. In the second model, we expanded the range to a minimum of 30 days and a maximum of 50 days.

The last two models are utilizing an alternative approach to identify abnormal attention increases at firm i . Instead of using the excess attention over the firm's 90-day moving average as criteria, we are using the z-score, which measures the deviation of the firms attention from their mean in relation to their standard deviation. Thus, for each firm i and time t , we calculate the z-score as follows:

$$Z_{i,t} = \frac{x_{i,t} - \mu_{i,t}}{\sigma_{i,t}}$$

Here, $x_{i,t}$ refers to the total daily attention of firm i at time t . $\mu_{i,t}$ is the 90-day moving average, and $\sigma_{i,t}$ is the 90-day rolling standard deviation for firm i at time t ¹. We consider a firm's attention as abnormal increased if its z-score is greater than 3, which is a common assumption in statistics (Shiffler, 1988). In addition, we maintain our second condition that the absolute attention must be of an appropriate level. Therefore, in our third model, we are building our sample based on the previously defined z-score and a minimum attention level of 500 end-user requests. To also challenge the second condition, we are relaxing this assumption in our fourth model, allowing z-scores greater than 3 and a minimum attention level of only 250 requests. Since over 30,000 records would fulfill this relaxed shock definition, which would consequently lead to a three times larger sample, we must limit the candidates selected in our sample to maintain a manageable size. Therefore, only in the fourth regression model, we randomly select 10,000 samples as shocked firms from all firms that fulfill our shock criteria. For all four models, all other specifications remain unchanged apart from the changes described above.

The results show first that varying the time window used to select the control group data does not have a notable effect compared to our initial results. We observe only minor shifts in the coefficients and p-values across the regressions. In particular, when

¹Each time a firm has not received any attention on a particular day, this day is considered as zero and is taken into account when calculating the mean and standard deviation for each rolling window.

comparing the results of our robustness checks in the first and second column with the results presented in table A.1, we observe the same magnitude of effects and draw the same conclusions. Second, the results also confirm the robustness of our shock definition. Utilizing z-scores as criteria to identify abnormal attention increases at firm i , leads to comparable results as in our initial analysis. The third column of table A.4 shows slightly increased coefficients, which nevertheless remain within the range of our initial analysis. However, these slightly higher coefficients balance each other out in the overall picture and leading to the same overall result². Analogously, the results of the fourth model, in which we relaxed the minimum attention level, are also consistent with our previous findings. The coefficients of the fourth model are mostly smaller in absolute terms when the minimum level of attention is reduced, suggesting that a certain level of attention is also required to generate spillover effects between firms.

In summary, we consistently demonstrate the reliability of our methodological framework under various testing conditions. We therefore conclude that our findings are robust and provide reliable evidence.

²For the near range between 0 and 250 miles, an effect of 91% was observed in table A.1, compared to 94% in column three of table A.4.

Dependent Variable	Increase Dummy _{t+0}	Absolute Attention _{t+0}	Increase Dummy _{t+0}	Absolute Attention _{t+0}
Model	Logit	Poisson	Logit	Poisson
Intercept	-1.123*** (0.002)		-1.153*** (0.002)	
$\log(1 + Distance)$	0.002*** (0.000)	0.0003 (0.0003)	0.000 (0.000)	0.0004* (0.0003)
Shock	0.653** (0.003)	0.0038 (0.0044)	0.564*** (0.001)	-0.0208*** (0.0041)
Product Similarity			-0.084*** (0.012)	-0.0220 (0.0176)
$\log(1 + Distance) \times Shock$	-0.014*** (0.000)	-0.0012* (0.0007)		
$Shock \times ProductSimilarity$			0.799*** (0.028)	0.7204*** (0.1987)
$D_{0,250} \times Shock$			0.043*** (0.002)	0.0082*** (0.0030)
$D_{250,500} \times Shock$			0.022*** (0.002)	0.0104*** (0.0038)
$D_{500,750} \times Shock$			0.022*** (0.002)	0.0124*** (0.0035)
$D_{750,1000} \times Shock$			-0.009*** (0.002)	0.0135*** (0.0040)
$D_{1000,1250} \times Shock$			0.004** (0.002)	0.0107*** (0.0038)
$D_{1250,1500} \times Shock$			0.019*** (0.002)	0.0100*** (0.0032)
$D_{1500,1750} \times Shock$			0.002 (0.002)	0.0092** (0.0036)
$D_{0,250} \times ProductSimilarity$			-0.026*** (0.020)	0.0154 (0.0262)
$D_{250,500} \times ProductSimilarity$			-0.268*** (0.020)	-0.0031 (0.0236)
$D_{500,750} \times ProductSimilarity$			-0.291*** (0.020)	0.0228 (0.0228)
$D_{750,1000} \times ProductSimilarity$			-0.140*** (0.022)	-0.0017 (0.0211)
$D_{1000,1250} \times ProductSimilarity$			-0.015 (0.025)	-0.0169 (0.0268)
$D_{1250,1500} \times ProductSimilarity$			0.031 (0.028)	0.0552* (0.0282)
$D_{1500,1750} \times ProductSimilarity$			0.200*** (0.029)	-0.0434 (0.0305)
$D_{0,250} \times Shock \times ProductSimilarity$			-0.208*** (0.043)	-0.2834*** (0.0901)
$D_{250,500} \times Shock \times ProductSimilarity$			-0.356*** (0.047)	-0.3452*** (0.1177)
$D_{500,750} \times Shock \times ProductSimilarity$			-0.406*** (0.048)	-0.4462*** (0.1393)
$D_{750,1000} \times Shock \times ProductSimilarity$			-0.584*** (0.053)	-0.3661** (0.1427)
$D_{1000,1250} \times Shock \times ProductSimilarity$			-0.360*** (0.060)	-0.4307*** (0.1288)

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Table A.3 – Continuation from previous page

Dependent Variable	Increase Dummy _{<i>t</i>+0}	Absolute Attention _{<i>t</i>+0}	Increase Dummy _{<i>t</i>+0}	Absolute Attention _{<i>t</i>+0}
Model	Logit	Poisson	Logit	Poisson
$D_{1250,1500} \times Shock \times ProductSimilarity$			-0.335*** (0.067)	-0.3980*** (0.1367)
$D_{1500,1750} \times Shock \times ProductSimilarity$			-0.561*** (0.071)	-0.2695** (0.1303)
Fixed Effects	No	Yes	No	Yes
Observations	123667577	123667577	101601783	101601763
<i>Note:</i>			*p<0.1; **p<0.05; ***p<0.01	

Table A.3: This table shows the regression results of our methodological robustness checks. The first two columns show the results using a continuous distance measure without considering economic relationships between the firms. The last two columns show the results using discrete distance bins and including the product similarity between firms. For each, we estimate two alternative models. The first model is a logistic regression using an increase dummy that is 1 if firm i 's attention is above its 90-day moving average, while the second is a Poisson regression using the absolute attention of firm i at date t . Control variables are included, but have been omitted for the sake of readability. Standard errors are presented in parentheses behind the coefficient estimates.

Dependent Variable	Absolute Attention _{t+0}	Absolute Attention _{t+0}	Absolute Attention _{t+0}	Absolute Attention _{t+0}
Model	1	2	3	4
$\log(1 + \text{Distance})$	0.0004** (0.0002)	0.0001 (0.0002)	0.0005*** (0.0002)	0.0002 (0.0002)
Shock	-0.0254*** (0.0060)	-0.0229*** (0.0053)	-0.0241*** (0.0055)	-0.0216*** (0.0058)
Product Similarity	-0.0983** (0.0451)	0.0083 (0.0143)	-0.0352* (0.0203)	-0.0241 (0.0206)
$\text{Shock} \times \text{ProductSimilarity}$	1.2580*** (0.4099)	1.1145*** (0.3524)	1.2083*** (0.3798)	1.1067*** (0.3867)
$D_{0,250} \times \text{Shock}$	0.0052** (0.0022)	0.0033 (0.0021)	0.0046** (0.0020)	0.0071*** (0.0020)
$D_{250,500} \times \text{Shock}$	0.0107*** (0.0031)	0.0098*** (0.0030)	0.0108*** (0.0030)	0.0097*** (0.0030)
$D_{500,750} \times \text{Shock}$	0.0137*** (0.0033)	0.0123*** (0.0031)	0.0144*** (0.0031)	0.0146*** (0.0042)
$D_{750,1000} \times \text{Shock}$	0.0139*** (0.0036)	0.0130*** (0.0034)	0.0131*** (0.0036)	0.0123*** (0.0038)
$D_{1000,1250} \times \text{Shock}$	0.0116*** (0.0035)	0.0104*** (0.0033)	0.0100*** (0.0033)	0.0095*** (0.0035)
$D_{1250,1500} \times \text{Shock}$	0.0098*** (0.0030)	0.0090*** (0.0028)	0.0089*** (0.0028)	0.0065** (0.0027)
$D_{1500,1750} \times \text{Shock}$	0.0083*** (0.0029)	0.0073*** (0.0028)	0.0063** (0.0031)	0.0059** (0.0024)
$D_{0,250} \times \text{ProductSimilarity}$	0.0129 (0.0221)	-0.0077 (0.0170)	0.0078 (0.0186)	0.0049 (0.0153)
$D_{250,500} \times \text{ProductSimilarity}$	0.0425 (0.0315)	-0.0068 (0.0176)	0.0354* (0.0202)	0.0224 (0.0206)
$D_{500,750} \times \text{ProductSimilarity}$	0.0884** (0.0348)	-0.0108 (0.0197)	0.0004 (0.0224)	0.0190 (0.0220)
$D_{750,1000} \times \text{ProductSimilarity}$	0.0230 (0.0401)	-0.0310 (0.0202)	0.0468* (0.0259)	0.0306 (0.0223)
$D_{1000,1250} \times \text{ProductSimilarity}$	0.0322 (0.0384)	-0.0152 (0.0192)	0.0601** (0.0269)	0.0276 (0.0224)
$D_{1250,1500} \times \text{ProductSimilarity}$	0.0494 (0.0383)	0.0382 (0.0269)	0.0118 (0.0273)	-0.0187 (0.0224)
$D_{1500,1750} \times \text{ProductSimilarity}$	0.0472 (0.0376)	0.0304 (0.0244)	0.0342 (0.0279)	0.0057 (0.0239)
$D_{0,250} \times \text{Shock} \times \text{ProductSimilarity}$	-0.2636** (0.1159)	-0.2344** (0.1034)	-0.2668*** (0.1012)	-0.2625** (0.1178)
$D_{250,500} \times \text{Shock} \times \text{ProductSimilarity}$	-0.6475*** (0.2272)	-0.5748*** (0.2013)	-0.6762*** (0.2154)	-0.5917*** (0.2209)
$D_{500,750} \times \text{Shock} \times \text{ProductSimilarity}$	-0.8201*** (0.2811)	-0.6947*** (0.2432)	-0.7518*** (0.2665)	-0.7939** (0.3124)
$D_{750,1000} \times \text{Shock} \times \text{ProductSimilarity}$	-0.7478** (0.2905)	-0.6659*** (0.2528)	-0.7681*** (0.2812)	-0.7883*** (0.2913)
$D_{1000,1250} \times \text{Shock} \times \text{ProductSimilarity}$	-0.7667*** (0.2662)	-0.6966*** (0.2290)	-0.8177*** (0.2466)	-0.6170** (0.2724)
$D_{1250,1500} \times \text{Shock} \times \text{ProductSimilarity}$	-0.6884*** (0.2630)	-0.6594*** (0.2326)	-0.6480*** (0.2426)	-0.5009** (0.2135)
$D_{1500,1750} \times \text{Shock} \times \text{ProductSimilarity}$	-0.5685** (0.2215)	-0.5321*** (0.1899)	-0.5636** (0.2243)	-0.4040** (0.1659)

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Table A.4 – Continuation from previous page

Dependent Variable	Absolute Attention _{t+0}	Absolute Attention _{t+0}	Absolute Attention _{t+0}	Absolute Attention _{t+0}
Model	1	2	3	4
Fixed Effects	Yes	Yes	Yes	Yes
Observations	101239391	101210638	98158256	113386948
<i>Note:</i>			*p<0.1; **p<0.05; ***p<0.01	

Table A.4: This table shows the regression results of our robustness checks concerning the sample construction. The first two columns show the results using alternative time windows to define pre and post periods around shocks. The last two columns show results from a sample derived using z-scores to identify abnormal increases in attention at firm i . All regressions are otherwise based on the specifications as described in chapters 2 and 3. Control variables are included, but have been omitted for the sake of readability. Standard errors are presented in parentheses behind the coefficient estimates.

B Appendix for Chapter 3

B.1 Robustness

In the following, we assess the robustness of our empirical framework by conducting a placebo pseudo-shock analysis to verify that the documented attention spillovers are not driven by pre-existing trends. As our empirical approach follows a difference-in-differences setting, it is essential that the parallel trends assumption holds. We therefore replace true attention shocks in our sample with placebo shock dates to examine whether, in the absence of abnormal attention increases, spurious correlations or pre-existing trends arise. For each shock in our sample, we randomly select a placebo date on which the same firm received attention between 35 and 60 days prior to the observed abnormal attention increase. We further ensure that no other attention shock occurs within this time window. Table B.1 presents the results for both main analyses.

	Regular Filings	Insider Filings	8-K Filings
Panel A: Pseudo-Shock Analysis - Product Similarity Specification (Table 3.3)			
$\log(1 + Distance)$	0.0002 (0.0001)	0.0011** (0.0005)	-0.0001 (0.0006)
$ProductSimilarity$	-0.0074 (0.0064)	-0.0140 (0.0253)	-0.0000 (0.0264)
$Shock$	-0.0005** (0.0003)	-0.0009 (0.0006)	0.0007 (0.0008)
$Shock \times ProductSimilarity$	0.0272* (0.0148)	0.0531 (0.0344)	-0.0388 (0.0411)
$Co-Shock_{Regular}$		0.0006 (0.0005)	0.0005 (0.0004)
$Co-Shock_{Insider}$	0.0005 (0.0004)		-0.0009 (0.0009)
$Co-Shock_{8K}$	0.0010* (0.0006)	-0.0012* (0.0007)	
$Filing_{ReleasedPeer}$	0.0941*** (0.0023)	0.0947*** (0.0030)	0.0944*** (0.0028)
$Size_{Peer}$	-0.0944*** (0.0311)	-0.0909*** (0.0245)	-0.1018*** (0.0363)
MBR_{Peer}	-0.0001 (0.0001)	-0.0004 (0.0005)	0.0000 (0.0001)
$Loss_{Peer}$	-0.0003 (0.0117)	0.0145 (0.0136)	0.0133 (0.0142)
$Leverage_{Peer}$	-0.0189 (0.0370)	0.0090 (0.0520)	-0.0452 (0.0422)
$Cashflow_{Peer}$	0.0312 (0.0330)	0.0277 (0.0388)	0.0406 (0.0360)
$Size_{Shocked}$	0.0000 (0.0000)	0.0000 (0.0001)	0.0000 (0.0000)
$MBR_{Shocked}$	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
$Loss_{Shocked}$	-0.0000 (0.0000)	-0.0001 (0.0002)	0.0000 (0.0001)
$Leverage_{Shocked}$	-0.0002 (0.0001)	-0.0002 (0.0002)	0.0004 (0.0003)
$Cashflow_{Shocked}$	-0.0002 (0.0001)	-0.0000 (0.0004)	0.0003 (0.0003)

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Table B.1 – Continuation from previous page

	Regular Filings	Insider Filings	8-K Filings
Panel B: Pseudo-Shock Analysis - Distance and Product Similarity Specification (Table 3.4)			
$\log(1 + \text{Distance})$	0.0002 (0.0002)	0.0012 (0.0007)	0.0002 (0.0006)
ProductSimilarity	-0.0078 (0.0177)	-0.0451 (0.0507)	0.0211 (0.0398)
Shock	0.0006 (0.0011)	0.0005 (0.0021)	-0.0030 (0.0033)
$\text{Shock} \times \text{ProductSimilarity}$	-0.0224 (0.0388)	0.0598 (0.0510)	-0.0392 (0.0782)
$D_{0,250} \times \text{Shock}$	-0.0015 (0.0019)	-0.0051 (0.0050)	0.0041 (0.0052)
$D_{250,500} \times \text{Shock}$	-0.0023 (0.0019)	-0.0021 (0.0033)	0.0051 (0.0043)
$D_{500,750} \times \text{Shock}$	-0.0028* (0.0014)	0.0041 (0.0070)	0.0089 (0.0058)
$D_{750,1000} \times \text{Shock}$	-0.0006 (0.0016)	-0.0050* (0.0027)	0.0083 (0.0055)
$D_{1000,1250} \times \text{Shock}$	-0.0009 (0.0019)	0.0021 (0.0036)	0.0062 (0.0040)
$D_{1250,1500} \times \text{Shock}$	-0.0008 (0.0012)	-0.0050 (0.0036)	0.0070 (0.0070)
$D_{1500,1750} \times \text{Shock}$	-0.0007 (0.0016)	-0.0027 (0.0038)	-0.0069 (0.0042)
$D_{0,250} \times \text{ProductSimilarity}$	0.0127 (0.0187)	0.0495 (0.0565)	-0.0026 (0.0359)
$D_{250,500} \times \text{ProductSimilarity}$	-0.0188 (0.0219)	0.0640 (0.0652)	-0.0189 (0.0352)
$D_{500,750} \times \text{ProductSimilarity}$	-0.0014 (0.0201)	-0.0059 (0.0525)	-0.0587 (0.0437)
$D_{750,1000} \times \text{ProductSimilarity}$	0.0054 (0.0187)	0.0291 (0.0496)	-0.0340 (0.0438)
$D_{1000,1250} \times \text{ProductSimilarity}$	-0.0054 (0.0271)	0.0460 (0.0566)	-0.0463 (0.0456)
$D_{1250,1500} \times \text{ProductSimilarity}$	0.0137 (0.0232)	0.0955 (0.0644)	-0.0594 (0.0451)
$D_{1500,1750} \times \text{ProductSimilarity}$	-0.0000 (0.0278)	0.0826* (0.0484)	-0.0078 (0.0542)
$D_{0,250} \times \text{Shock} \times \text{ProductSimilarity}$	0.0637 (0.0506)	0.0481 (0.0642)	-0.0275 (0.0827)
$D_{250,500} \times \text{Shock} \times \text{ProductSimilarity}$	0.1214 (0.0802)	-0.0375 (0.0822)	0.0378 (0.1056)
$D_{500,750} \times \text{Shock} \times \text{ProductSimilarity}$	0.0607 (0.0422)	0.0407 (0.0869)	-0.0246 (0.0907)
$D_{750,1000} \times \text{Shock} \times \text{ProductSimilarity}$	0.0345 (0.0465)	-0.0498 (0.0769)	-0.0861 (0.1108)
$D_{1000,1250} \times \text{Shock} \times \text{ProductSimilarity}$	0.0774 (0.0823)	-0.1176 (0.0872)	0.0139 (0.0969)
$D_{1250,1500} \times \text{Shock} \times \text{ProductSimilarity}$	0.0080 (0.0409)	-0.0845 (0.0995)	0.0028 (0.1188)
$D_{1500,1750} \times \text{Shock} \times \text{ProductSimilarity}$	0.0116 (0.0435)	-0.0106 (0.0918)	0.1781* (0.1042)
Co-Shock Regular		0.0009 (0.0007)	-0.0004 (0.0006)
Co-Shock Insider	0.0005 (0.0005)		0.0014 (0.0018)
Co-Shock 8K	0.0011** (0.0005)	-0.0009 (0.0010)	
$\text{Filing Released}_{\text{Peer}}$	0.0941*** (0.0023)	0.0947*** (0.0030)	0.0944*** (0.0028)
$\text{Size}_{\text{Peer}}$	-0.0944*** (0.0311)	-0.0909*** (0.0245)	-0.1018*** (0.0363)
MBR_{Peer}	-0.0001 (0.0001)	-0.0004 (0.0005)	0.0000 (0.0001)
$\text{Loss}_{\text{Peer}}$	-0.0003 (0.0117)	0.0145 (0.0136)	0.0132 (0.0142)
$\text{Leverage}_{\text{Peer}}$	-0.0189 (0.0370)	0.0091 (0.0520)	-0.0451 (0.0422)
$\text{Cashflow}_{\text{Peer}}$	0.0312 (0.0330)	0.0276 (0.0388)	0.0403 (0.0360)
$\text{Size}_{\text{Shocked}}$	0.0000 (0.0000)	0.0000 (0.0001)	0.0000 (0.0000)
$\text{MBR}_{\text{Shocked}}$	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
$\text{Loss}_{\text{Shocked}}$	-0.0001 (0.0000)	-0.0001 (0.0002)	0.0001 (0.0001)
$\text{Leverage}_{\text{Shocked}}$	-0.0002 (0.0001)	-0.0003 (0.0002)	0.0003 (0.0003)
$\text{Cashflow}_{\text{Shocked}}$	-0.0002 (0.0001)	0.0000 (0.0004)	0.0004 (0.0003)
Daily Fixed Effects	Yes	Yes	Yes

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Table B.1 – Continuation from previous page

	Regular Filings	Insider Filings	8-K Filings
Firm Fixed Effects	Yes	Yes	Yes
Firm $\times$ Month-Year Fixed Effects	Yes	Yes	Yes
S.E. type	Clustered by: Firm and Time		
Observations	113,278,471	13,314,728	17,236,096
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01		
	Format of coefficient cell: Coefficient (Std. Error)		

Table B.1: Placebo (pseudo-shock) tests for investor attention spillovers. This table reports results from re-estimating the main PPML regressions in Tables 3.3 and 3.4, replacing the true shock indicator with a pseudo shock that should not generate spillovers. Results are reported for regular, insider, and 8-K filings, with two-way clustered standard errors (firm and date) shown in parentheses.

Across both analyses, we find no economically meaningful or systematic spillover effects. The coefficients on the pseudo-shock indicator as well as the interaction terms capturing product-market similarity, geographic proximity, and their combination are uniformly small and largely statistically insignificant across all filing types. Importantly, the characteristic patterns observed in the main analysis are entirely absent in the placebo regressions.

While a small number of coefficients show statistical significance at conventional levels, these estimates are economically negligible and do not form coherent spillover patterns. Their magnitude is orders of magnitude smaller than the corresponding estimates in the main results and is consistent with statistical noise rather than systematic attention reallocation.

Overall, the absence of robust placebo effects provides strong support for the parallel trends assumption and indicates that the attention spillovers documented in the main analysis are not driven by pre-trends. Instead, the results reinforce the interpretation that observed spillovers are causally linked to genuine filing-induced attention shocks rather than to underlying firm similarity or geographic clustering alone.

B.2 Item-Level Analysis of 8-K Filings

Item	Category Description
Item 1	Registrant's Business and Operations
Item 2	Financial Information
Item 3	Securities and Trading Markets

Continued on next page

Table B.2 – Continuation from previous page		
Item	Category	Description
Item 4		Matters Related to Accountants and Financial Statements
Item 5		Corporate Governance and Management
Item 6		Asset-Backed Securities
Item 7		Regulation FD
Item 8		Other Events
Item 9		Financial Statements and Exhibits

Table B.2: This table lists the top-level item categories of Form 8-K filings.

	Item 1	Item 2	Item 3	Item 5	Item 7	Item 8
$\log(1 + \text{Distance})$	0.0007 (0.0011)	0.0018 (0.0012)	-0.0042 (0.0046)	0.0002 (0.0013)	0.0006 (0.0010)	-0.0000 (0.0011)
<i>ProductSimilarity</i>	-0.0323 (0.0567)	-0.0687 (0.0518)	0.1242 (0.1781)	0.0298 (0.0766)	0.2466** (0.0968)	0.0461 (0.0507)
<i>Shock</i>	-0.0005 (0.0044)	-0.0122 (0.0081)	0.0018 (0.0131)	-0.0177** (0.0087)	0.0000 (0.0042)	-0.0075** (0.0032)
<i>Shock</i> $\times$ <i>ProductSimilarity</i>	-0.0709 (0.1188)	0.0363 (0.1201)	-0.0373 (0.2775)	0.0811 (0.1213)	-0.3749*** (0.1436)	-0.1473 (0.1215)
$D_{0,250} \times \text{Shock}$	0.0064 (0.0090)	0.0106 (0.0095)	0.0383 (0.0276)	0.0078 (0.0119)	0.0016 (0.0071)	0.0022 (0.0058)
$D_{250,500} \times \text{Shock}$	-0.0053 (0.0063)	0.0055 (0.0082)	-0.0062 (0.0192)	0.0115 (0.0103)	-0.0084 (0.0086)	0.0028 (0.0049)
$D_{500,750} \times \text{Shock}$	-0.0029 (0.0073)	0.0063 (0.0088)	-0.0186 (0.0196)	0.0260 (0.0168)	-0.0037 (0.0057)	0.0053 (0.0053)
$D_{750,1000} \times \text{Shock}$	0.0002 (0.0069)	0.0184* (0.0098)	0.0077 (0.0166)	0.0199 (0.0125)	0.0021 (0.0073)	0.0084 (0.0055)
$D_{1000,1250} \times \text{Shock}$	-0.0042 (0.0076)	0.0078 (0.0091)	-0.0076 (0.0160)	0.0128 (0.0129)	0.0014 (0.0060)	0.0010 (0.0064)
$D_{1250,1500} \times \text{Shock}$	-0.0145 (0.0099)	-0.0014 (0.0078)	-0.0257 (0.0202)	0.0127 (0.0094)	-0.0118 (0.0075)	0.0072 (0.0058)
$D_{1500,1750} \times \text{Shock}$	-0.0013 (0.0066)	-0.0044 (0.0075)	0.0198 (0.0152)	0.0154* (0.0087)	-0.0098 (0.0076)	0.0077 (0.0058)
$D_{0,250} \times \text{ProductSimilarity}$	0.0648 (0.0648)	0.0374 (0.0961)	-0.1964 (0.2073)	-0.0154 (0.0945)	-0.0556 (0.0780)	-0.0610 (0.0491)
$D_{250,500} \times \text{ProductSimilarity}$	0.0648 (0.0648)	0.0374 (0.0961)	-0.1964 (0.2073)	-0.0154 (0.0945)	-0.0556 (0.0780)	-0.0610 (0.0491)
$D_{500,750} \times \text{ProductSimilarity}$	0.0403 (0.1058)	0.0907 (0.0688)	-0.1384 (0.1554)	-0.1710 (0.1398)	-0.1694** (0.0800)	-0.1021* (0.0578)
$D_{750,1000} \times \text{ProductSimilarity}$	-0.0087 (0.0747)	0.1255* (0.0749)	-0.2491 (0.1983)	-0.0844 (0.1048)	-0.2134** (0.0886)	-0.1838*** (0.0635)
$D_{1000,1250} \times \text{ProductSimilarity}$	-0.0467 (0.0665)	0.0658 (0.0752)	-0.0329 (0.1889)	-0.1761* (0.1054)	-0.2139*** (0.0827)	-0.1801** (0.0707)
$D_{1250,1500} \times \text{ProductSimilarity}$	-0.0087 (0.0685)	0.0458 (0.1026)	-0.0678 (0.1916)	0.0299 (0.1477)	-0.0523 (0.1009)	-0.1478** (0.0752)
$D_{1500,1750} \times \text{ProductSimilarity}$	0.1297 (0.1261)	0.0306 (0.0854)	-0.2061 (0.2047)	-0.1593 (0.1374)	-0.1703 (0.1206)	-0.1669* (0.0877)
$D_{0,250} \times \text{Shock} \times \text{ProductSimilarity}$	0.1565 (0.1607)	0.1930 (0.1769)	-0.1281 (0.3432)	-0.0700 (0.1684)	0.2083 (0.1292)	0.1026 (0.1084)
$D_{250,500} \times \text{Shock} \times \text{ProductSimilarity}$	0.1446 (0.1563)	0.1816 (0.1386)	0.2397 (0.3138)	0.1106 (0.1577)	0.3448** (0.1651)	0.1118 (0.1134)
$D_{500,750} \times \text{Shock} \times \text{ProductSimilarity}$	0.0942 (0.1796)	0.0865 (0.1713)	0.4594 (0.3022)	0.4133 (0.3812)	0.4833*** (0.1766)	0.2294* (0.1370)
$D_{750,1000} \times \text{Shock} \times \text{ProductSimilarity}$	0.1585 (0.1654)	-0.2070 (0.1582)	-0.0229 (0.3198)	-0.1469 (0.1874)	0.4215** (0.1672)	0.3140* (0.1816)
$D_{1000,1250} \times \text{Shock} \times \text{ProductSimilarity}$	0.3971** (0.1649)	0.2038 (0.1493)	0.1643 (0.3217)	0.3594* (0.2085)	0.3727** (0.1601)	0.4083** (0.1724)
$D_{1250,1500} \times \text{Shock} \times \text{ProductSimilarity}$	0.3043 (0.2290)	0.1683 (0.1753)	0.2977 (0.4059)	0.1565 (0.2733)	0.5398*** (0.1949)	0.2659 (0.1778)
$D_{1500,1750} \times \text{Shock} \times \text{ProductSimilarity}$	0.0976 (0.2491)	0.2306 (0.2098)	0.0876 (0.3746)	0.4002 (0.3150)	0.4651** (0.2363)	0.2262 (0.1882)
<i>Co-Shock</i>	-0.0128** (0.0052)	-0.0008 (0.0017)	-38233.4173 (6981743713.5880)	-47.0131 (49892593.9158)	-0.0036* (0.0021)	-0.0047 (0.0045)

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Table B.3 – Continuation from previous page

	Item 1	Item 2	Item 3	Item 5	Item 7	Item 8
<i>Filing Released_{Peer}</i>	0.0965*** (0.0033)	0.0982*** (0.0034)	0.0993*** (0.0064)	0.0969*** (0.0044)	0.0980*** (0.0039)	0.1006*** (0.0038)
<i>Size_{Peer}</i>	-0.0426 (0.0484)	-0.1301*** (0.0487)	0.0185 (0.0889)	-0.1496*** (0.0402)	-0.0967*** (0.0361)	-0.0907** (0.0399)
<i>MBR_{Peer}</i>	0.0003 (0.0002)	-0.0001 (0.0002)	0.0002 (0.0002)	0.0012 (0.0019)	0.0001 (0.0003)	-0.0001 (0.0003)
<i>Loss_{Peer}</i>	-0.0174 (0.0141)	-0.0191 (0.0142)	-0.0074 (0.0299)	-0.0075 (0.0259)	-0.0082 (0.0135)	0.0071 (0.0142)
<i>Leverage_{Peer}</i>	-0.0713 (0.0582)	-0.0143 (0.0385)	-0.1065 (0.1004)	-0.0553 (0.1004)	-0.0641 (0.0712)	0.0115 (0.0497)
<i>Cashflow_{Peer}</i>	0.0042 (0.0495)	0.0259 (0.0485)	-0.0529 (0.0786)	-0.0423 (0.0698)	0.0486 (0.0423)	0.0196 (0.0479)
<i>Size_{Shocked}</i>	-0.0002 (0.0002)	-0.0007 (0.0006)	-0.0000 (0.0005)	-0.0004 (0.0003)	-0.0007 (0.0004)	-0.0000 (0.0001)
<i>MBR_{Shocked}</i>	-0.0006 (0.0004)	-0.0002 (0.0003)	-0.0005 (0.0009)	0.0002 (0.0003)	-0.0002 (0.0002)	-0.0001 (0.0001)
<i>Loss_{Shocked}</i>	-0.0004 (0.0004)	-0.0036 (0.0035)	-0.0002 (0.0010)	0.0003 (0.0008)	-0.0028 (0.0018)	0.0003 (0.0004)
<i>Leverage_{Shocked}</i>	0.0016** (0.0008)	0.0007 (0.0014)	0.0015 (0.0025)	0.0070 (0.0054)	0.0017* (0.0010)	0.0007 (0.0006)
<i>Cashflow_{Shocked}</i>	-0.0015 (0.0012)	-0.0005 (0.0010)	-0.0016 (0.0048)	0.0070 (0.0054)	-0.0008 (0.0019)	-0.0012 (0.0011)
Daily Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Month-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
S.E. type	Clustered by: Firm and Time					
Observations	4,821,324	5,471,842	1,245,073	3,075,564	6,909,711	7,110,940
<i>Note:</i> *p<0.1; **p<0.05; ***p<0.01						
Format of coefficient cell: Coefficient (Std. Error)						

Table B.3: This table shows PPML regression results for attention spillovers following 8-K filings, separately by top-level item category. For each shock date, the focal firm experiences an abnormal attention increase to its 8-K filings. Shock dates are assigned to a specific item type when more than 50% of all accessed 8-K filings on that day include that item. Results are reported with two-way clustered standard errors (firm and date) shown in parentheses.

C Appendix for Chapter 4

	Attention	Attention	Attention	Attention	Attention
Female	-0.0348 (0.2036)	-0.1890 (0.1959)	1.0646*** (0.4032)	-0.0279 (0.2017)	-0.0415 (0.2042)
Announcement	1.3756*** (0.1095)	1.3408*** (0.1270)	1.4150*** (0.2169)	1.2966*** (0.1178)	1.3103*** (0.1198)
Announcement $\times$ Female	-0.1409 (0.2896)	0.0904 (0.2586)	-2.2152*** (0.5516)	0.0020 (0.2700)	0.0088 (0.2706)
Log(MarketCap)	-0.0588*** (0.0222)	-0.0640*** (0.0228)	-0.0430** (0.0204)	-0.0584*** (0.0222)	-0.0599*** (0.0223)
Female $\times$ Log(MarketCap)	0.0002 (0.0093)	0.0074 (0.0090)	-0.0505*** (0.0179)	0.0001 (0.0093)	0.0007 (0.0094)
Announcement $\times$ Log(MarketCap)	-0.0489*** (0.0054)	-0.0487*** (0.0064)	-0.0473*** (0.0102)	-0.0460*** (0.0059)	-0.0466*** (0.0060)
Announcement $\times$ Female $\times$ Log(MarketCap)	0.0050 (0.0134)	-0.0050 (0.0119)	0.1037*** (0.0248)	-0.0016 (0.0124)	-0.0020 (0.0125)
TimeInCo	0.0022** (0.0011)	-0.0024 (0.0016)	-0.0024 (0.0016)	-0.0004 (0.0012)	-0.0007 (0.0013)
TimeRetirement	0.0023 (0.0033)	0.0021 (0.0033)	0.0021 (0.0033)	0.0011 (0.0034)	0.0014 (0.0034)
Age	0.0004 (0.0032)	0.0010 (0.0033)	0.0010 (0.0033)	-0.0001 (0.0033)	0.0002 (0.0033)
BoardExpTot	0.0031*** (0.0007)	0.0035*** (0.0007)	0.0035*** (0.0007)	0.0035*** (0.0007)	0.0035*** (0.0007)
BoardExpCo	0.0003 (0.0016)	0.0044** (0.0020)	0.0044** (0.0020)	0.0032* (0.0018)	0.0035* (0.0018)
Filing Released	0.1678*** (0.0158)	0.1706*** (0.0156)	0.1706*** (0.0156)	0.1715*** (0.0156)	0.1712*** (0.0157)
8K-attention	0.0008*** (0.0002)	0.0008*** (0.0002)	0.0008*** (0.0002)	0.0008*** (0.0002)	0.0008*** (0.0002)
Executive		-0.4091** (0.1827)			
Announcement $\times$ Executive		0.0742 (0.2565)			
Female $\times$ Executive		1.2537*** (0.4207)			
Executive $\times$ Log(MarketCap)		0.0209** (0.0088)			
Announcement $\times$ Female $\times$ Executive		-2.3055*** (0.6191)			
Announcement $\times$ Executive $\times$ Log(MarketCap)		0.0014 (0.0124)			
Female $\times$ Executive $\times$ Log(MarketCap)		-0.0578*** (0.0187)			
Announcement $\times$ Female $\times$ Executive $\times$ Log(MarketCap)		0.1086*** (0.0280)			
Supervisor			0.4091** (0.1827)		
Announcement $\times$ Supervisor			-0.0742 (0.2565)		
Female $\times$ Supervisor			-1.2537*** (0.4207)		
Supervisor $\times$ Log(MarketCap)			-0.0209** (0.0088)		
Announcement $\times$ Female $\times$ Supervisor			2.3055*** (0.6191)		
Announcement $\times$ Supervisor $\times$ Log(MarketCap)			-0.0014 (0.0124)		
Female $\times$ Supervisor $\times$ Log(MarketCap)			0.0578*** (0.0187)		
Announcement $\times$ Female $\times$ Supervisor $\times$ Log(MarketCap)			-0.1086*** (0.0280)		

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Table C.1 – Continuation from previous page

	Attention	Attention	Attention	Attention	Attention
CEO				-0.2523 (0.2036)	
Announcement $\times$ CEO				0.3381 (0.3126)	
Female $\times$ CEO				0.2839 (0.4762)	
CEO $\times$ Log(MarketCap)				0.0142 (0.0097)	
Announcement $\times$ Female $\times$ CEO				-1.9680*** (0.6738)	
Announcement $\times$ CEO $\times$ Log(MarketCap)				-0.0113 (0.0151)	
Female $\times$ CEO $\times$ Log(MarketCap)				-0.0147 (0.0209)	
Announcement $\times$ Female $\times$ CEO $\times$ Log(MarketCap)				0.0976*** (0.0309)	
Chief					-0.2838 (0.1873)
Announcement $\times$ Chief					0.2370 (0.2784)
Female $\times$ Chief					0.4278 (0.4267)
Chief $\times$ Log(MarketCap)					0.0154* (0.0090)
Announcement $\times$ Female $\times$ Chief					-1.9732*** (0.5872)
Announcement $\times$ Chief $\times$ Log(MarketCap)					-0.0073 (0.0134)
Female $\times$ Chief $\times$ Log(MarketCap)					-0.0207 (0.0189)
Announcement $\times$ Female $\times$ Chief $\times$ Log(MarketCap)					0.0973*** (0.0269)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	192984	192984	192984	192984	192984
S.E. type	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm
<i>Note:</i>				*p<0.1; **p<0.05; ***p<0.01	
				Format of coefficient cell: Coefficient (Std. Error)	

Table C.1: This table shows PPML regression results analyzing the impact of board appointment announcements on investor attention, allowing for heterogeneity by market capitalization and role. Coefficients are shown with clustered standard errors in parentheses.

	CAR(1,5) Super- visor	CAV(1,5) Supervi- sor	CAR(1,5) Exec	CAV(1,5) Exec	CAR(1,5) CEO	CAV(1,5) CEO
Female	156.68 (141.33)	-52.63 (66.50)	-130.44*** (47.00)	19.98 (24.51)	-125.59*** (45.04)	15.22 (23.46)
Attention Var.	7.74 (11.99)	59.37*** (7.28)	0.90 (9.21)	50.83*** (6.14)	-0.37 (8.68)	51.68*** (5.68)
<i>Female</i> $\times$ <i>AttentionVar</i>	-30.21 (40.53)	15.71 (20.37)	26.55** (12.26)	-13.55* (7.34)	27.43** (11.86)	-11.70* (7.01)
<i>Supervisor</i>	52.34 (46.23)	-5.10 (24.47)				
<i>Supervisor</i> $\times$ <i>AttentionVar</i>	-6.84 (12.37)	-8.55 (7.62)				
<i>Female</i> $\times$ <i>Supervisor</i>	-287.12* (149.06)	72.60 (69.66)				
<i>Female</i> $\times$ <i>Supervisor</i> $\times$ <i>AttentionVar</i>	56.77 (42.32)	-29.26 (21.46)				
<i>Executive</i>			-52.34 (46.23)	5.10 (24.47)		
<i>Executive</i> $\times$ <i>attention_{var}</i>			6.84 (12.37)	8.55 (7.62)		
<i>Female</i> $\times$ <i>Executive</i>			287.12* (149.06)	-72.60 (69.66)		
<i>Female</i> $\times$ <i>Executive</i> $\times$ <i>AttentionVar</i>			-56.77 (42.32)	29.26 (21.46)		
<i>CEO</i>					-71.80 (56.33)	9.89 (27.94)
<i>CEO</i> $\times$ <i>AttentionVar</i>					17.99 (15.00)	8.75 (8.45)
<i>Female</i> $\times$ <i>CEO</i>					450.41*** (166.45)	-51.33 (83.46)
<i>Female</i> $\times$ <i>CEO</i> $\times$ <i>AttentionVar</i>					-99.64** (42.55)	16.86 (23.71)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16850	16850	16850	16850	16850	16850
S.E. type	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm	by: Firm
<i>Note:</i>				*p<0.1; **p<0.05; ***p<0.01		
				Format of coefficient cell: Coefficient (Std. Error)		

Table C.2: This table reports OLS regression estimates of board appointment announcements on cumulative abnormal returns $CAR(1,5)$ and cumulative abnormal trading volume $CAV(1,5)$, allowing the attention-moderation effect to vary by role (supervisory, executive, and CEO). As the attention measure, we use announcement-day attention at $t = 0$. Coefficients are presented with clustered standard errors in parentheses.

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