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Distracted Institutional Investors

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Abstract

I investigate how distraction affects the trading behavior of professional asset managers. Exploring detailed transaction-level data, I show that managers with a large fraction of portfolio stocks that have an earnings announcement are significantly less likely to trade in other stocks, suggesting that these announcements divert attention from trading decisions for other stocks. This distraction effect is more pronounced for nonpassive managers who engage in active stock selection choices. Finally, I identify three channels through which distraction hurts managers' performance: Distracted managers trade less profitably, incur slightly higher transaction costs, and are less likely to close losing positions.

I. Introduction

Attention is scarce. Yet, little is known about how limited attention affects the trading behavior of institutional asset managers, arguably the most important class of investors in financial markets today.[1] This lack of knowledge arises for two reasons. First, professional investors employ significant resources to overcome attention constraints: They hire additional research staff, acquire access to real-time news feeds, and invest in computer capacities for algorithmic trading or smart order routing. Hence, institutional asset managers are assumed to be less attention constrained to begin with. Second, any empirical investigation in this domain faces the problem that attention is unobserved and plagued by endogeneity.

In this article, I propose a way to address this empirical challenge and, in doing so, uncover evidence suggesting that attention constraints can be binding even for professional asset managers. Specifically, exploiting detailed transaction-level

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[1] Stambaugh (2014) reports that at the end of 2012, roughly 22% of U.S. equity was directly owned by individuals. The flip side of this is that more than 75% of equity ownership is delegated in one way or another.

data for a large sample of U.S. institutional investors, I identify attention shifts between different stocks that are on the "radar screen" of a particular investor. Exploring the ramifications of such attention shifts, I shed light on several important questions: How severe are attention constraints among professional investors? Do investors cope with them in a rational manner? And, finally, what are the channels through which attention constraints manifest themselves in investors' trading activity and performance?

My identification builds on the premise that an investor cannot pay equal attention to all stocks. Thus, he focuses on a subset, or "watchlist," of stocks. To see this, consider the following example: There are two investors: 1 and 2. Investor 1 watches stocks A and B. Investor 2 watches stocks A and C. Suppose there is important news about stock B, but not about stock C. Under limited attention, investor 1 is expected to pay less attention to stock A compared to investor 2. The reason is that, unlike investor 2, investor 1 needs to digest and respond to news about stock B, which distracts him from trading in stock A. In another period, stock C may have important news. Then, investor 2 is expected to be distracted compared to investor 1. By comparing the trading of investors 1 and 2 in the same stock, my identification exploits such attention redirections at the investor–stock–time level.

An appealing feature of the 3-dimensional data structure (investor $\times$ stock $\times$ time) is the rich cross-sectional and time-series variation in investor distraction, which I exploit in my regression approach. In particular, through high-dimensional fixed effects, I absorb a large fraction of the variation in trading activity that could be a source of endogeneity. For example, whether a stock has important news in a given week is itself an important determinant of trading activity. The inclusion of stock $\times$ date fixed effects ensures that my results are not driven by stock-level effects. Similarly, institutions may have different preferences for certain stocks, and these preferences could be correlated with their trading response. Stock $\times$ manager fixed effects control for such time-invariant preferences. In effect, my results stem from comparing the trading activity of different investors in the same stock at the same time. I view my identification strategy as a significant improvement over prior studies in this field.

The institutional transaction data used in this article come from Ancerno Ltd., a consulting firm that helps institutional investors monitor their trading costs. Prior research finds that Ancerno trades represent approximately 10% of all institutional trading volume in the United States and that they are not significantly different from trades made by average U.S. institutional investors (Puckett and Yan (2011), Anand, Irvine, Puckett, and Venkataraman (2012)). The key feature of these data is that, in addition to detailed trading records, they provide a unique identifier for the trading institutions. This enables me to implement my identification strategy at the institutional investor level.[2]

[2]Ideally, I would prefer to conduct my analysis at the fund level. Unfortunately, the Ancerno data do not provide a unique fund identifier, and thus I must work at the institution level. To the degree that attention constraints operate at the fund level, my distraction measures contain measurement error, which could lead to attenuation bias. Hence, the distraction effects documented in this article can be understood as a lower-bound estimate of the real attention constraints faced by institutional investors.

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In my baseline approach, I use investors' portfolio holdings reported on Form 13F to define what I call the "portfolio watchlist": the set of stocks that investors hold at the end of the previous quarter. I verify that portfolio watchlists predict future trading, much better than do randomly assigned placebo watchlists, which confirms that investors pay more attention to their watchlist stocks. As a robustness check, I consider a trade-based watchlist, which is defined as the set of stocks an investor traded in the previous 12 weeks. The results for this alternative watchlist definition, reported in Appendix B in the Supplementary Material, offer an important consistency check for my approach.

I use quarterly earnings announcement dates to proxy for important stock news. Indeed, earnings announcements are arguably the most important recurring news events for individual stocks, justifying their preeminent role in the literature on public information disclosures (see, e.g., Beaver (1968), Aharony and Swary (1980), Bernard and Thomas (1989), and Kim and Verrecchia (1994)). Institutional investors have a professional mandate to keep their fingers on the pulse of stock market developments. As such, they routinely attend earnings conference calls, and when the news is substantial, they may swiftly rescale their positions (e.g., Bushee, Jung, and Miller (2011)). All this requires attention,[3] which I argue is missing for trading in other stocks. My primary distraction proxy is thus the (portfolio-weighted) fraction of stocks on the investor's watchlist that have an earnings announcement in a given period.[4] When I construct the distraction measure for a given stock and investor, I calculate this fraction by summing over all other stocks on the investor's watchlist. Thus, my measure captures distraction coming from other stocks on the watchlist.

My first finding, summarized in Figure 1, is that institutional investors are significantly less likely to trade in a given stock when there are many earnings announcements for other stocks on their watchlist. An increase from the bottom to the top quartile of distraction reduces the propensity to trade in a given stock by approximately 3.5%. For the subset of managers who follow active investment strategies, that is, those who are not identified as quasi-indexers according to the investor classification by Bushee and Noe (2000) and Bushee (2001), the effect doubles to more than 7%. As explained earlier, these results obtain in panel regressions that control for both stock $\times$ time and stock $\times$ manager fixed effects, thereby removing endogeneity concerns arising from unobserved stock-level shocks or fixed investor preferences.

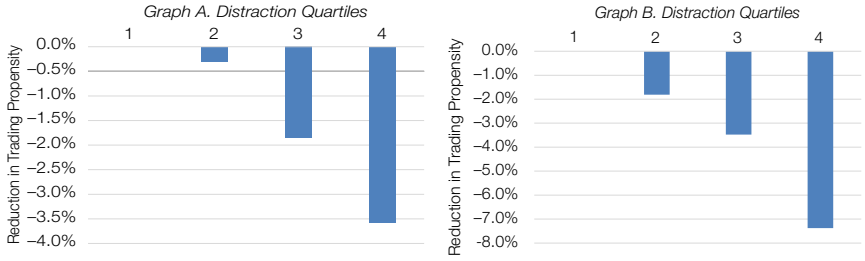
In contrast to the strong effect at the extensive margin, I find no distraction effect at the intensive margin. That is, conditional on trading in a given stock, institutional investors do not trade less when watchlist stocks have many earnings announcements. This no-result is difficult to reconcile with standard models of information acquisition in which inattentive investors adjust at the intensive margin how much information to gather (e.g., Verrecchia (1982), Van Nieuwerburgh and Veldkamp (2010), and Kacperczyk, Van Nieuwerburgh, and Veldkamp (2016)).

[3]This is confirmed by Hirshleifer, Lim, and Teoh (2009), who show that the incorporation of earnings news is delayed on days with a large number of earnings announcements.

[4]I obtain consistent results when distraction is defined as the equal-weighted fraction of portfolio stocks with an earnings announcement, or when the earnings announcement dummy is replaced by earnings surprise quintiles; see the robustness results in Section VIII.

FIGURE 1
Distraction and Trading Propensity

Figure 1 shows the economic magnitude of the distraction effect for 4 distraction quartiles. Economic magnitude is measured as the reduction in the propensity to trade relative to its unconditional mean. The numbers come from regressions similar to those in Panel A of Table 2, except that the continuous distraction measure is replaced by quartile dummies. The numbers for quartiles 2 to 4 show the additional distraction relative to quartile 1 (least distraction). Graph A shows the quartile results for the overall sample that includes all managers. Graph B shows the quartile results for the subset of managers who are classified as non-indexers according to Bushee and Noe (2000) and Bushee (2001).



Instead, my results suggest that, even among professional traders, attention is better modeled in terms of a fixed cost to searching and trading in a particular stock (akin to the recognition cost in Merton (1987); see also Reis (2006), Abel, Eberly, and Panageas (2007), and Chien, Cole, and Lustig (2012)).

I then conduct several sample splits to shed light on which type of managers are more distracted. I find that the distraction effect is stronger for managers who trade actively, where activeness is proxied by the intensity of rebalancing trades as opposed to flow-induced trades. Because the former involves a stock-selection choice, whereas the latter amounts to a mechanical rescaling of existing positions, I expect rebalancing trades to be more susceptible to distraction, and this is what I find. I further show that my results are concentrated for institutions with a diverse watchlist across industries. This is intuitive, as a stock's earnings announcement is also news to other stocks in the same industry. Hence, institutions with high industry concentration may be attracted to rather than distracted from trading stocks when there are earnings announcements for other watchlist stocks in the same industry.

Next, I identify two channels through which distraction affects managers' performance. First, distraction leads managers to make poorer trading decisions. Compared to nondistracted managers trading in the same stock, distracted managers have past-trade returns over the next 4 weeks that are 20 basis points lower (40 basis points in the subsample of quasi-indexers). Second, distracted managers incur slightly higher transaction costs on their trades.[5] I acknowledge, however, that the magnitude of these effects is relatively modest.

[5]For trade profitability, results are consistent with rational attention models. For transaction costs, results can be rationalized by microstructure models suggesting that limited attention exposes limit order users to the risk of being "picked off" or not executed. These effects could be at work even when order execution is outsourced to brokers, as distracted institutions may send their orders with delay and thus higher urgency.

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Finally, I conduct an in-depth analysis of the channels that appear to explain observed attention choices. I start by testing an important corollary of the rational attention paradigm. Specifically, I test whether the distraction effect is reduced for stocks that matter more to an investor. Consistent with this intuition, I find that investors remain relatively more attentive to stocks in which they have a large portfolio stake or stocks with an imminent earnings announcement. Probing for other (i.e., behavioral) factors that mediate the distraction effect, I uncover two additional findings. First, the distraction effect on trade, and in particular sell decisions, is more pronounced for stocks that trade at a loss. I argue that this finding is consistent with the recent literature suggesting that information can have a direct effect on investors' utility (Karlsson, Loewenstein, and Seppi (2009), Sicherman, Loewenstein, and Seppi (2015)), prompting them to avoid unpleasant information when they have the excuse of a distracting event. Second, I document that salience, which is found to strongly attract attention (Barber and Odean (2008), Bordalo, Gennaioli, and Shleifer (2013)), mitigates the distraction effect. Overall, these results suggest that in addition to rational considerations, attention-allocation decisions are influenced by subconscious and/or psychological factors such as emotions toward gains and losses and a stock's salience.

My article contributes to the literature on inattention in financial markets (see, e.g., Cohen and Frazzini (2008), DellaVigna and Pollet (2009), and Hirshleifer et al. (2009)). Although this literature has been burgeoning, few papers focus on professional investors, presumably because these investors are assumed to be less attention constrained to begin with. Fang, Peress, and Zheng (2014) show that certain mutual funds persistently buy into stocks covered in the media and that these funds underperform relative to other funds. They interpret their findings as indirect evidence for the presence of attention constraints among this subset of mutual funds. Lu, Ray, and Teo (2016) collect a sample of marriage and divorce events for hedge fund managers and find that their performance suffers during those events. Ben-Rephael, Da, and Israelsen (2017) propose search volume on Bloomberg terminals as a proxy for institutional attention and show that it correlates with the timely incorporation of earnings news. Kempf, Manconi, and Spalt (2017) explore a similar identification approach to mine, but aggregated and at lower frequency, to study how shareholder distraction affects corporate actions. They find that firms with distracted shareholders engage more in value-destroying acquisitions, presumably because of less intense monitoring. By looking at individual trades of institutional investors, my article precisely identifies the channels determining how inattention affects managers' trading behavior and performance.

This article proceeds as follows: Section II presents the empirical hypotheses. Section III describes the data. Section IV introduces the identification approach. Section V considers the effect of institutional distraction on trading activity. Section VI studies how distraction affects performance. Section VII investigates whether observed attention choices are more in line with rational or irrational attention models. Section VIII presents robustness checks. Section IX concludes.

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II. Hypotheses

A. Distraction, Trading Activity, and Performance

In the empirical analysis, I focus on 4 outcome variables that are well suited for my identification strategy:[6] i) investors' trading propensity (i.e., their decision whether to trade), ii) trading volume (i.e., how much they trade conditional on trading), iii) trade profitability, and iv) incurred transaction costs. In this section, I draw on the literature to lay out the empirical predictions pertaining to these outcome variables.

Trading propensity is measured by a trade dummy that equals 1 if the stock is traded by a given manager. Models featuring a fixed attention cost for trading or searching for investment opportunities predict a negative relation between managers' distraction and their propensity to trade (see Merton (1987), Reis (2006), Abel et al. (2007), (2013), and Chien et al. (2012)). If distracted investors are less likely to incur this cost, they will be less likely to trade a given stock. When investors are short-sale constrained, the search costs for buy decisions, and thus any potential distraction effects, exceed the costs for sell decisions (Barber and Odean (2008)). It is not clear, however, how short-sale constrained the institutional investors in my sample are.[7] Hence, I define dummy variables that separately flag buy and sell decisions.

Trading volume is defined as the natural logarithm of dollar trading volume conditional on trading. A large literature models attention as choosing the precision of a trading signal (e.g., Verrecchia (1982), He and Wang (1995), Vives (1995), Peng and Xiong (2006), Van Nieuwerburgh and Veldkamp (2010), and Kacperczyk et al. (2016)). In these models, a distracted manager receives a less precise signal and thus faces higher uncertainty, causing him to trade less aggressively. It is not clear, however, whether the link from signal precision to trade size is relevant in practice. Indeed, position sizes are often determined by an asset's historical volatility (e.g., through a position limit imposed by a risk constraint; see Pedersen (2015)) rather than by the perceived precision of a recently obtained trading signal.

Trade profitability is defined as the post-trade stock return, multiplied by -1 for sells. Rational attention models typically predict that distracted managers trade less profitably. This prediction follows immediately in models that link attention with signal precisions, but it can also arise in models featuring fixed search costs. For instance, consider a model in which an investor needs to incur a fixed search cost (e.g., the cost associated with conducting a fundamental analysis) for uncovering a profitable trading opportunity (e.g., finding a stock that is sufficiently under- or overvalued given the estimated fundamental value). If a distracted investor does not search, he will not find and thus cannot act on this profitable trade. If one further assumes that the investor has to make some less profitable trades in any case (which are not, or are at least less, subject to distraction, perhaps

[6] As explained earlier, my identification strategy relies on exploiting the variation in trading activity of different investors in the same stock at the same time. As such, candidate outcome variables need to be at the individual trade level.

[7] According to Jame (2018), the Ancerno data contain short sales, but it is not possible to distinguish them from other sales.

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because these trades are required to accommodate in- or outflows to the fund), average trade profitability goes down when a distracted investor stops searching for profitable trading opportunities.[8]

Transaction costs are measured by the incurred relative transaction spread. Although rarely modeled explicitly, it is intuitive to think that attention can matter for transaction costs, which is itself a key driver of performance for large institutional investors (Anand et al. (2012)). For instance, one may hypothesize that limit orders yield better prices but require more attention (because limit orders give rise to the risk of being picked off and/or not being executed).[9] Attention-constrained investors may hence decide to use market orders instead of limit orders. Alternatively, they may spend less time looking for the best quotes and/or bargaining with brokers, or they may decide to fragment their trades less. These effects could be at work even when investors outsource trade execution to their brokers because distracted investors may send their orders with delay and thus higher urgency.

B. Rational or Irrational Attention Allocation

I now discuss additional cross-sectional predictions about when and where distraction effects are expected to be more or less pronounced.

Rational attention models presume that investors are fully cognizant of their attention constraints and cope with them in a rational manner. As such, these models predict that investors reallocate their attention on the basis of a cost–benefit analysis. Specifically, investors should be less likely to divert attention from stocks that matter more to their utility (Corwin and Coughenour (2008)), for example, stocks in which they have a large stake or for which they expect the release of important value-relevant information. In the empirical analysis, I study how distraction effects interact with a stock's watchlist weight and a dummy indicating whether there is an imminent earnings announcement.

Although there is no widely accepted alternative to the rational attention paradigm, the literature has documented other factors that help explain observed attention allocation behavior. Here, I discuss 2 such factors that yield empirical predictions that contrast with those from rational attention models.

The first factor concerns investors' emotions felt toward paper gains and losses. A growing literature in behavioral economics suggests that information about such gains and losses may directly affect the utility of economic agents, above its indirect effect through their choices (Caplin and Leahy (2001), Brunnermeier and Parker (2005); for a survey, see Golman, Hagmann, and Loewenstein (2017)). A natural implication of such models is that agents may choose to selectively avoid information they expect to be bad.[10] I posit that attention-constrained

[8]Put differently, models with fixed attention costs require some heterogeneity in fixed costs (in the sense that more profitable trades are more costly) to predict that lower attention results in lower average profits.

[9]Dugast (2018) presents a model of limit order trading with infrequent monitoring due to limited attention. Moreover, I believe that such an intuition can arise naturally in models of endogenous limit order trading as in Handa and Schwartz (1996) and Goettler, Parlour, and Rajan (2005), (2009).

[10]Studying the online login behavior for a sample of 401k retirement plans, Karlsson et al. (2009) and Sicherman et al. (2015) find strong evidence for what they dub the "ostrich effect": During market downturns, investors with equity exposure are significantly less likely to log into their pension accounts.

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investors may be particularly prone to engage in such behavior: When choosing which portfolio stock to ignore, distracted investors may choose the one that is trading at a loss to avoid the associated disutility. Put differently, investors may unconsciously use distracting news coming from other stocks as an excuse to look away from a particular stock. A similar prediction arises in models that assume investors derive utility from realizing gains and losses (Barberis and Xiong (2012), Ingersoll and Jin (2013)): When a realization-utility investor anticipates that he does not want to act on bad information (i.e., realizing a loss), he may decide to stop paying attention to a losing stock when he is distracted. In the empirical analysis, I study how distraction effects interact with a stock's past return. Based on this discussion, I expect a stronger distraction effect for portfolio stocks trading at a loss, especially for sell decisions.[11]

The second behavioral factor is salience. Barber and Odean (2008) document that salient stocks exhibit net buying pressure from individual investors. Hartzmark (2015) finds that both institutional and retail investors are more likely to sell portfolio stocks that rank either best or worst in terms of relative performance since purchase, consistent with the idea that these portfolio positions are more salient to the investor. Bordalo et al. (2013) explore the asset pricing implications of a model in which investors overweigh salient asset payoffs.[12] Cosemans and Frehen (2017) find consistent evidence in the cross-section of stock returns. A common theme of this literature is that salience draws attention at a subconscious level, more or less outside the control of the investor. As such, I expect the distraction effect to be mitigated for salient stocks. I test this prediction using the extreme rank measure proposed by Hartzmark (2015).

III. Data

A. Institutional Trading Data

I obtain institutional trading data from Ancerno (formerly known as Abel Noser Solutions), a leading transaction cost consultant for institutional investors.[13] Puckett and Yan (2011) report that Ancerno trades represent approximately 10%

[11]This prediction also suggests a connection between inattention and the disposition effect, that is, the well-known tendency of investors to prefer selling positions trading at a gain compared to those trading at a loss (for evidence at the individual investor level, see Shefrin and Statman (1985), Odean (1998), and Grinblatt and Keloharju (2001); for evidence at the institutional investor level, see Grinblatt and Han (2005), Frazzini (2006)). Indeed, if distraction causes investors to focus on their winning positions at the expense of their losing positions, I expect distraction to also exacerbate the disposition effect. In Appendix C.4 of the Supplementary Material, I provide tentative evidence for this prediction using a standard measure of an (institutional) investor's propensity to succumb to the disposition effect (Odean (1998)).

[12]Bordalo, Gennaioli, and Shleifer (2014), (2016) write down consumer choice models in which economic agents overweigh salient attributes of a product.

[13]Previous papers using these data include Goldstein, Irvine, Kandel, and Weiner (2009), Chemmanur, He, and Hu (2009), Puckett and Yan (2011), Anand et al. (2012), Hu, McLean, Pontiff, and Wang (2014), Franzoni and Plazzi (2015), Goetzmann, Kim, Kumar, and Wang (2015), Jame (2018), Chakrabarty, Moulton, and Trzcinka (2017), Ben-Rephael and Israelsen (2018), and Eisele, Tamara, Parise, and Peijnenburg (2019).

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of institutional trading volume in U.S. equities. Although institutional investors subscribing to Ancerno are relatively large, their trades and stock holdings have been found to be comparable to those of the average investor in the universe of institutional asset management. My sample period starts in Jan. 1999 and ends in June 2011, after which Ancerno stopped providing an identifier for the trading institution.

Each row in the Ancerno data set represents an executed trade, including information on the date and time of the trade, identity of the stock traded, trade direction (buy or sell), number of shares traded, transaction price, and commissions paid. One crucial feature of the Ancerno data for my purpose is that it contains a unique identifier corresponding to the management company executing the trade (*manager code*). I also have access to a reference file that links manager codes to the names of those companies. Ideally, I would like to have identification at the fund level; however, the Ancerno data do not provide this information.[14] Hence, I conduct my analysis at the manager level. There are 835 different managers in my sample.

To gauge the performance of institutional trades, I map stock returns from the Center for Research in Security Prices (CRSP) onto the Ancerno trades (see Appendix A.1 of the Supplementary Material for details). Because I conduct my main analyses at the manager–stock–time level, working at a daily frequency becomes computationally infeasible. I therefore aggregate trades at the weekly frequency.

B. Link to Form 13F

Using available manager names, I hand-match Ancerno managers to institutional holdings data reported in Form 13F (see Appendix A.2 of the Supplementary Material for details). I find corresponding 13F information for 670 of the 835 managers in my sample. This match serves several purposes. First, as detailed later, I use holdings data in 13F to assemble a list of stocks held by each manager at the end of the previous quarter. Second, I use this match to obtain a link to static investor characteristics reported on Brian Bushee's Web site.[15] I am particularly interested in the classification of managers into "quasi-indexers" and others,[16] as I expect distraction effects to be weaker for managers following passive investment strategies. Third, matching allows me to construct important control variables such as the level of and change in managers' assets under management.

[14] Ancerno contains an additional variable called *clientmgrcode*. However, interactions with Ancerno as well as my reading of the literature convince me that this is not a unique fund identifier. For instance, Jame (2018) writes: "[D]iscussions with Ancerno representatives indicate that different *Clientmgrcodes* within a client-manager generally do not reflect different fund products" (p. 3310).

[15] These data are available at: http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html.

[16] More precisely, Bushee and Noe (2000) and Bushee (2001) classify managers into three categories: quasi-indexers, transient investors, and dedicated investors. The latter two categories differ mainly in their trading activity. Because results for these two categories are similar and I have no expectation as to which group should be more affected, I merge them in my analysis.

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C. Watchlist Construction

My identification rests on the intuitive assumption that investors pay more attention to portfolio stocks than to other stocks. After all, investors must first be aware of a stock before they can buy it and, given that their money is at stake, have strong incentives to watch a portfolio stock vigilantly.

To operationalize this idea, I construct a so-called portfolio watchlist. A given stock i enters this watchlist if manager m reports a positive holding in the stock at the end of the quarter before week t. Let w_{imt} be the portfolio weight of stock i, defined as:

$$w_{imt} = \frac{\text{dollar value of position in stock } i \text{ at the end of the previous quarter}}{\text{total dollar value of positions at the end of the previous quarter}}.$$

As explained later, I use this portfolio watchlist and the corresponding portfolio weights to set up my baseline regression design.

In Appendix B of the Supplementary Material, I report results for an alternative watchlist definition that reflects past trading. Specifically, I consider all stocks that a manager has traded in the past 12 weeks and define as watchlist weights the stocks' fraction of the manager's total dollar volume of trade. Note that there is only limited overlap between the portfolio watchlist and this alternative (trade-based) watchlist. For instance, managers often report holdings for stocks they did not trade recently (in which case the stock is only on the portfolio watchlist), and managers quickly trade in and out of a stock (in which case the stock is only on the trade-based watchlist). My analysis based on the trade-based watchlist therefore provides an important consistency check for the results reported in this article.

D. Sanity Check

If my portfolio watchlists capture stocks that managers are paying attention to, I expect those stocks to be traded with higher probability compared to a random sample of stocks. To test this prediction, I construct randomly assigned placebo watchlists in the following way. First, I randomly reshuffle the holdings data while maintaining differences in holding intensities across managers and stocks.[17] Second, I use the reshuffled data to construct a new "placebo" watchlist. I then compare the fraction of watchlist stocks that are traded in the subsequent week across portfolio and placebo watchlists. Panel B of Table 1 shows that the average fraction of traded watchlist stocks is approximately 4 times larger for the portfolio watchlist than for the placebo watchlist, a difference that is strongly statistically significant.[18] This suggests that institutional investors are indeed paying close attention to their portfolio stocks.

[17] Specifically, when a manager is holding 100 different stocks in the original data for a given week, the placebo watchlists also feature 100 different stocks (which are randomly assigned) for the manager in that week.

[18] For the trade-based watchlist used in Appendix B of the Supplementary Material, the difference in the fraction of stocks traded compared to the placebo watchlist is, if anything, even larger.

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TABLE 1
Summary Statistics

Table 1 describes the data used in this study. Panel A provides summary statistics for all variables used in the panel regressions. All variables are defined in the Appendix. Panel B reports the results of a comparison of the propensity to trade between the portfolio watchlist and a "placebo" watchlist. For the portfolio watchlist, the mean number of stocks on the watchlist, the mean number of those stocks that are traded in the next week, and the fraction of the two are reported. This fraction is compared to a similar fraction of traded stocks for the placebo watchlist (in which stocks are randomly assigned to a given manager's watchlist). The last column reports the *t*-statistic of a difference-in-mean test clustered at the manager level. *** indicates significance at the 1% level.

Panel A. Summary Statistics for All Variables

Variable	Mean	Median	Std. Dev.	P1	P25	P75	P99
Dependent Variables							
TRADE_VOLUME	0.7645	0.0849	4.6140	0.0007	0.0210	0.3883	11.5189
BUY_VOLUME	0.5902	0.0612	3.1035	0.0006	0.0165	0.2599	9.3704
SELL_VOLUME	0.7792	0.0693	5.2791	0.0005	0.0156	0.3639	12.0006
TRADE_DUMMY	0.1144	0.0000	0.3183	0.0000	0.0000	0.0000	1.0000
BUY_DUMMY	0.0780	0.0000	0.2682	0.0000	0.0000	0.0000	1.0000
SELL_DUMMY	0.0691	0.0000	0.2536	0.0000	0.0000	0.0000	1.0000
TRADE_PROFITABILITY	0.0853	0.0771	13.9784	−39.2857	−6.0672	6.2340	39.5683
TRANSACTION_SPREAD	0.3722	0.0000	0.9887	0.0000	0.0000	0.3245	4.4698
Independent Variables							
DISTRACTION	8.1021	3.1167	10.7951	0.0000	0.9456	10.2790	41.6299
STOCKS_ON_WATCHLIST	6.9037	7.1770	1.1246	4.0073	6.1181	7.9095	8.2953
TRADE_NUMBER	3.1149	0.0000	9.0043	0.0000	0.0000	1.0000	53.0000
TRADE_VOLUME_MANAGER	6.0621	0.1559	19.0157	0.0000	0.0336	1.0845	92.5586
AUM	98.0730	19.0161	152.4120	0.2263	4.6257	124.8788	564.5344
CHANGE_IN_AUM	2.4507	2.6060	13.7883	−33.4867	−4.6912	9.5284	50.0000
No. of obs.	40,436,795						

Panel B. Sanity Check: Are Watchlist Stocks More Likely to Be Traded Compared to Other Stocks?

Mean No. of Stocks on Watchlist	Mean No. of Traded Stocks on Watchlist	Fraction Traded (in %)	Placebo: Fraction Traded (in %)	*t*-Stat. of Difference
486.83	64.58	13.30	3.45	11.87***

E. Earnings Announcement Dates

I study how news events in some portfolio stocks affect trading in other portfolio stocks. To proxy for news events, I use earnings announcement dates from Institutional Brokers' Estimate System (IBES) and Compustat. Earnings announcements constitute the most important recurring news releases for individual firms (see, e.g., Beaver (1968), Aharony and Swary (1980), Bernard and Thomas (1989), and Kim and Verrecchia (1994)); they receive significant media attention and institutional investors routinely attend earnings conference calls. As such, they are well suited for my analysis.

Following DellaVigna and Pollet (2009), I use the earlier of the dates in IBES and Compustat when the two dates do not coincide for the same fiscal quarter. I drop the earnings announcement when the firm had another announcement less than 11 days earlier. I define an earnings announcement dummy, EA_DUMMY_{it}, that equals 1 if firm i had an earnings announcement in week t, and 0 otherwise.[19] Overall, there are 274,840 earnings announcement weeks, representing roughly 8% of all stock-week observations in my sample period.

[19] Earnings announcements on a Friday are treated slightly differently. As I do not have the exact time of the announcement, I am not sure whether the earnings news is priced on Friday or on Monday of the following week. For this reason, EA_DUMMY equals 1 for both weeks t and $t+1$ when the announcement occurs on a Friday.

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IV. Methodology

A. Distraction Measure

The key idea I exploit in this article is that different managers pay attention to different stocks and are thus exposed to (and in turn distracted by) different news shocks over time.

My baseline distraction measure is constructed as follows. Recall that w_{jmt} is the weight of stock j in manager m's portfolio, and that EA_DUMMY_{jt} flags stocks with an earnings announcement. For a given stock i, manager m, and week t, my distraction measure is the weighted fraction of watchlist stocks with an earnings announcement,

$$\text{DISTRACTION}_{imt} = \frac{\sum_{j \neq i} w_{jmt} \times \text{EA_DUMMY}_{jt}}{\sum_{j \neq i} w_{jmt}}.$$

Importantly, the weighted average is formed over all watchlist stocks excluding the stock in question. Hence, the measure is not affected by whether stock i itself has an earnings announcement. Note also that my distraction measure always lies between 0 and 1 by definition.

The intuition for using watchlist weights to construct the distraction measure is that managers have a bigger incentive to pay close attention to the earnings announcement of stocks in which they have a large stake.[20] For example, when a stock makes up 30% of an investor's portfolio, he should be much more attentive to any news that affect that stock's value compared to another investor who only owns 0.3% of that stock; after all, for the first investor a much larger fraction of his assets under management are on the line. As such, I expect the first investor to be more distracted from trading other stocks compared to the second investor, for whom the announcing stock is only peripheral.[21]

Panel A of Table 1 shows descriptive statistics for my distraction measure and the other variables used in this study. For example, it reports that the average manager trades a given watchlist stock with a probability of 11% per week and, conditional on trading, trades $764,500. For the average manager, roughly 8.1% of watchlists stocks exhibit an earnings announcement in a given week. The standard deviation of this measure exceeds 10%, ensuring that I have sufficient variation in distraction. I also see that the median manager has $19 billion in assets under management and trades a total volume of $156 million over 12 weeks.

[20] My results for the sanity check reported earlier are consistent with this intuition, as I find that stocks with a positive watchlist weight are significantly more likely to be traded (and thus attended to) than random stocks (with a zero watchlist weight).

[21] In Section VII.A, I show that my results are robust to calculating distraction as the equal-weighted average of the announcement dummy.

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B. Regression Methodology

My main regression specification is

$$(1) \quad y_{imt} = \alpha_{it} + \alpha_{im} + \alpha_{mq(t)} + \beta \text{DISTRACTION}_{imt} + \gamma \text{TRADE_NUMBER}_{imt} + \delta \text{MANAGER_CONTROLS}_{mt} + \varepsilon_{imt},$$

where y_{imt} is 1 of the 4 outcome variables introduced earlier. In principle, each manager could trade every available stock, resulting in an enormous data matrix of possible trades. Working with such a data set is neither feasible nor desirable (because there would be zero trading for the vast majority of observations). I therefore estimate specification (1) only on the subset of portfolio watchlist stocks for each manager.

One crucial feature of my empirical setting is the 3-dimensional data structure, which accommodates a great deal of cross variation in trading activity by including various fixed effects. For example, suppose that in any week, certain stocks attract significant trading, perhaps because they exhibit an earnings announcement or are the target of takeover speculation. Suppose further that distracted managers concentrate on such attention-grabbing stocks (Barber and Odean (2008)), whereas non-distracted ones also trade in other stocks. As a result, distracted managers could appear to be relatively more active, which would confound my identification. Next, consider the stock–manager dimension. Different managers choose to trade different stocks for reasons that are largely unobserved. To the extent that such predispositions correlate with my distraction measure, a naïve comparison of the trading activity across distracted and non-distracted managers is again bound to be problematic. The inclusion of stock × date (α_{it}) and stock × manager (α_{im}) fixed effects immunize against these and related concerns.

As my identification draws on a comparison across managers with different levels of distraction at a given time, I cannot include fund × date fixed effects in my specification.[22] I can, however, control for slow-moving manager characteristics by including manager × quarter ($\alpha_{mq(t)}$) fixed effects.

In addition to these high-dimensional fixed effects, I include several control variables. First, because trading is relatively sticky, I include a measure of past trading activity. Specifically, TRADE_NUMBER is the number of days in which manager m traded stock i within the previous 12 weeks. Second, to account for time-varying manager characteristics, I include several proxies of manager size (matrix MANAGER_CONTROLS): the natural logarithm of the manager's dollar trading volume in the past 12 weeks and the level of and change in assets under management at the end of the previous quarter. Note that the latter two controls, which vary at the manager–quarter level, are subsumed when I include manager × quarter fixed effects. Standard errors are clustered at the manager level.

[22] Specifically, when such fixed effects are included, distraction for non-announcing stocks is not distinguishable from attraction to announcing stocks. In other words, the within-manager variation in my distraction measure is not meaningful in my setting. See Section VII.A for an illustration of this point.

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V. Distraction and Trading Activity

A. Baseline Results

In this section, I examine how distraction affects trading activity. Panel A of Table 2 presents results for the extensive margin of trading (i.e., the dependent variable is TRADE_DUMMY): first for all trades (columns 1–3) and then for buys and sells separately (columns 4–9). It reveals a pervasive distraction effect. In terms of magnitude, I find that a 1-standard-deviation increase in my distraction measure reduces the probability of trading by 3.3% relative to its unconditional mean. Although the effect is not large, it is important to note that this is the average effect across all types of managers, including those that follow passive investment strategies and that are therefore unlikely to be affected by distraction. Thus, the effect is economically meaningful. In Appendix C.1 of the Supplementary Material, I show that the distraction effect does not revert in subsequent weeks. To the contrary, I find that the tendency to trade fewer stocks persists (with decaying magnitude) for up to 2 weeks before turning insignificant. Hence, managers do not catch up on missed trades when the distraction subsides.

Peress and Schmidt (2019) find that distracted retail investors buy but do not sell fewer distinct stocks. In contrast, I find a symmetric effect for the buy and sell decisions of institutional investors (columns 4–9 of Table 2). This makes sense: Contrary to retail investors, institutional investors have much larger portfolios and some routinely go short. Hence, conditional on having decided to sell, a retail investor can choose only among a handful of portfolio stocks, whereas an institutional investor faces a much larger choice set. Because a complex choice is more susceptible to distraction, this explains why there is a significant distraction effect for institutional sells but not for retail sells.

In Panel B of Table 2, I study the impact of distraction on the intensive margin of trade, that is, the decision of how much to buy or sell conditional on trading (TRADE_VOLUME). Rational attention models linking attention choices with signal precision invariably predict that paying less attention leads investors to trade less aggressively. In contrast to this prediction, I find no evidence that distracted managers curb back the dollar volume of their trades (conditional on trading). Based on the estimated standard error, I reject any intensive margin effect that exceeds 1% of the average dollar volume per standard deviation increase in distraction. Hence, even if such an effect exists, its economic magnitude is small. The failure to find an effect at the intensive margin highlights a discrepancy between theory and practice. Although it is reasonable to think that more attention results in better information, trade size in practice seems to be little determined by the perceived precision of this information (e.g., because position limits depend on risk estimates based on historical volatility).

Overall, these results suggest that it is the decision about which stocks to trade that requires the most attention, which is thus most affected by distraction. Hence, my results are most consistent with models that feature a fixed search cost for deciding which stocks to trade (as in Merton (1987)).

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TABLE 2
Distraction and Trading Activity

Table 2 reports results of regressions of managers' trading activity on the distraction measure (specification (1) in the text) at the stock–manager–week level. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In Panel A, trading activity is measured at the extensive margin, that is, by a dummy that equals 1 if the manager trades a given stock in a given week, and 0 otherwise. In Panel B, trading activity is measured at the intensive margin, that is, as the natural logarithm of dollar trading volume by the manager in a given stock and week (the measure is set to missing if the manager does not trade the stock in that week). Columns 1–3 provide results for all trades (buys and sells combined). Columns 4–6 and 7–9 separate between buy and sell transactions, respectively. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 11% levels, respectively.

Panel A. Extensive Margin

	Dependent Variable: TRADE_DUMMY								
	All Trades			Buys			Sells		
Variable	1	2	3	4	5	6	7	8	9
DISTRACTION	−0.0354***	−0.0353***	−0.0340***	−0.0207***	−0.0203***	−0.0214***	−0.0227**	−0.0228**	−0.0208**
	(−3.26)	(−3.23)	(−3.10)	(−3.27)	(−3.20)	(−3.40)	(−2.33)	(−2.33)	(−2.11)
STOCKS_ON_WATCHLIST	0.0085	0.0158	−0.7313***	0.0044	0.0061	−0.4057***	−0.0002	−0.0032	−0.7181*
	(1.30)	(1.57)	(−3.49)	(1.28)	(1.21)	(−3.17)	(−0.05)	(−0.42)	(−1.84)
TRADE_VOLUME_MANAGER	0.0117***	0.0119***	−0.0106***	0.0049***	0.0049***	−0.0072***	0.0055***	0.0054***	−0.0076***
	(4.44)	(4.41)	(−5.06)	(3.81)	(3.80)	(−5.26)	(4.14)	(4.20)	(−4.93)
TRADE_NUMBER	0.0158***	0.0159***	0.0155***	0.0140***	0.0140***	0.0132***	0.0147***	0.0147***	0.0128***
	(8.72)	(8.96)	(14.20)	(25.11)	(25.67)	(35.51)	(50.08)	(46.93)	(29.10)
AUM		−0.0087			−0.0022			0.0037	
		(−1.37)			(−0.61)			(0.64)	
CHANGE_IN_AUM		−0.0040			0.0055			−0.0058	
		(−0.63)			(1.53)			(−1.26)	
No. of obs.	39,093,780	38,921,958	39,093,780	39,093,780	38,921,958	39,093,780	39,093,780	38,921,958	39,093,780
Adj. R^2	0.48	0.48	0.49	0.49	0.49	0.50	0.38	0.38	0.39
Stock × week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes	No	No	Yes

(continued on next page)

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TABLE 2 (continued)
Distraction and Trading Activity

Panel B. Intensive Margin

	Dependent Variable: TRADE_VOLUME								
	All Trades			Buys			Sells		
Variable	1	2	3	4	5	6	7	8	9
DISTRACTION	−0.0838 (−0.35)	−0.0874 (−0.36)	0.0783 (0.39)	0.0048 (0.02)	0.0064 (0.02)	0.1816 (0.76)	−0.2493 (−0.98)	−0.2514 (−0.99)	−0.0791 (−0.34)
STOCKS_ON_WATCHLIST	−0.0870 (−0.99)	−0.1453 (−1.09)	−14.1346** (−2.10)	−0.1520 (−1.43)	−0.3016* (−1.86)	−14.9132** (−2.17)	−0.0102 (−0.14)	−0.0083 (−0.09)	−28.6154** (−2.15)
TRADE_VOLUME_MANAGER	0.1747*** (6.20)	0.1743*** (6.12)	−0.1760*** (−3.72)	0.1742*** (7.00)	0.1737*** (7.01)	−0.1129** (−2.19)	0.1407*** (4.62)	0.1402*** (4.57)	−0.2040*** (−4.35)
TRADE_NUMBER	0.0065*** (3.08)	0.0061*** (2.72)	0.0124*** (4.07)	−0.0056** (−2.13)	−0.0066*** (−2.76)	−0.0025 (−1.00)	−0.0005 (−0.17)	−0.0005 (−0.16)	0.0113*** (3.57)
AUM		0.0967 (0.86)			0.2395* (1.89)			0.0015 (0.02)	
CHANGE_IN_AUM		0.0715 (0.55)			0.2643* (1.76)			−0.0070 (−0.04)	
No. of obs.	3,951,889	3,937,499	3,951,772	2,376,168	2,369,366	2,375,884	2,191,200	2,182,982	2,190,922
Adj. R^2	0.42	0.42	0.45	0.41	0.41	0.45	0.38	0.38	0.41
Stock × week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes	No	No	Yes

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B. Quasi-Indexers

If my results are due to investor distraction as I posit, I expect them to be concentrated for certain types of managers. For example, some managers openly or covertly mimic an index. Because such passive investment strategies require little attention, there is no scope for distraction. I use the investor classification by Bushee and Noe (2000) and Bushee (2001) to sort managers into "quasi-indexers" and others, and repeat my regression analysis for these 2 groups. Table 3 reports the results. Panel A documents that the distraction effect at the extensive margin is highly concentrated in the group of non-indexers: Although not significant for quasi-indexers (columns 1 and 3), the effect for non-indexers (columns 2 and 4) is double the magnitude of the baseline effect documented in Panel A of Table 2, with a 1-standard-deviation increase in distraction leading to a 7% reduction in the propensity to trade. As shown at the bottom of Panel A, the differences between the 2 subgroups are statistically significant. In contrast, Panel B reveals no differences in distraction at the intensive margin. Indeed, the coefficient on the distraction measure is insignificant for both quasi-indexers and others.

C. Additional Sample Splits

In this subsection, I provide additional sample splits for the trade propensity dummy to examine which type of managers is more distracted. Each row in Table 4 represents one sample split, including the test statistic for the difference (columns 4 and 8). For brevity, I show only the coefficient on the distraction measure, although I always run the full specification with controls and fixed effects.

My first two sample splits are meant to reinforce the point that active management requires more attention and thus suffers more from inattention. First, I classify managers into terciles based on their average watchlist turnover (defined as dollar trading volume over the total market capitalization of the watchlist portfolio). Managers who trade only to invest/divest as a function of fund inflows/outflows are likely to score low on this measure and hence I expect them to be less distracted. Row 1 in Table 4 confirms this expectation: Distraction is strongest for high-turnover managers, whereas low-turnover managers do not appear to be distracted. This difference is statistically significant.

Second, I attempt to separate between rebalancing trades and flow-induced trades. The idea is that rebalancing trades involve stock selection and are thus prone to distraction. In contrast, flow-induced trades lead to a mechanic rescaling of existing positions. To capture the degree of flow-induced versus rebalancing trades, I calculate, for each week, the minimum of a manager's dollar buys and dollar sells, divided by his total trading volume.[23] I then average across weeks and call this measure trade activeness. Managers who score high on this measure buy and sell a lot at the same time, thereby rebalancing their portfolios from one stock to another. Managers who score low on this measure either buy or sell in a given week, presumably because they are responding to in- and outflows to and from their funds. I then run my analysis separately for managers in the top, middle,

[23]This measure is similar in spirit to the portfolio turnover proxy used in Wermers (2000) and Brunnermeier and Nagel (2004) except that I scale by total trading volume rather than portfolio holdings.

TABLE 3
Excluding Quasi-Indexers

Table 3 splits the sample split by whether a manager is a quasi-indexer. I run regressions of managers' trading activity on the distraction measure (specification (1) in the text) at the stock–manager–week level. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In Panel A, trading activity is measured at the extensive margin, that is, by a dummy that equals 1 if the manager trades a given stock in a given week, and 0 otherwise. In Panel B, trading activity is measured at the intensive margin, that is, as the natural logarithm of dollar trading volume by the manager in a given stock and week (the measure is set to missing if the manager does not trade the stock in that week). In Panel B, columns 1 and 2 report results for the specification with assets under management (AUM) controls; columns 3 and 4 report results for the specification with manager × quarter fixed effects (which subsume the AUM controls). Columns 1 and 3 include only managers who are identified as quasi-indexers according to the classification by Bushee and Noe (2000) and Bushee (2001), and columns 2 and 4 exclude these managers. The statistical significance of the difference between the 2 subgroups is reported at the bottom of the table. This significance is based on a regression model where all explanatory variables and fixed effects are interacted with a dummy equal to 1 if the manager is a quasi-indexer. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. ** and *** indicate significance at the 5% and 1% levels, respectively.

Panel A. Extensive Margin

	Dependent Variable: TRADE_DUMMY			
	Quasi-Indexer	Other	Quasi-Indexer	Other
Variable	1	2	3	4
DISTRACTION	−0.0033	−0.0737***	−0.0006	−0.0743***
	(−0.32)	(−3.04)	(−0.06)	(−3.12)
STOCKS_ON_WATCHLIST	0.0231	0.0069	−1.0397***	−0.5832**
	(1.64)	(0.92)	(−3.07)	(−2.31)
TRADE_VOLUME_MANAGER	0.0109***	0.0138***	−0.0089***	−0.0210***
	(3.59)	(4.50)	(−3.94)	(−4.84)
TRADE_NUMBER	0.0141***	0.0174***	0.0143***	0.0157***
	(8.37)	(12.58)	(10.77)	(14.93)
AUM	−0.0097	−0.0020		
	(−1.43)	(−0.30)		
CHANGE_IN_AUM	−0.0052	0.0028		
	(−0.63)	(0.32)		
No. of obs.	21,605,783	10,731,575	21,662,609	10,834,767
Adj. R^2	0.56	0.36	0.57	0.37
Difference in distraction (*t*-stat.)	2.72***		2.89***	
Stock × week fixed effects	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	Yes

Panel B. Intensive Margin

	Dependent Variable: TRADE_VOLUME			
	Quasi-Indexer	Other	Quasi-Indexer	Other
Variable	1	2	3	4
DISTRACTION	−0.3457	0.1158	−0.1371	0.1665
	(−1.30)	(1.08)	(−0.61)	(1.19)
STOCKS_ON_WATCHLIST	−0.3831***	0.0108	−0.2396***	−0.5126
	(−2.60)	(0.11)	(−2.84)	(−0.29)
TRADE_VOLUME_MANAGER	0.1397***	0.1122***	−0.2435***	−0.2034***
	(6.01)	(2.84)	(−6.26)	(−7.05)
TRADE_NUMBER	0.0035	0.0055	0.0095***	0.0165***
	(1.18)	(1.15)	(2.89)	(4.25)
AUM	0.2435***	0.0303		
	(2.85)	(0.19)		
CHANGE_IN_AUM	0.0396	0.1426		
	(0.16)	(0.91)		
No. of obs.	1,767,950	912,637	1,773,262	919,167
Adj. R^2	0.47	0.35	0.50	0.38
Difference in distraction (*t*-stat.)	−1.23		−1.07	
Stock × week fixed effects	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	Yes

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TABLE 4

Trading Propensity: Sample Split by Manager Characteristics

Table 4 reports results for sample splits for regressions of a manager's trading propensity (TRADE_DUMMY) on the distraction measure (specification (1) in the text) at the stock–manager–week level. Each row represents a different sample split as indicated in the row header (and explained in Section III.C). For brevity, the table reports only the coefficient on the distraction measure. Columns 1–3 report results for the specification with assets under management (AUM) controls; columns 5–7 report results for the specification with manager × quarter fixed effects (which subsume the AUM controls). Columns 4 and 8 report test statistics for the difference between the high/yes and low/no groups. These significance tests are based on a regression model where all explanatory variables and fixed effects are interacted with a dummy equal to 1 if an observation is in the high/yes group and 0 if it is in the low/no group. The split variables are defined as follows: TURNOVER is the manager's average dollar trading volume in watchlist stocks over their market capitalization; TRADE_ACTIVENESS is defined as the minimum of a manager's dollar buys and dollar sells, divided by his total trading volume; INDUSTRY_CONCENTRATION is defined as the Herfindahl concentration index of a manager's reported stock holdings across Fama–French 49 industries; INSTITUTION_AUM is the institution's average assets under management; WATCHLIST_SIZE is the average number of stocks on the institution's watchlist; AVERAGE_PROFITS is the average 48-week-ahead portfolio return of the watchlist portfolio; and HEDGE_FUND is a dummy variable equal to 1 for hedge funds, and 0 otherwise (obtained from Russell Jame; explained in Jame (2018)). Standard errors are clustered at the manager level, and *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: TRADE_DUMMY							
Variable	Low/No	Medium	High/Yes	*t*-Stat. Difference	Low/No	Medium	High/Yes	*t*-Stat. Difference
	1	2	3	4	5	6	7	8
TURNOVER	0.0025	−0.0395***	−0.0642**	2.42**	0.0020	−0.0397***	−0.0551**	2.05**
	(0.26)	(−2.69)	(−2.48)		(0.22)	(−2.75)	(−2.09)	
TRADE_ACTIVENESS	0.0112	−0.0048	−0.0738***	3.61***	0.0118	−0.0032	−0.0702***	3.36***
	(1.12)	(−0.51)	(−3.45)		(1.12)	(−0.33)	(−3.19)	
INDUSTRY_CONCENTRATION	−0.0418*	−0.0359***	−0.0148	−1.06	−0.0391**	−0.0336***	−0.0179*	−0.81
	(−1.77)	(−2.70)	(−1.35)		(−2.31)	(−2.61)	(−1.75)	
INSTITUTION_AUM	−0.0186**	−0.0488**	−0.0639	1.03	−0.0188**	−0.0430**	−0.0564	0.84
	(−2.38)	(−2.17)	(−1.47)		(−2.41)	(−2.09)	(−1.27)	
WATCHLIST_SIZE	−0.0122	−0.0130	−0.0409**	1.33	−0.0119	−0.0142	−0.0393**	1.26
	(−0.92)	(−1.15)	(−2.24)		(−0.89)	(−1.32)	(−2.15)	
AVERAGE_PROFITS	−0.0508***	−0.0204*	−0.0431*	−0.27	−0.0542***	−0.0175	−0.0512**	−0.11
	(−2.85)	(−1.77)	(−1.93)		(−3.27)	(−1.54)	(−2.38)	
HEDGE_FUND	−0.0379***	N/A	−0.0208	−1.00	−0.0356***	N/A	−0.0243*	−0.65
	(−3.08)		(−1.50)		(−2.89)		(−1.73)	
Past trade controls	Yes	Yes	Yes		Yes	Yes	Yes	
AUM and CHANGE_IN_AUM	Yes	Yes	Yes		No	No	No	
Stock × week fixed effects	Yes	Yes	Yes		Yes	Yes	Yes	
Manager × stock fixed effects	Yes	Yes	Yes		Yes	Yes	Yes	
Manager × quarter fixed effects	No	No	No		Yes	Yes	Yes	

and bottom terciles in terms of this trade activeness measure. Row 2 in Table 4 reveals that, as expected, the strongest distraction effect is for managers with high trade activeness, that is, managers who make active rebalancing decisions on a regular basis. For the top group, a 1-standard-deviation increase in distraction is associated with a 7% reduction in the propensity to trade, whereas there is no discernible distraction effect for the bottom group. This difference is again statistically significant.

My third sample split is meant to disentangle between the distraction effect and the information effect of earnings news. The idea is that for stocks in the same industry as the announcing stock, the announcement provides information and may thus attract rather than distract investors' attention (e.g., Patton and Verardo (2012)). Such a confounding effect should be particularly strong for managers with concentrated industry portfolios and hence I expect to find a weaker distraction effect for this group. I therefore sort managers into terciles based on the average Herfindahl index of their watchlist stocks across the Fama–French 49 industries (available on Kenneth French's Web site, http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html). Row 3 in Table 4 shows that,

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consistent with my expectation, the distraction effect is larger for managers with low industry concentration (although the difference from the high-concentration group is not significant).

Fourth, I split managers by average assets under management over the sample period.[24] Row 4 in Table 4 shows that only small and medium-sized managers are significantly distracted. At first glance, the economic magnitude of the distraction effect seems to be larger for big institutions, although the difference is not significant. It turns out that these magnitudes are misleading, however, as the underlying propensity to trade is higher for large institutions. When scaled appropriately, the economic magnitude of the distraction effect appears comparable across size groups (a 1-standard-deviation increase in distraction reduces the propensity to trade by 3%–4% and 4%–5% for small and large managers, respectively). It may nonetheless seem surprising that large institutions are as distracted as small institutions. After all, large institutions presumably comprise more different funds, which should attenuate the distraction effect through measurement error. Other factors may work against this attenuation, however. First, larger institutions are typically less focused (e.g., have a lower industry concentration), which means that the distraction effect is less confounded by the information effect of earnings news (see earlier discussion). Second, even when an institution has many funds, some trading decisions may yet be taken at or depend on input from the institutional level (e.g., because trades are executed by a single trading division, because the same research division gives recommendations for all funds within the institution, or because trades are authorized by a groupwide risk management division).

The argument that less focused institutions are more prone to distraction also explains the results for my fifth sample split, where I find a stronger distraction effect for managers with a large number of watchlist stocks (row 5 in Table 4). Sixth, I sort managers by average trading profits, measured as the mean (watchlist-weighted) return of the watchlist portfolio over a 48-week horizon. There is a tendency for managers with low or medium profits to be more distracted than those with high profits, but this difference is not significant. This evidence is consistent with skilled managers relying less on (public) earnings news, presumably because they are able to uncover valuable private information (Kacperczyk and Seru (2007)).

Finally, I check whether distraction is stronger or weaker for the 75 hedge funds in my sample.[25] I have no prior for this exercise: On the one hand, hedge funds are more likely to follow active investment strategies, which should make them prone to distraction. On the other hand, hedge funds are more focused and more likely to employ trading algorithms, which should limit their capacity to be

[24] For this sample split, I do not classify managers into equal terciles because this results in model overfitting for the tercile of managers with low assets under management. This is because small managers' watchlists do not overlap enough, which means that my full model including stock × week and manager × stock fixed effects is poorly identified. Instead, to balance the number of observations in the different size groups, I classify the 60% smallest institutions as low assets under management, the 20% largest institutions as high assets under management, and all others as medium assets under management.

[25] I thank Russell Jame for providing the hedge fund identifiers (described in Jame (2018)).

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distracted. My results, shown in row 7 of Table 4, appear more consistent with the second interpretation, as the distraction effect for hedge funds is not statistically significant. I acknowledge, however, that the insignificance could also arise from low statistical power, as the economic magnitude of the distraction effect is not much different from that of other institutions.

In summary, the results from this section show that news events divert attention from trading in other stocks, especially for managers who follow active investment strategies across different industries.

VI. Distraction and Performance

In this section, I study whether distraction affects managers' performance. I consider two possible channels: distracted managers may trade less profitably and incur higher transaction costs.

A. Trade Profitability

To study whether distraction affects trade profitability, I repeat my panel regression from specification (1) with the post-trade return as the dependent variable (TRADE_PROFITABILITY). For buys, the posttrade return is simply the stock return over the subsequent 4 weeks. For sells, it is the stock return over 4 weeks times -1.[26] Importantly, these post-trade returns are measured starting from the market close at the end of the week of the trade; hence, any effect on this measure cannot come from transaction costs incurred by these trades.

Note that because stock $\times$ time fixed effects are included in my regression specification, I essentially test whether distracted managers are more or less likely to be on the "winning side of a trade." In other words, I test whether distracted managers, compared to less distracted managers, are more likely to buy (sell) stocks that subsequently underperform (outperform).[27] As argued in Section I, this is a clear prediction of any type of attention model in which there is a link between attention and signal precision.

I calculate trade profitability over 4 weeks because a 1-month horizon is commonly used in research on investors' performance (e.g., Daniel, Grinblatt, Titman, and Wermers (1997), Coval and Moskowitz (2001)) and is in line with previous analyses of the performance of Ancerno institutions (Puckett and Yan (2011), Chakrabarty et al. (2017), and Jame (2018)). Underlying this choice is the assumption that information advantages by institutional investors should be fleeting and may thus be detectable only for post-trade returns in the short term.[28,29]

[26] Buys (sells) here mean trading weeks in which the manager's net trade imbalance is positive (negative), meaning that he bought (sold) more of the stock than he sold (bought).

[27] This also implies that any stock-specific factors, such as risk adjustments or price pressures, cannot explain these results.

[28] Of course, investors often hold their stocks for longer than 1 month, perhaps because they also want to earn the market risk premium.

[29] In Appendix C.2 of the Supplementary Material, I find economically similar albeit statistically weaker results when I recalculate trade profitability using managers' median investment horizons inferred from their round-trip trades in the Ancerno sample. The median horizon across managers is approximately 12 weeks. The fact that these results are statistically weaker should not be surprising, because longer term returns are necessarily noisier.

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Panel A of Table 5 presents the results for the overall sample. I find a statistically significant distraction effect on trade profitability: A 1-standard-deviation increase in distraction decreases average post-trade profitability over the subsequent 4 weeks from roughly 0.1% to −0.1%. Thus, although the institutional

TABLE 5
Distraction and Trade Profitability

Table 5 reports results of regressions of trade profitability on the distraction measure (specification (1) in the text) at the stock–manager–week level. The dependent variable is the post-trade return for buys and the post-trade return times −1 for sells. Post-trade returns are calculated over the subsequent 4 weeks of the transaction week. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. Panel A reports the results for the whole sample. Panel B splits the sample by whether a manager is a quasi-indexer. In Panel B, columns 1 and 3 include only managers who are identified as quasi-indexers according to the classification by Bushee and Noe (2000) and Bushee (2001), and columns 2 and 4 excludes these managers. The statistical significance of the difference between the 2 subgroups is reported at the bottom of the table. This significance is based on a regression model where all explanatory variables and fixed effects are interacted with a dummy equal to one if the manager is a quasi-indexer. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. ** and *** indicate significance at the 5% and 1% levels, respectively.

Panel A. Overall

	Dependent Variable: TRADE_PROFITABILITY		
Variable	1	2	3
DISTRACTION	−0.0185**	−0.0199**	−0.0225**
	(−2.14)	(−2.26)	(−2.58)
STOCKS_ON_WATCHLIST	0.0004	−0.0006	−0.0978
	(0.34)	(−0.31)	(−1.32)
TRADE_VOLUME_MANAGER	−0.0007	−0.0007	−0.0009
	(−1.27)	(−1.33)	(−0.69)
TRADE_NUMBER	−0.0001***	−0.0001**	−0.0001***
	(−2.83)	(−2.55)	(−3.23)
AUM		0.0015	
		(1.04)	
CHANGE_IN_AUM		−0.0038	
		(−1.00)	
No. of obs.	3,936,029	3,921,673	3,935,910
Adj. R^2	0.10	0.10	0.11
Stock × week fixed effects	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes

Panel B. Excluding Quasi-Indexers

	Dependent Variable: TRADE_PROFITABILITY			
	Quasi-Indexer	Other	Quasi-Indexer	Other
Variable	1	2	3	4
DISTRACTION	0.0046	−0.0290***	0.0033	−0.0316***
	(0.66)	(−3.06)	(0.48)	(−3.02)
STOCKS_ON_WATCHLIST	0.0030	−0.0036	−0.0955	−0.0614
	(1.59)	(−1.31)	(−0.96)	(−0.38)
TRADE_VOLUME_MANAGER	0.0003	−0.0005	0.0002	0.0006
	(0.62)	(−0.64)	(0.24)	(0.30)
TRADE_NUMBER	−0.0001***	−0.0001	−0.0001***	−0.0001
	(−4.67)	(−0.93)	(−4.85)	(−1.60)
AUM	−0.0028**	0.0013		
	(−2.16)	(0.99)		
CHANGE_IN_AUM	0.0012	−0.0084		
	(0.43)	(−1.10)		
No. of obs.	1,759,716	909,281	1,765,021	915,790
Adj. R^2	0.10	0.11	0.11	0.12
Difference in distraction (*t*-stat.)	2.19**		2.79***	
Stock × week fixed effects	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	Yes

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managers in my sample on average earn money with their trades, they begin losing money when they are distracted. Panel B further shows that this distraction effect is pronounced for managers classified as non-indexers, with a magnitude that is twice as large as for the baseline sample. In contrast, managers classified as quasi-indexers do not see a reduction in post-trade profitability when a large fraction of watchlist stocks report their earning figures. This absence of a distraction effect is consistent with the idea that quasi-indexers do not choose which stocks to trade and hence do not engage in a decision-making process that is prone to distraction.

B. Transaction Costs

I now examine whether differences in distraction affect order execution quality. To this end, I regress a measure of managers' incurred transaction costs on distraction in my usual panel regression framework (specification (1)). Given the inclusion of stock × date and stock × manager fixed effects, I am essentially testing whether distracted managers trade at worse prices compared to nondistracted managers.

My proxy for order execution quality is the average relative transaction spread (TRANSACTION_SPREAD), defined as the difference between the transaction price and the previous-day closing price for buys (and the same difference times −1 for sells), scaled by the previous-day closing price. I then average (weighted by trading volume) the incurred spreads for buys and sells for each manager–stock–week. Transaction spreads are missing when a manager does not trade the stock in a given week.

Using this measure as the dependent variable in Table 6, I find support for the notion that distraction hurts execution quality: The effect of distraction is positive and significant but economically small (Panel A). A 1-standard-deviation increase in distraction leads to a rise in transaction spread of approximately 3.5% relative to its unconditional mean (see Table 1). Panel B further shows that this distraction effect has a similar magnitude for quasi-indexers and other institutional investors. This finding suggests that although a lack of attention does not affect the trade profitability for quasi-indexers, it can affect their trade execution quality.

Taken together, my results show that distraction hurts performance: Distracted managers trade less profitably and incur slightly higher transaction costs. Although the former finding is in line with the idea that more attention should result in better information, the explanation for the latter finding is less clear. One possibility is that distracted managers send their trading orders with delay, giving the in-house trading desk or the outside broker less time to "work the order." Another possibility is that the order-handling trader is itself distracted. In both cases, one may expect the orders by distracted managers to be less fragmented. In Appendix C.3 of the Supplementary Material, I find results that are consistent with this idea, although the evidence is marginally significant at best (presumably because my proxies for the extent of order fragmentation are too noisy).

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TABLE 6
Distraction and Transaction Costs

Table 6 reports results of regressions of transaction costs on the distraction measure (specification (1) in the text) at the stock–manager–week level. The dependent variable is the relative transaction spread, defined as the difference between the transaction price and the previous-day closing price for buys (and the same difference times −1 for sells), scaled by the previous-day closing price. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. Panel A reports results for the whole sample. Panel B reports a sample split by whether a manager is a quasi-indexer. In Panel B, columns 1 and 3 include only managers who are identified as quasi-indexers according to the classification by Bushee and Noe (2000) and Bushee (2001), and columns 2 and 4 excludes these managers. The statistical significance of the difference between the 2 subgroups is reported at the bottom of the table. This significance is based on a regression model where all explanatory variables and fixed effects are interacted with a dummy equal to 1 if the manager is a quasi-indexer. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Overall

	Dependent Variable: TRANSACTION_SPREAD		
Variable	1	2	3
DISTRACTION	0.0012***	0.0011**	0.0013**
	(2.83)	(2.36)	(2.10)
STOCKS_ON_WATCHLIST	0.0001	0.0002	−0.0003
	(0.59)	(0.89)	(−0.06)
TRADE_VOLUME_MANAGER	0.0000	0.0000	0.0001*
	(0.80)	(0.76)	(1.81)
TRADE_NUMBER	−0.0000***	−0.0000***	−0.0000***
	(−5.13)	(−5.02)	(−4.68)
AUM		−0.0001	
		(−1.07)	
No. of obs.	3,950,989	3,936,599	3,950,871
Adj. R^2	0.20	0.20	0.21
Stock × week fixed effects	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes

Panel B. Excluding Quasi-Indexers

	Dependent Variable: TRANSACTION_SPREAD			
	Quasi-Indexer	Other	Quasi-Indexer	Other
Variable	1	2	3	4
DISTRACTION	0.0009*	0.0010	0.0011*	0.0008
	(1.81)	(1.62)	(1.72)	(1.58)
STOCKS_ON_WATCHLIST	0.0005***	0.0002	−0.0008	0.0005
	(4.35)	(1.43)	(−0.09)	(0.07)
TRADE_VOLUME_MANAGER	0.0001	0.0000	0.0001	0.0002**
	(1.15)	(0.65)	(0.77)	(2.60)
TRADE_NUMBER	−0.0000***	−0.0000***	−0.0000***	−0.0000***
	(−3.62)	(−6.16)	(−6.92)	(−6.49)
AUM	−0.0002	0.0000		
	(−1.64)	(0.08)		
CHANGE_IN_AUM	0.0007	−0.0005		
	(0.82)	(−1.34)		
No. of obs.	1,767,715	912,207	1,773,026	918,738
Adj. R^2	0.19	0.19	0.20	0.20
Difference in distraction (*t*-stat.)	−0.35		−0.64	
Stock × week fixed effects	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	Yes

VII. Rational and Irrational Attention Allocation

A. Rational Attention Allocation

When investors are conscious about their attention constraints, they should reallocate their attention on the basis of a cost–benefit analysis. In particular, they

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should be less likely to divert attention from stocks that matter more to their utility (Corwin and Coughenour (2008)), for example, stocks in which they have a large portfolio stake or for which they expect the release of value-relevant information. I now test this prediction.

In Table 7, I return to the trade propensity regressions and interact my distraction measure with 2 measures of the stock's importance for a given investor. The first interaction variable is the watchlist weight (i.e., the portfolio weight). Intuitively, as attention constraints tighten, rational investors want to remain more attentive to stocks in which they have more money at stake. The results, shown in columns 1–3, strongly confirm this prediction: Managers are less distracted from trading in stock positions with a large watchlist weight.[30]

In my second test, I interact the distraction measure with an earnings announcement dummy, which flags weeks in which the stock has an earnings announcement. Because these announcements can be anticipated,

TABLE 7

Trading Propensity: Rational Attention Allocation?

Table 7 reports results of regressions of a manager's trading propensity (TRADE_DUMMY) on the distraction measure (specification (1) in the text) at the stock–manager–week level. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In columns 1–3, the distraction measure is interacted with the watchlist weight (WL_WEIGHT) of a stock, which equals the portfolio weight at the end of the previous quarter. In this case, the level effect of the interaction variable is added as an additional control. In columns 4–6, the distraction measure is interacted with a dummy variable (EA) that equals 1 if there is an earnings announcement for the given stock × week, and 0 otherwise. In this case, the level effect of the interaction variable is subsumed by the stock × week fixed effects. All other variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: TRADE_DUMMY					
Variable	1	2	3	4	5	6
DISTRACTION	−0.0385***	−0.0384***	−0.0367***	−0.0384***	−0.0382***	−0.0371***
	(−3.48)	(−3.45)	(−3.28)	(−3.48)	(−3.44)	(−3.35)
DISTRACTION × WL_WEIGHT	0.5364***	0.5423***	0.4844***			
	(3.64)	(3.58)	(3.53)			
WL_WEIGHT	1.7576***	1.7962***	2.0706***			
	(5.97)	(6.08)	(7.57)			
DISTRACTION × EA				0.0164*	0.0157*	0.0163**
				(1.84)	(1.75)	(2.06)
STOCKS_ON_WATCHLIST	0.0115*	0.0184*	−0.9061***	0.0085	0.0158	−0.7311***
	(1.72)	(1.82)	(−4.09)	(1.30)	(1.57)	(−3.49)
TRADE_VOLUME_MANAGER	0.0118***	0.0119***	−0.0106***	0.0117***	0.0119***	−0.0106***
	(4.47)	(4.44)	(−5.06)	(4.44)	(4.41)	(−5.06)
TRADE_NUMBER	0.0158***	0.0158***	0.0153***	0.0158***	0.0159***	0.0155***
	(8.80)	(9.04)	(14.50)	(8.72)	(8.96)	(14.20)
AUM		−0.0082			−0.0087	
		(−1.31)			(−1.37)	
CHANGE_IN_AUM		−0.0039			−0.0040	
		(−0.62)			(−0.63)	
No. of obs.	39,093,780	38,921,958	39,093,780	39,093,780	38,921,958	39,093,780
Adj. R^2	0.48	0.48	0.49	0.48	0.48	0.49
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes

[30]Note that the average portfolio weight is only 0.2%. Hence, the positive interaction term does not overcompensate for the distraction effect.

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attention-constrained investors can rationally choose to remain attentive to them, in which case I expect the distraction effect on trading propensity to be reduced. The results, shown in columns 4–6 of Table 7, are again consistent with this expectation as the interaction coefficient is positive and significant. Hence, managers appear to be less distracted for stocks with an imminent earnings announcement compared to other stocks.[31]

Overall, these tests support the idea that managers allocate their attention on the basis of rational considerations: As attention becomes scarce, they choose to remain relatively more attentive to stocks in which they have more money at stake and/or for which they anticipate the announcement of important information. Next, I explore other factors that may influence managers' attention choices.

B. Exacerbating Behavioral Biases?

Although an all-encompassing behavioral alternative to the rational expectation framework does not exist, there are some behavioral factors that are known to trigger attention/inattention. One prominent idea in this domain, the so-called ostrich effect, springs from belief-based utility and describes an "active information avoidance" (Caplin and Leahy (2001), Brunnermeier and Parker (2005); see also Karlsson et al. (2009), Sicherman et al. (2015) for empirical evidence using data on online account logins). At the heart of these models lies the assumption that information can have a direct impact on utility ("hedonic value") that is separate from its usefulness. In a similar spirit, models with realization utility assume that investors derive additional utility from realizing gains and losses over and above the utility derived from paper gains/losses (Barberis and Xiong (2012), Ingersoll and Jin (2013)).[32]

Motivated by these theories, I study how the distraction effect for trading propensity interacts with the past returns of a stock position (measured over the previous 4 weeks). The idea is that distracted managers may choose to selectively overlook stocks that have done poorly to avoid facing the negative emotions associated with acknowledging a trading loss. Table 8 reports the results of this exercise. As indicated by the positive albeit weakly significant interaction coefficient shown in columns 1 and 2, the distraction effect is less pronounced for stock positions that trade at a gain. The flip side of this is that, consistent with ostrich-type behavior, distracted investors are particularly unlikely to trade in stocks that have had low returns.

The realization utility framework further predicts that the interaction between distraction and past stock performance should depend on the type of trade: When a stock has done poorly, investors with an open long position may be

[31] In unreported analyses, I find that distraction effects are not weaker and, if anything, even stronger for stocks with above-median absolute returns and share turnover. This evidence is less consistent with rational attention allocation. One possibility is that investors choose to pay less attention to stocks for which they expect news to be bad, a behavior that is consistent with the so-called ostrich effect (Karlsson et al. (2009), Sicherman et al. (2015)). I explore this possibility in detail later.

[32] Using measures of neural activity obtained from functional magnetic resonance imaging (fMRI), Frydman, Barberis, Camerer, Bossaerts, and Rangel (2014) find evidence consistent with such type of preferences.

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TABLE 8
Trading Propensity: Exacerbating Behavioral Biases?

Table 8 reports results of regressions of a manager's trading propensity on the distraction measure (specification (1) in the text) at the stock–manager–week level. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In columns 1–3, the dependent variable is trading propensity, that is, a dummy variable that equals 1 if the manager buys or sells the stock in a given week, and 0 otherwise. In columns 4–6, the dependent variable is buying propensity, that is, a dummy that equals 1 if the manager buys a given stock in a given week, and 0 otherwise. In columns 7–9, the dependent variable is selling propensity, that is, a dummy variable that equals 1 if the manager sells the stock in a given week, and 0 otherwise. In all columns, the distraction measure is interacted with the stock's return in the previous 4 weeks (PAST_RETURN). The level effect of the interaction variable is subsumed by the stock × week fixed effects. All other variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable								
	TRADE_DUMMY			BUY_DUMMY			SELL_DUMMY		
Variable	1	2	3	4	5	6	7	8	9
DISTRACTION	−0.0356***	−0.0355***	−0.0341***	−0.0206***	−0.0203***	−0.0213***	−0.0228**	−0.0229**	−0.0210**
	(−3.29)	(−3.26)	(−3.12)	(−3.27)	(−3.20)	(−3.39)	(−2.35)	(−2.35)	(−2.13)
DISTRACTION × PAST_RETURN	0.0530*	0.0537*	0.0483	−0.0046	−0.0030	−0.0233	0.0491**	0.0478**	0.0440**
	(1.88)	(1.83)	(1.63)	(−0.18)	(−0.12)	(−1.00)	(2.55)	(2.32)	(2.17)
STOCKS_ON_WATCHLIST	0.0086	0.0158	−0.7322***	0.0044	0.0060	−0.4068***	−0.0002	−0.0032	−0.7197*
	(1.30)	(1.58)	(−3.49)	(1.28)	(1.21)	(−3.18)	(−0.04)	(−0.42)	(−1.84)
TRADE_VOLUME_MANAGER	0.0117***	0.0119***	−0.0106***	0.0049***	0.0049***	−0.0072***	0.0055***	0.0054***	−0.0076***
	(4.44)	(4.41)	(−5.06)	(3.81)	(3.80)	(−5.26)	(4.14)	(4.19)	(−4.93)
TRADE_NUMBER	0.0158***	0.0159***	0.0155***	0.0140***	0.0140***	0.0132***	0.0147***	0.0147***	0.0128***
	(8.73)	(8.97)	(14.20)	(25.16)	(25.73)	(35.51)	(50.25)	(47.09)	(29.16)
AUM		−0.0087			−0.0022			0.0038	
		(−1.37)			(−0.61)			(0.64)	
CHANGE_IN_AUM		−0.0040			0.0055			−0.0058	
		(−0.63)			(1.53)			(−1.26)	
No. of obs.	39,067,464	38,895,699	39,067,464	39,067,464	38,895,699	39,067,464	39,067,464	38,895,699	39,067,464
Adj. R^2	0.48	0.48	0.49	0.49	0.49	0.50	0.38	0.38	0.39
Stock × week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes	No	No	Yes

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particularly reluctant to sell the stock (as this would lock in a trading loss) but they may be fine buying it. The results shown in columns 4–9 of Table 8 confirm this prediction: Distracted investors are significantly less likely to sell stocks with low returns but are not less likely to buy them.

These results lend support to theories assuming that information has a direct utility impact: By choosing to pay less attention to stocks trading at a loss, investors can postpone facing the disutility associated with them (e.g., Karlsson et al. (2009), Barberis and Xiong (2012)). Moreover, the fact that I find a stronger distraction effect for sells following low returns hints at a connection to the disposition effect, that is, the well-documented tendency that investors sell their winners too early while holding on to their losers (e.g., Shefrin and Statman (1985), Odean (1998)).[33]

C. Interaction with Salience

A recent strand in the behavioral economics literature argues that an important driver of attention allocation decisions is salience (Bordalo et al. (2014), (2016)), broadly defined as how much an attribute of a certain product (e.g., the payoff of a financial asset) stands out compared to the same attribute of the average product in a choice set.[34] Perhaps the cleanest piece of evidence consistent with salience-based decision making is the "rank effect" documented by Hartzmark (2015): Investors are significantly more likely to sell the worst and best performing positions of their portfolios, presumably because these positions are more salient.[35]

In this subsection, I explore how the distraction effect interacts with the salience of a stock position, which I proxy for using an extreme rank dummy as proposed by Hartzmark (2015). Specifically, I define the extreme rank dummy to be equal to 1 if the return of a watchlist stock is the lowest or highest over the previous 4 weeks compared to all other watchlist stocks for a given investor. I then interact this variable with my distraction measure in the baseline specification. Because salient stocks subconsciously catch investors' attention (i.e., without involving a distraction-prone choice on the part of the investor), I expect the distraction effect to be mitigated for salient stock positions.

My results, shown in Table 9, confirm this intuition: The distraction effect is almost fully reversed for stock positions that rank at the bottom or top in terms of past performance relative to other watchlist stocks. Another interpretation of this finding is that the importance of salience for explaining which stocks are traded is exacerbated when attention is scarce. This result is more pronounced for

[33] In Appendix C.4 of the Supplementary Material, I explore this connection and find results that are weakly consistent with the idea that distracted managers are more prone to the disposition effect.

[34] Bordalo et al. (2013) and Cosemans and Frehen (2017) document the implications of salience for asset pricing. Barber and Odean (2008) find evidence of salience-induced buying pressure from retail investors.

[35] Because a stock's performance ranking varies investor by investor, Hartzmark (2015) is able to rule out that this behavior is explained by stock-specific factors. Moreover, he finds a similar rank effect for an economically meaningless ranking based on the alphabetical order of portfolio positions.

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TABLE 9

Trading Propensity: Interaction with Salience

Table 9 reports results of regressions of a manager's trading propensity on the distraction measure (specification (1) in the text) at the stock–manager–week level. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In columns 1–3, the dependent variable is trading propensity, that is, a dummy variable that equals 1 if the manager buys or sells the stock in a given week, and 0 otherwise. In columns 4–6, the dependent variable is buying propensity, that is, a dummy that equals 1 if the manager buys a given stock in a given week, and 0 otherwise. In columns 7–9, the dependent variable is selling propensity, that is, a dummy variable that equals 1 if the manager sells the stock in a given week, and 0 otherwise. In all columns, the distraction measure is interacted with a dummy variable flagging a managers' watchlist stocks with the most extreme (positive or negative) past returns over the previous 4 weeks (EXTREME_RANK), which captures the rank effect documented by Hartzmark (2015). The level effect of the interaction variable is added as an additional control. All other variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable:								
	TRADE_DUMMY			BUY_DUMMY			SELL_DUMMY		
Variable	1	2	3	4	5	6	7	8	9
DISTRACTION	−0.0342***	−0.0352***	−0.0333***	−0.0209***	−0.0202***	−0.0226***	−0.0241**	−0.0232**	−0.0220**
	(−3.28)	(−3.30)	(−3.08)	(−3.36)	(−3.24)	(−3.45)	(−2.38)	(−2.34)	(−2.16)
DISTRACTION × EXTREME_RANK	0.0420***	0.0407***	0.0372***	0.0144**	0.0139**	0.0119*	0.0324***	0.0325***	0.0295***
	(4.75)	(4.61)	(4.36)	(2.22)	(2.10)	(1.86)	(3.83)	(3.75)	(3.60)
EXTREME_RANK	0.0017	0.0019	0.0015	−0.0019	−0.0020	−0.0016	0.0048**	0.0043**	0.0037*
	(0.98)	(1.36)	(1.47)	(−1.41)	(−1.07)	(−1.08)	(2.49)	(2.39)	(1.87)
STOCKS_ON_WATCHLIST	0.0086	0.0157	−0.7300***	0.0042	0.0060	−0.4060***	−0.0001	−0.0034	−0.7182*
	(1.31)	(1.58)	(−3.46)	(1.28)	(1.22)	(−3.15)	(−0.04)	(−0.43)	(−1.85)
TRADE_VOLUME_MANAGER	0.0118***	0.0119***	−0.0107***	0.0049***	0.0050***	−0.0070***	0.0054***	0.0055***	−0.0074***
	(4.43)	(4.44)	(−5.03)	(3.80)	(3.78)	(−5.24)	(4.12)	(4.17)	(−4.91)
TRADE_NUMBER	0.0155***	0.0156***	0.0157***	0.0142***	0.0144***	0.0133***	0.0147***	0.0148***	0.0129***
	(8.71)	(8.94)	(14.17)	(25.13)	(25.72)	(35.50)	(50.24)	(47.06)	(29.13)
AUM		−0.0089			−0.0023			0.0039	
		(−1.38)			(−0.60)			(0.65)	
CHANGE_IN_AUM		−0.0041			0.0053			−0.0055	
		(−0.64)			(1.52)			(−1.24)	
No. of obs.	39,067,464	38,895,699	39,067,464	39,067,464	38,895,699	39,067,464	39,067,464	38,895,699	39,067,464
Adj. R^2	0.48	0.48	0.49	0.49	0.49	0.50	0.38	0.38	0.39
Stock × week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes	No	No	Yes

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sell decisions, which Hartzmark (2015) shows to be more affected by the rank effect.[36] Finally, note that for sell decisions, the level effect of the extreme rank dummy is positive and significant, confirming the existence of the rank effect in my sample.

VIII. Robustness

In this section, I provide robustness checks for my results. I first experiment with alternative variable definitions and control for confounding factors in my baseline regression setting. I then consider a regression setting using a daily frequency. Additional robustness checks and results are provided in the accompanying Supplementary Material. For example, Appendix B of the Supplementary Material shows that consistent results are found when a trade-based watchlist definition is used (instead of the portfolio watchlist used in this article).

A. Robustness of the Baseline Regression Setting

In this subsection, I present robustness checks for the effect of distraction on my 4 variables of interest. Panels A and B of Table 10 provide the results. For brevity, I show only the coefficient on the distraction measure, although I always run the full specification with 13F controls or industry × quarter fixed effects.

In my first robustness check, I calculate the distraction measure as before, except that I now exclude not only the stock in question but all stocks in the same Fama–French 49 industries. The idea is that earnings announcements may represent important economic news events for all stocks in the same industry, and hence distraction would be better defined by looking at earnings announcements among stocks in other industries. As shown in row 1 of Panels A and B of Table 10, distraction continues to have a negative effect on trading propensity (trade dummy) and trade profitability, a positive effect on transaction costs, and an insignificant effect on trading volume.

Second, I address the concern that my results could be driven by institutional capital or risk management constraints. When such constraints are binding, an institution's decision to trade on the earnings announcement of a watchlist stock is certainly interlinked with its decision to trade in other stocks. I argue, however, that in its most straightforward interpretation, such an explanation predicts the opposite of what I find. For example, suppose an institution wants to buy a stock with a positive earnings surprise. If the institution is capital constrained, it may need to sell another position to finance this purchase. But in this case I would expect to find more, not less, trading of other watchlist stocks when there are many earnings announcements. Hence, a more subtle variant of this explanation is needed to explain my results. For instance, one may argue that institutions are reluctant to close existing positions but hold a certain cash balance (or face a limited risk-taking capacity) for entering new trades. Buying the announcing stock then means there is less money (or risk-taking capacity) for buying other stocks.

To control for this possibility, I include the (natural logarithm of the) total amount traded in announcing stocks as a control variable (while excluding all

[36] Compare Table 4 in Hartzmark (2015) with Table IA.25 of its accompanying Internet Appendix.

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TABLE 10
Robustness Checks

Table 10 reports robustness checks for regressions of trading activity and performance measures on the distraction measure (specification (1) in the text) at the stock–manager–week level. Panel A reports results for trading activity and Panel B for performance. In Panel A, the dependent variables are the trade dummy (columns 1–3) and the natural logarithm of trading volume (columns 4–6). In Panel B, the dependent variables are trade profitability (columns 1–3) and relative transaction spread (columns 4–6). Each row represents a different robustness check as indicated in the row header (and explained in Section VII.A). For brevity, the table shows only the coefficient on the distraction measure. EA is earnings announcement. AUM is assets under management. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Trading Activity

	Dependent Variable					
	TRADE_DUMMY			TRADE_VOLUME		
Robustness Check	1	2	3	4	5	6
Exclude same industry	−0.0340***	−0.0338***	−0.0327***	−0.0896	−0.0938	0.0709
	(−3.23)	(−3.20)	(−3.08)	(−0.38)	(−0.40)	(0.36)
Control for trading in EA stocks	−0.0903***	−0.0899***	−0.0781***	−0.3443	−0.3490	−0.2191
	(−6.29)	(−6.24)	(−5.70)	(−0.94)	(−1.06)	(−0.84)
Distraction (equal weighted)	−0.0483**	−0.0485**	−0.0427***	0.1353	0.1262	0.1644
	(−2.24)	(−2.25)	(−2.81)	(0.69)	(0.66)	(0.69)
Distraction (EA surprise)	−0.0120***	−0.0121***	−0.0097**	0.0263	0.0242	0.0859
	(−3.04)	(−3.05)	(−2.32)	(0.33)	(0.30)	(1.15)
Distraction (alternative)	−0.0419***	−0.0416***	−0.0405***	−0.2160	−0.2189	−0.0224
	(−3.75)	(−3.70)	(−3.61)	(−0.76)	(−0.77)	(−0.10)
Past trade controls	Yes	Yes	Yes	Yes	Yes	Yes
AUM and CHANGE_IN_AUM	No	Yes	No	No	Yes	No
Stock × week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes

Panel B. Performance

	Dependent Variable					
	TRADE_PROFITABILITY			TRANSACTION_SPREAD		
Robustness Check	1	2	3	4	5	6
Exclude same industry	−0.0175**	−0.0187**	−0.0215**	0.0012***	0.0010**	0.0008**
	(−2.10)	(−2.20)	(−2.54)	(2.73)	(2.55)	(2.01)
Control for trading in EA stocks	−0.0190**	−0.0201**	−0.0235***	0.0016***	0.0015***	0.0014*
	(−2.11)	(−2.18)	(−2.68)	(3.26)	(3.24)	(1.93)
Distraction (equal weighted)	−0.0198**	−0.0183**	−0.0195**	0.0011*	0.0009	0.0011*
	(−2.24)	(−2.10)	(−2.58)	(1.89)	(1.56)	(1.81)
Distraction (EA surprise)	−0.0063**	−0.0065**	−0.0077**	0.0004***	0.0004***	0.0004**
	(−2.14)	(−2.20)	(−2.48)	(2.72)	(2.65)	(2.20)
Distraction (alternative)	−0.0205**	−0.0215**	−0.0249***	0.0012**	0.0011**	0.0013*
	(−2.24)	(−2.31)	(−2.76)	(2.32)	(2.31)	(1.91)
Past trade controls	Yes	Yes	Yes	Yes	Yes	Yes
AUM and CHANGE_IN_AUM	No	Yes	No	No	Yes	No
Stock × week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes

stock-weeks with an earnings announcement). The idea is that if my results are driven by capital constraints, it should be the actual trades in announcing stocks that matter. In other words, if an institution does not trade on the earnings announcement (so that the capital constraint is unaffected), there is no reason to expect it to trade less in other stocks. Focusing on trading propensity (columns 1–3 in Panel A of Table 10), row 2 shows that both the statistical and economic significance of the measured distraction effect is largely increased by including

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the control (which itself has a positive and significant coefficient, not shown for brevity). This could mean that the true distraction effect is indeed attenuated by the presence of capital/risk management constraints (as conjectured in the previous "straightforward" interpretation). I nevertheless prefer the specification without this control because the decision to trade in announcing stocks is certainly endogenous (and not predetermined as for the other controls). For the other outcome variables, including this control has virtually no effect.

Third, I examine the robustness of my distraction measure. My baseline measure uses watchlist weights to calculate the average; row 3 in Panels A and B of Table 10 show that I get similar albeit slightly weaker results when I just calculate the equal-weighted average. The fact that these results are slightly weaker suggests that earnings announcements for stocks that have a larger portfolio (watchlist) weight receive more attention, and are thus more distracting for other stocks. In any case, both the trading propensity and trade profitability remain significantly reduced, and transaction spreads remain increased.

Fourth, I modify the distraction measure to take into account the earnings announcement surprise. The idea is that a more surprising earnings announcement requires more attention to digest, therefore causing a stronger distraction effect. To test whether this is the case, I follow standard practice and calculate the earnings surprise as the difference between the actual earnings and the median earnings forecast reported in IBES, scaled by the stock's price 5 days before the announcement. I then take the absolute value of the earnings surprise and group the resulting measure into quintiles. Finally, I replace the earnings announcement dummy in my definition of the distraction measure with the absolute earnings surprise quintile:[37]

$$\text{DISTRACTION(EA_SURPRISE)}_{imt} = \frac{\sum_{j \neq i} w^{x}_{jmt} \times |\text{EA_SURPRISE_QUINTILE}|_{jt}}{\sum_{j \neq i} w^{x}_{jmt}}.$$

When the analyses are repeated using this modified distraction measure, I again find a negative and significant distraction effect for trading propensity and trade profitability, as well as an increase in transaction costs (see row 4 in Panels A and B of Table 10). In terms of economic magnitude, a 1-standard-deviation increase in distraction (24%) causes a drop in the propensity to trade by approximately 2.5%, which is comparable to what I find in my baseline case (see Table 2). For the other outcome variables, the economic magnitudes are similar to those reported earlier.

In the final robustness check, I redefine the distraction measure to remedy one unappealing feature of the original definition. To see the issue, suppose a

[37] In order not to lose all of the observations when there is no earnings announcement, I set the absolute earnings surprise quintile to 0 for stock-weeks in which there is no earnings announcement. The absolute earnings surprise quintile thus equals 0 for no announcement and ranges from 1 for the smallest earnings surprises to 5 for the largest earnings surprises.

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manager has 3 watchlist stocks: stock A with a weight of 0.4, stock B with a weight of 0.4, and stock C with a weight of 0.2. Suppose further that stock A has an earnings announcement. With the original definition, the distraction measure is 0 for stock A, ⅔ for stock B, and ½ for stock C. Thus, it appears as if distraction is higher for stock B compared to stock C, although this difference is only due to the respective watchlist weight that is excluded. In other words, the within-manager variation in my original distraction measure is not meaningful.

For this reason, I calculate distraction as:

$$\text{DISTRACTION(alternative)}_{imt} = \sum_{j \neq i} w_{jmt} \times \text{EA_DUMMY}_{jt}.$$

In the earlier example, this definition yields a distraction of 0 for stock A and 0.4 for stocks B and C. Hence, distraction now appears similar for stocks B and C. I repeat my regressions with this new measure after excluding earnings announcements from the sample. Now, distraction varies only across manager-weeks (while being the same for all stocks of the same manager). As shown in row 5 of Panels A and B of Table 10, my results for all 4 outcome variables are qualitatively and quantitatively similar to those reported earlier.

B. Daily Regression Setting

My baseline regression setting is at the weekly frequency. In this subsection, I consider an alternative regression setting at the daily frequency. On the one hand, this setting promises sharper identification; on the other hand, it becomes computationally infeasible to run the regression for the whole sample of watchlist stocks. I therefore focus on the subsample of earnings announcements.

Specifically, I construct a panel data set that joins earnings announcement stocks with all managers that hold them in their portfolios (and thus have them on their watchlists). I then construct all dependent and independent variables at the daily frequency and rerun specification (1). The results are shown in Table 11. As before, I find a strong, significant effect on trading propensity (extensive margin), but no effect on trading volume (intensive margin). In terms of economic magnitude, a 1-standard-deviation increase in distraction leads to a reduction in trading propensity of approximately 4%,[38] which is roughly 20% larger than the corresponding magnitude from the weekly regression (column 1 in Panel A of Table 2). As shown in Panel B of Table 11, I again find that both trade profitability and transaction costs suffer when managers are distracted by other announcements. For example, a 1-standard-deviation increase in distraction decreases trade profitability by approximately 22 basis points, which is 10% larger than the corresponding effect at a weekly frequency. The statistical significance is a bit weaker, however, perhaps owing to the reduced number of observations.

The fact that the results at daily frequency are larger than those at weekly frequency is consistent with the idea that earnings announcements are most attention

[38]The average daily propensity to trade on announcement dates is 4.4%; the standard deviation of the daily distraction measure is 4.1%. Taking the regression coefficient of −0.0433 from column 1 in Panel A of Table 11, there is a 4% drop in the probability to trade.

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TABLE 11
Daily Regression Setting

Table 11 reports results of regressions of trading activity and performance measures on the distraction measure (specification (1) in the text) at the stock–manager–date level for the sample of earnings announcements. Panel A reports the results for trading activity and Panel B for performance. In Panel A, the dependent variables are the trade dummy (columns 1–3) and the natural logarithm of trading volume (columns 4–6). In Panel B, the dependent variables are trade profitability (columns 1–3) and the relative transaction spread (columns 4–6). All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Trading Activity

	Dependent Variable					
	TRADE_DUMMY			TRADE_VOLUME		
Variable	1	2	3	4	5	6
DISTRACTION	−0.0433***	−0.0376**	−0.0316**	−0.0883	−0.0098	−0.0747
	(−2.63)	(−2.41)	(−2.15)	(−0.13)	(−0.01)	(−0.89)
STOCKS_ON_WATCHLIST	−0.0025	−0.0084	−0.0021*	−0.0517	−0.0701	0.0052
	(−0.53)	(−0.95)	(−1.81)	(−1.01)	(−1.19)	(0.10)
TRADE_VOLUME_MANAGER	0.0120***	0.0116**	−0.0024**	0.3178***	0.3148***	0.0144
	(2.62)	(2.53)	(−2.02)	(4.58)	(4.57)	(0.12)
TRADE_NUMBER	0.0002***	0.0002***	0.0001***	0.0002*	0.0002*	0.0002*
	(2.73)	(2.75)	(3.37)	(1.72)	(1.74)	(1.92)
AUM		0.0148			0.1567	
		(1.09)			(0.93)	
CHANGE_IN_AUM		0.0126			0.0506	
		(1.22)			(0.20)	
No. of obs.	2,427,358	2,415,715	2,427,314	92,244	92,096	90,471
Adj. R^2	0.35	0.35	0.40	0.39	0.39	0.41
Stock × date fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes

Panel B. Performance

	Dependent Variable					
	TRADE_PROFITABILITY			TRANSACTION_SPREAD		
Variable	1	2	3	4	5	6
DISTRACTION	−0.0536**	−0.0544*	−0.0497*	0.0027*	0.0026*	0.0028*
	(−2.11)	(−1.78)	(−1.75)	(1.73)	(1.68)	(1.73)
STOCKS_ON_WATCHLIST	0.0011	0.0012	−0.0024	0.0000	0.0000	0.0001
	(0.45)	(0.50)	(−1.13)	(0.38)	(0.11)	(1.26)
TRADE_VOLUME_MANAGER	−0.0011	−0.0012	−0.0044	0.0001***	0.0001***	0.0001
	(−1.52)	(−1.56)	(−1.14)	(4.35)	(4.52)	(0.72)
TRADE_NUMBER	−0.0000	−0.0000	−0.0000	0.0000	0.0000	0.0000
	(−1.48)	(−1.46)	(−1.42)	(0.82)	(0.77)	(0.85)
AUM		0.0002			0.0002*	
		(0.05)			(1.91)	
CHANGE_IN_AUM		−0.0308**			0.0006	
		(−2.16)			(1.59)	
No. of obs.	92,107	91,959	90,338	92,244	92,096	90,471
Adj. R^2	0.05	0.05	0.07	0.08	0.08	0.10
Stock × date fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Manager × quarter fixed effects	No	No	Yes	No	No	Yes

absorbing (and thus most distracting) on the day of the announcement. At the same time, the increase in magnitude is modest for at least two reasons. First, as documented in Table 7, the distraction effect is mitigated when the stock in question has an earnings announcement itself, and this is the case for my daily subsample. Second, the distraction effect is not confined to the announcement

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date. In fact, I show in Appendix C.5 of the Supplementary Material that the distraction effect for trading propensity starts on the day of the announcements and persists until up to 4 days after the announcement.

IX. Conclusion

Exploring detailed transaction records, I investigate and quantify attention constraints among professional asset managers. These investors employ significant resources to overcome attention constraints: They hire research staff, acquire access to real-time news feeds, and invest in computer capacities for algorithmic trading or smart order routing. I find that despite all of these efforts, attention constraints occasionally appear to be binding. Specifically, I find that managers with a large fraction of watchlist stocks that have an earnings announcement are significantly less likely to trade in other stocks, but conditional on trading, they do not trade in smaller amounts. I further document that distracted managers trade less profitably and incur higher transaction costs, although both of these effects are economically modest.

Next, I analyze how institutional investors cope with their attention constraints. My findings suggest that attention allocation decisions have multiple facets. On the one hand, I find that investors are less distracted from trading in stocks with an imminent earnings announcement or stocks in which they have a large portfolio stake, consistent with them understanding that it is in their interest to remain attentive to these stocks. On the other hand, I find that the distraction effect is weaker (stronger) for watchlist stocks that trade at a gain (loss), suggesting that distracted managers redirect their attention in a way that makes them feel better. Finally, I show that the distraction effect is mediated by a stock's salience.

My findings have important implications for the development of a positive theory of attention choice, which to the best of my knowledge does not yet exist. Overall, they appear to be most consistent with a model in which investors face a fixed search cost for finding profitable trading opportunities and in which attention governs the quality of trade execution. They are less consistent, however, with the common approach of modeling attention as a choice of signal precision, at least as long as one insists that signal precision also affects the choice of position sizes. In terms of attention allocation across stocks, such a model should combine both rational and behavioral elements. Indeed, my results suggest that investors possess an intuitive understanding for which stocks attention is most valuable, but they also suggest that attention allocation decisions are influenced by subconscious and/or psychological factors such as salience and emotions toward gains and losses. I hope that these results can guide future theoretical work in this area.

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Appendix. Variable Definitions

This Appendix provides definitions of the variables used in this article. All continuous variables are winsorized at the 1% threshold on both sides. IBES is Institutional Brokers' Estimate System. CRSP is Center for Research in Security Prices.

Variable Name	Definition	Level	Source
DISTRACTION	Weighted fraction of a manager's watchlist stocks that have an earnings announcement, where the weights correspond to the portfolio weight of stocks held at the end of the previous quarter. (For the trade-based watchlist, for which results are reported in the Supplementary Material, the weight corresponds to the fraction of the manager's trading volume in the stock over the past 12 weeks.)	Manager–week–stock	Ancerno, 13F, IBES
STOCKS_ON_WATCHLIST	Logarithm of the number of stocks the manager held at the end of the previous quarter. (For the trade-based watchlist, this is the number of stocks the manager traded in the past 12 weeks.)	Manager–week	Ancerno, 13F
TRADE_VOLUME_MANAGER	Natural logarithm of the dollar trading volume of the manager in the preceding 12 weeks.	Manager–week	Ancerno
TRADE_NUMBER	Number of days in the last 12 weeks the manager traded the stock.	Manager–week–stock	Ancerno
AUM	Natural logarithm of the dollar amount of assets under management at the end of the previous quarter.	Manager–week	13F
CHANGE_IN_AUM	Relative change in assets under management from the beginning to the end of the previous quarter.	Manager–week	13F
TRADE_DUMMY	Dummy variable equal to 1 if the manager traded the stock in that week.	Manager–week–stock	Ancerno
BUY_DUMMY	Dummy variable equal to 1 if the manager bought the stock in that week.	Manager–week–stock	Ancerno
SELL_DUMMY	Dummy variable equal to 1 if the manager sold the stock in that week.	Manager–week–stock	Ancerno
TRADE_VOLUME	Natural logarithm of the dollar trading volume of the manager in the stock in that week.	Manager–week–stock	Ancerno
BUY_VOLUME	Natural logarithm of the dollar buy volume of the manager in the stock in that week.	Manager–week–stock	Ancerno
SELL_VOLUME	Natural logarithm of the dollar sell volume of the manager in the stock in that week.	Manager–week–stock	Ancerno
TRADE_PROFITABILITY	Compounded stock return over the subsequent 4 weeks for buys; compounded stock return times −1 over the subsequent 4 weeks for sells.	Manager–week–stock	Ancerno, CRSP
TRANSACTION_SPREAD	Volume-weighted average relative transaction spread, defined as the difference between the execution price and the previous day's closing price for buys (and vice versa for sells), scaled by the previous day's closing price.	Manager–week–stock	Ancerno, CRSP

Supplementary Material

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