Distracted Institutional Investors

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ABSTRACT

We investigate how distraction affects the trading behavior of professional asset managers. Exploring detailed transaction-level data, we show that managers with a large fraction of portfolio stocks exhibiting an earnings announcement are significantly less likely to trade in *other* stocks, suggesting that these announcements absorb attention which is missing for the choice of which stocks to trade. This distraction effect is more pronounced for non-passive managers that engage in active stock selection choices. Finally, we 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.

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Attention is scarce. Yet, we know very little 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 paper, we propose a way to address this empirical challenge and—in doing so—uncover *well-identified* evidence suggesting that attention constraints can be binding even for professional asset managers. Specifically, exploiting detailed transaction-level data for a large sample of U.S. institutional investors, we are able to identify attention shifts between different stocks that are on the "radar screen" of a particular investor. Exploring the ramifications of such attention shifts, we shed light on a number of important questions: How severe are attention constraints among professional investors? Do investors cope with them in a rational manner? And, finally, what are the channel through which attention constraints manifest themselves in investors' trading activity and performance?

Our identification builds on the premise that an investor cannot pay equal attention to all stocks. He will thus have to focus on a subset or "watchlist" of stocks. To see the idea, 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

[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.

about stock C. Under limited attention, we expect investor 1 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 the news of stock B, which distracts him from trading in stock A. In another period, stock C may have important news and we would then expect investor 2 to be distracted relative to investor 1. By comparing the trading of investors 1 and 2 in the same stock, our identification exploits such attention redirections at the *investor-stock-time level*.

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

Our institutional transaction data comes from ANcerno Ltd, a consulting firm that helps institutional investors to monitor their trading costs. Prior research finds that ANcerno trades represent approximately 10% of all institutional trading volume in the U.S. and that they are not significantly different from trades made by the average U.S. institutional investor (Puckett and Yan, 2011; Anand et al., 2012). The key feature of this data is that, in addition to detailed trading records, it provides a unique identifier for the trading institutions. This enables us to implement our identification strategy at the level of the institutional investor.[2]

In our baseline approach, we use investors' portfolio holdings reported on form 13f to define what we call the "*portfolio* watchlist": the set of stocks that the investor held at the end of the previous quarter. We verify that portfolio watchlists highly predict future trading, and much better so than randomly-assigned placebo watchlists, which confirms that investors' pay more attention to their watchlist stocks. As a robustness check, we also consider a trade-based watchlist, which is simply defined as the set of stocks that the investor traded in the previous 12 weeks. The results for this alternative watchlist definition, reported in Internet Appendix B, offer an important consistency check for our approach.

We 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; Kim and Verrecchia, 1994). Institutional investors have the 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 position (e.g., Bushee et al, 2011). All this requires attention[3]—attention that we argue is missing for trading in other stocks. Our

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[2] Ideally, we would want to conduct our analysis at the fund-level. Unfortunately, the ANcerno data does not provide a unique fund identifier, and we are thus forced to work at the level of the institution. To the degree that attention constraints really operate at the fund-level, our distraction measures contain measurement error which could lead to an attenuation bias. Hence, the distraction effects documented in this paper can be understood as a lower bound estimate of the real attention constraints faced by institutional investors.

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

primary distraction proxy is thus the (portfolio-weighted) fraction of stocks on the investor's watchlist that exhibit an earnings announcement in a given period.[4] Importantly, when we construct the distraction measure for a given stock and investor, we calculate this fraction by summing over all *other* stocks on the investor's watchlist. Thus, our measure captures distraction coming from other stocks on the watchlist.

Our 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 about 3.5%. For the subset of managers that follow active investment strategies; i.e., those that 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×time and stock×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, we find no distraction effect at the intensive margin. That is, conditional on trading in a given stock, institutional investors do not trade less when there are many earnings announcements for watchlist stocks. 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; Kacperczyk et al., 2016). Instead, our results suggest

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[4] We obtain consistent results when distraction is defined as the equal-weighted fraction of portfolio stocks with an earnings announcements, or when we replace the earnings announcement dummy by earnings surprise quintiles; see the robustness section below.

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 et al., 2007; Chien et al., 2012).

We then conduct a number of sample splits to shed light on which type of managers are more distracted. We find that the distraction effect is stronger for managers that trade actively, where activeness is proxied by the intensity of rebalancing trades as opposed to flow-induced trades. Since the former involve a stock selection choice, whereas the latter amount to a mechanical rescaling of existing positions, we expect rebalancing trades to be more susceptible to distraction and this is what we find. We further show that our 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 a 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, we identify two channels through which distraction affects managers' performance. First, distraction leads managers to make poorer trading decisions. Compared to non-distracted managers trading in the same stock, distracted ones have past-trade returns over the next four weeks that are 20 basis points lower (40 basis points in the subsample of quasi-indexers). Second, we find that distracted managers incur slightly higher transaction costs on their trades.[5] We acknowledge, however, that the magnitudes 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.

Finally, we conduct an in-depth analysis of the channels that appear to explain observed attention choices. We start with testing an important corollary of the rational attention paradigm. Specifically, we test whether the distraction effect is reduced for stocks that matter more to an investor. Consistent with this intuition, we find that investors remain relatively more attentive to stocks in which they have a large portfolio stake or stocks with an immanent earnings announcement. Probing for other (i.e., behavioral) factors that mediate the distraction effect, we uncover two additional findings. First, the distraction effect on trade and in particular sell decisions is pronounced for stocks that trade at a loss. We argue that this finding is consistent with recent literature suggesting that information can have a direct effect on investors' utility (Karlsson et al., 2009; Sicherman et al., 2015), prompting them to avoid unpleasant information when they have an excuse in the form of a distracting event. We also find suggestive evidence that this behavior exacerbates the disposition effect (Shefrin and Statman, 1985)—i.e., the well-documented tendency of investors to hold on to losing stocks while selling winning ones. Second, we document that salience, which is found to strongly attract attention (Barber and Odean, 2008; Bordalo et al., 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 a stock's salience and emotions toward gains and losses.

Our paper contributes to the literature on inattention in financial markets (see, for instance, Cohen and Frazzini, 2008, DellaVigna and Pollet, 2009, and Hirshleifer et al., 2009). While this literature has been burgeoning, there are only few papers that specifically focus on professional investors—presumably because these investors are assumed to be less attention constrained to begin with. Fang et al. (2014) show that certain mutual funds persistently buy into stocks that have been 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 et al. (2016) collect a sample of marriage and divorce events for hedge fund managers and find that their performance suffers during those events. Ben-Rephael et al. (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 et al. (2017) explore a similar identification approach to ours, 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, our paper improves on the identification and allows studying the *exact channel* of how inattention affects managers' trading behavior and performance.

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

I. Hypotheses

A. Distraction, Trading Activity, and Performance

In our empirical analysis, we focus on four outcome variables that are well-suited in the context of our identification strategy:[6] (1) investors' trading propensity (i.e., their decision to trade or

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

not), (2) trading volume (i.e., how much they trade conditional on trading), (3) trade profitability and (4) incurred transaction costs. In this section, we draw on existing literature in order to lay out the empirical predictions pertaining to these outcome variables.

Trading propensity—measured by a *trade dummy* taking the value one 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; or 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 ones for sell decisions (Barber and Odean, 2008). It is not clear, however, how short-sale constrained the institutional investors in our sample are.[7] Hence, we define dummy variables that separately flag buy and sell decisions.

Trading volume—defined as the 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; 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 precisions to trade sizes is relevant in practice. Indeed, position sizes are often determined by an asset's historical volatility (e.g., through a

[7] According to Jame (2017), the ANcerno data contains short-sales, but it is not possible to distinguish them from other sales.

position limit imposed by a risk constraint; see Pedersen, 2015) rather than by the perceived precision of a recently obtained trading signal.

Trade profitability—defined as the *post-trade stock return*, multiplied by minus one 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 at least less subject to distraction, perhaps because these trades are required to accommodate in- or outflows into the fund), then the average trade profitability goes down when a distracted investor stops searching for profitable trading opportunities.[8]

Transaction costs—measured by the incurred *relative transaction spread*: Although rarely modelled explicitly, it is intuitive to think that attention can matter for transaction costs, which is itself a key performance driver 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]

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

[9] Dugast (2017) presents a model of limit order trading under with infrequent monitoring due to limited attention. Moreover, we believe that such an intuition can arise naturally in models of endogenous limit order trading as in Handa and Schwartz (1996) and Goettler et al. (2005, 2009).

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

We now discuss additional *cross-sectional predictions* about when and where we expect distraction effects 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, they 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)—e.g., stocks in which they have a large stake or for which they expect the release of important value-relevant information. In our empirical analysis, we study how distraction effects interact with a stock's watchlist weight and a dummy whether there is an immanent earnings announcement.

While there is no widely-accepted alternative to the rational attention paradigm, the literature has documented other factors that help understand observed attention allocation behavior. Here, we discuss two such factors that yield empirical predictions which contrast with those from rational attention models.

The first factor we consider 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, over and above its indirect effect through their choices (Caplin and Leahy, 2001; Brunnermeier and Parker, 2005; for a survey see Golman et al., 2017). A natural implication of such models is that agents may choose to selectively avoid information that they expect to be bad.[10] We posit that attention-constrained investors may be particularly prone to engage in such behavior: when choosing which portfolio stock not to pay attention to, distracted investors may choose the one that is trading at a loss in order to avoid the disutility associated with it. 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 our empirical analysis, we study how distraction effects interact with a stock's past return. Based on the previous discussion, we expect a stronger distraction effect for portfolio stocks trading at a loss—especially for sell decisions. This prediction also suggests a connection between inattention and the disposition effect; i.e., the well-known tendency of investors to prefer selling positions trading at a gain compared to those trading at a loss (Shefrin and Statman, 1985).[11] Indeed, if distraction causes investors to focus on their winning positions at the expense of their losing ones, we also expect distraction to exacerbate the disposition effect.

[10] Studying the online login behavior for a sample of 401k retirement plans, Karlsson et al. (2009) and Sicherman et al. (2016) 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.

[11] While this behavior is most strongly observed for individual investors (Odean, 1998; Grinblatt and Keloharju, 2001), Grinblatt and Han (2005) and Frazzini (2006) find evidence that it also applies to institutional investors.

We test this prediction using a standard measure of an (institutional) investor's propensity to succumb to the disposition effect (Odean, 1998).

The second behavioral factor we consider 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 of the control of the investor. As such, we expect the distraction effect to be mitigated for salient stocks. We test this prediction using the extreme rank measure proposed by Hartzmark (2015).

II. Data

A. Institutional Trading Data

We obtain institutional trading data from ANcerno Ltd (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% of institutional trading

[12] Bordalo et al. (2014, 2015) write down consumer choice models in which economic agents overweigh salient attributes of a product.

[13] Previous papers using this data include Goldstein et al. (2009), Chemmanur et al. (2009), Puckett and Yan (2011), Anand et al. (2012), Ben-Rephael and Israelson (2014), Hu et al. (2014), Franzoni and Plazzi (2015), Goetzmann et al. (2015), Jame (2017), Chakrabarty et al. (2017), and Eisele et al. (2017).

volume in U.S. equities. While 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. Our sample period starts in January 1999 and ends in June 2011, after which ANcerno stopped the provision of an identifier for the trading institution.

Each row in the ANcerno dataset 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 our purpose is that it contains a unique identifier corresponding to the management company executing the trade (*manager code*). We also have access to a reference file that links manager codes to the names of those companies. Ideally, we would want to have identification at the fund-level; however, the ANcerno data does not provide this information.[14] Hence, we are forced to conduct our analysis at the manager-level. We have 835 different managers in our sample.

In order to gauge the performance of institutional trades, we map stock returns from CRSP onto the ANcerno trades.[15] Since we conduct our main analyses at the manager-stock-time level, working at daily frequency becomes computationally infeasible. We therefore aggregate trades at weekly frequency.

B. Link to 13F

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[14] ANcerno contains an additional variable called *clientmgrcode*. However, interactions with ANcerno as well as our reading of the literature convince us that this is not a unique fund identifier. For instance, Jame (2017) writes (see footnote 4): "discussions with ANcerno representatives indicate that different *Clientmgrcodes* within a client-manager generally do not reflect different fund products."

[15] See Internet Appendix A.1 for details.

Using the manager names available to us, we hand-match ANcerno managers to institutional holdings data reported in 13f.[16] We are able to find corresponding 13f information for 670 out of the 835 managers in our sample. This match serves several purposes. First, as detailed below, we exploit holdings data in 13f to assemble the list of stocks held by each manager at the end of the previous quarter. Second, we use this match to obtain a link to static investor characteristics reported on Brian Bushee's website. We are particularly interested in the classification of managers into "quasi-indexers" and others,[17] as we expect distraction effects to be weaker for managers following passive investment strategies. Third, it allows us to construct important control variables such as the level and change of managers' assets under management.

C. Watchlist Construction

Our identification rests on the intuitive assumption that investors pay more attention to portfolio stocks than to others. 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, we construct a so-called *portfolio watchlist*. A given stock i enters this watchlist if manager m reported a positive holding in the stock at the end of the quarter prior to week t. Let w_{imt} be the portfolio weight of stock i, defined as:

[16] See Internet Appendix A.2 for details.

[17] More precisely, Bushee and Noe (2001) and Bushee (2002) classify managers into three categories: quasi-indexers, transient and dedicated investors. The latter two categories differ mainly in their trading activity. Since results for these two categories are similar and since we have no expectation as to which group should be more affected, we merge them in our analysis. The investor classification data is available at: http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html.

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

As explained below, we use this portfolio watchlist and the corresponding portfolio weights to set-up our baseline regression design.

In Internet Appendix B, we report results for an alternative watchlist definition that reflects past trading. Specifically, we 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 which they did not trade recently (in which case the stock is only in the portfolio watchlist), and managers quickly trade in and out of a stock (in which case the stock only enters the trade-based watchlist). Our analysis based on the trade-based watchlist therefore provides an important consistency check for the results reported in this paper.

D. Sanity Check

If our portfolio watchlists capture stocks that managers are paying attention to, we expect those stocks to be traded with higher probability compared to a random sample of stocks. To test this prediction, we construct randomly-assigned placebo watchlists in the following way. First, we randomly reshuffle the holdings data, while maintaining differences in holding intensities across managers and stocks.[18] Second, we use the reshuffled data to construct a new "placebo" watchlist. We then compare the fraction of watchlist stocks that are traded in the

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

subsequent week across portfolio and placebo watchlists. Table 1 Panel B shows that the average fraction of traded watchlist stocks is about four-times larger for the portfolio watchlist compared to the placebo one—a difference that is strongly statistically significant.[19] This suggests that institutional investors are indeed paying close attention to their portfolio stocks.

E. Earnings Announcement Dates

We study how news events in some portfolio stocks affect trading in other portfolio stocks. To proxy for news events, we use earnings announcement dates from I/B/E/S and Compustat. Earnings announcements constitute the most important recurring news releases for individual firms;[20] they receive significant media attention and institutional investors routinely attend earnings conference calls. As such, they are well suited for our analysis.

Following DellaVigna and Pollet (2009), we use the earlier of the dates in I/B/E/S and Compustat when the two dates do not coincide for the same fiscal quarter. We drop the earnings announcement when the firm had another announcement less than 11 days earlier. We define an earnings announcement dummy, $EA\ dummy_{it}$, that takes the value of one if firm i had an earnings announcement in week t and zero otherwise.[21] Overall, we have 274,840 earnings announcement weeks, representing roughly 8% of all stock-week observations in our sample period.

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

[20] See, e.g., Beaver (1968), Aharony and Swary (1980), Bernard and Thomas (1989), and Kim and Verreccia (1994).

[21] Earnings announcements on a Friday are treated slightly differently. As we don't have the exact time of the announcement, we are not sure whether the earnings news is priced in on Friday or on Monday of the following week. For this reason, $EA\ dummy$ is set to one for both weeks t and $t+1$ when the announcement occurred on a Friday.

III. Methodology

A. Distraction Measure

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

Our 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, our distraction measure is the weighted fraction of watchlist stocks with an earnings announcement,

$$distraction_{imt} = \frac{\sum_{j \neq i} w_{jmt} \times EA\ dummy_{jt}}{\sum_{j \neq i} w_{jmt}}.$$

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 our 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.[22] 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

[22] Our results for the sanity check reported above are consistent with this intuition, since we 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).

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, we 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.[23]

Table 1 Panel A shows descriptive statistics for our 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 an amount of 764,500$. For the average manager, roughly 8.1% of watchlists stocks exhibit an earnings announcement in a given week. The standard deviation of this measure exceeds 10%, ensuring that we have sufficient variation in distraction. We also see that the median manager has 19$ billion of assets under management and trades a total volume of 156$ million over the course of 12 weeks.

[Include Table 1 here.]

B. Regression Methodology

Our main regression specification is

$$y_{imt} = \alpha_{it} + \alpha_{im} + \alpha_{mq(t)} + \beta\ distraction_{imt} + \gamma\ trade\ number_{imt} \qquad (1)$$
$$+\delta\ managercontrols_{mt} + \varepsilon_{imt}$$

where y_{imt} is one of the four outcome variables introduced above. In principle, each manager could trade every available stock, resulting in an enormous data matrix of possible trades. Working with such a dataset is neither feasible nor desirable (because there would be zero

[23] In Subsection VII.A below, we show that our results are robust to calculating distraction as the equal-weighted average of the announcement dummy.

trading for a vast majority of observations). We therefore estimate specification (1) only on the subset of portfolio watchlist stocks for each manager.

One crucial feature of our empirical setting is the three-dimensional data structure, which enables us to soak up a great deal of the cross-variation in trading activity through the inclusion of various fixed effects. For example, in any week, certain stocks happen to 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 as being relatively more active, which would confound our identification. Next, consider the stock-manager dimension. Different managers choose to trade different stocks for reasons which are largely unobserved. To the extent that such predispositions correlate with our 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 us against these and related concerns.

As our identification draws on the comparison across managers with different levels of distraction at a given point in time, we cannot include fund×date fixed effects in our specification.[24] We can, however, control for slow-moving manager characteristics with the inclusion of manager×quarter ($\alpha_{mq(t)}$) fixed effects.

[24] 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 our distraction measure is not meaningful in our setting. See Subsection VII.A for an illustration of this point.

In addition to these high-dimensional fixed effects, we include a number of control variables. First, because trading is relatively sticky, we 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, we include several proxies of manager size: the logarithm of the manager's dollar trading volume in the past 12 weeks and the level and change of 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 once we include manager×quarter fixed effects. Standard errors are clustered at the manager level.

IV. Distraction and Trading Activity

A. Baseline Results

In this section, we examine how distraction affects trading activity. Table 2 Panel A shows the 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)—and reveals a pervasive distraction effect. In terms of magnitude, we find that a one-standard deviation increase in our distraction measure reduces the probability to trade by 3.3% relative to its unconditional mean. While 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 which are therefore unlikely to be affected by distraction. Thus, the effect is economically meaningful. In Internet Appendix C.1, we show that the distraction effect does not revert in subsequent weeks. To the contrary, we find that the tendency to trade fewer stocks persists (with decaying magnitude) for up to two weeks before turning insignificant. Hence, managers do not catch up on missed trades once the distraction subsides.

[Include Table 2 here.]

Peress and Schmidt (2018) find that distracted retail investors buy but do not sell fewer distinct stocks. In contrast, we find a symmetric effect for the buy and sell decisions of institutional investors (columns 4-9). 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 only choose among the handful of portfolio stocks, whereas an institutional investor faces a much larger choice set. Since a complex choice is more susceptible to distraction, this explains why there is a significant distraction effect for institutional sells but not for retail ones.

In Table 2 Panel B, we study the impact of distraction on the intensive margin of trade; i.e., the decision of how much to buy or sell conditional on trading. Rational attention models linking attention choices with signal precisions invariably predict that paying less attention leads investors to trade less aggressively. In contrast to this prediction, we find no evidence that distracted managers curb back the dollar volume of their trades (conditional on trading). Based on the estimated standard error, we can 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 will be small. The failure to find an effect at the intensive margin highlights a discrepancy between theory and practice. While it is reasonable to think that more attention results in better information, trade sizes in practice seem 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 of which stocks to trade that requires the most attention—and which is thus most affected by distraction. Hence, they are most consistent with models that feature a *fixed search cost* for deciding which stock to trade (as in Merton, 1987).

B. Quasi-Indexers

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

[Include Table 3 here.]

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

[Include Table 4 here.]

Our first two sample splits are meant to reinforce the point that active management requires more attention and thus suffers more from inattention. First, we 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 that only trade to invest/divest as a function of fund inflows/outflows are likely to score low on this measure and hence we expect them to be less distracted. Row 1 in Table 4 confirms this expectation: distraction is strongest for the managers with high turnover, whereas low-turnover managers do not appear to be distracted. This difference is statistically significant.

Second, we attempt to separate between rebalancing and flow-induced trades. The idea is that rebalancing trades involve stock selection and are thus prone to distraction. Instead, flow-induced trades lead to a mechanic rescaling of existing positions. To capture the degree of flow induced vs. rebalancing trades, we calculate, for each week, the minimum of a manager's dollar buys and dollar sells, divided by his total trading volume.[25] We then average across weeks and call this measure *trade activeness*. Managers that score high on this measure buy and sell a lot

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

at the same time, thereby rebalancing their portfolios from one stock to another. Managers that 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. We then run our analysis separately for managers in the bottom, middle and top tercile in terms of this trade activeness measure. Row 2 shows that, as expected, we find the strongest distraction effect for managers with high trade activeness; i.e., those managers that make active rebalancing decisions on a regular basis. For the top group, a one-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.

Our 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 we expect to find a weaker distraction effect for this group. We therefore sort managers into terciles based on the average Herfindahl index of their watchlist stocks across the Fama-French 49 industries. Row 3 shows that, consistent with our expectation, the distraction effect is larger for the group of managers with low industry concentration (although the difference to the high-concentration group is not significant).

Forth, we split managers by average assets under management over the sample period.[26] Row 4 in Table 4 shows that only small and medium-sized managers are significantly distracted. At first sight, 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 one-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 ones. After all, large institutions presumably comprise more different funds, which should attenuate our 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 will be less confounded by the information effect of earnings news (see above). Second, even when an institution has many funds, some trading decisions may yet be taken at or depend on input from the institutional level (for example, 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 group-wide risk management division).

The argument that less focused institutions are more prone to distraction also explains the results for our fifth sample split, where we find a stronger distraction effect for managers with

[26] For this sample split, we 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 our full model with the inclusion of stock×week and manager×stock fixed effects is poorly identified. Instead, to balance the number of observations in the different size groups, we 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.

a large number of watchlist stocks (row 5). Sixth, we sort managers by average trading profits, measured as the mean (watchlist-weighted) return of the watchlist portfolio over a 48 weeks 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, we check whether distraction is stronger or weaker for the 75 hedge funds in our sample.[27] We have no particular 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 distracted. Our results, shown in Table 4 row 7, appear more consistent with the second interpretation, as the distraction effect for hedge funds is not statistically significant. We 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 the one for the other institutions.

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

V. Distraction and Performance

[27] We thank Russell Jame for providing the hedge fund identifiers (described in Jame, 2017).

In this section, we study whether distraction affects managers' performance. We 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, we repeat our panel regression from specification (1) with the post-trade return as the dependent variable. For buys, the post-trade return is simply the stock return over the subsequent 4 weeks. For sells, it is the stock return over 4 weeks times minus one.[28] 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 further that, due to the inclusion of stock×time fixed effects in our regression specification, we essentially test whether distracted managers are more or less likely to be on the "winning side of a trade." In other words, we test whether distracted managers, compared to less distracted ones, are more likely to buy (sell) stocks that subsequently underperform (outperform).[29] 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.

We calculate the trade profitability over 4 weeks as the one-month horizon is commonly used in research on investors' performance (e.g., Daniel et al., 1997; Coval and Moskowitz, 2001) and is further in line with previous analyses on the performance of ANcerno institutions (Puckett

[28] 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).

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

and Yan, 2011; Chakrabarty et al., 2017; Jame 2017). Underlying this choice is the assumption that information advantages by institutional investors should be fleeting and may thus only be detectable for post-trade returns in the short term.[30, 31]

Table 5 Panel A shows the results for the overall sample. We find a statistically significant distraction effect on trade profitability: a one-standard deviation increase in distraction decreases the average post-trade profitability over the subsequent four weeks from roughly 0.1% to -0.1%. Thus, while the institutional managers in our 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 the group of non-indexers—with a magnitude that is twice as large as the one 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.

[Include Table 5 here.]

B. Transaction Costs

We now examine whether differences in distraction affect order execution quality. To this end, we regress a measure of managers' incurred transaction costs on distraction in our usual panel

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

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

regression framework (specification (1)). Given the inclusion of stock×date and stock×manager fixed effects, we are essentially testing whether distracted managers trade at worse prices compared to non-distracted ones.

Our proxy for order execution quality is the average relative transaction spread, defined as the difference between the transaction price and the previous day closing price for buys (and the same difference times minus one for sells), scaled by the previous day closing price. We 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, we find support for the notion that distraction hurts execution quality: the effect of distraction is significantly positive but economically small (Panel A). A one-standard deviation increase in distraction leads to a rise in transaction spread of about 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 suggests that, while a lack of attention does not affect the trade profitability for quasi-indexers, it can affect their trade execution quality.

[Include Table 6 here.]

Taken together, our results show that distraction hurts performance: distracted managers trade less profitably and incur slightly higher transaction costs. While 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 Internet Appendix C.3, we find results that are consistent with this idea, although the evidence is marginally significant at best (presumably because our proxies for the extent of order fragmentation are too noisy).

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

In Table 7, we return to the trade propensity regressions and interact our distraction measure with two 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 will 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.[32]

[Include Table 7 here.]

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

In our second test, we interact the distraction measure with an earnings announcement dummy, which flags weeks in which the stock has an earnings announcement. Since these announcements can be anticipated, attention-constrained investors can rationally choose to remain attentive to them, in which case we expect the distraction effect on the trading propensity to be reduced. The results, shown in columns 4-6, are again consistent with this expectation as the interaction coefficient is positive significant. Hence, managers appear to be less distracted for stocks with an immanent earnings announcement compared to other stocks.[33]

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, we explore other factors that may influence their attention choices.

B. Exacerbating Behavioral Biases?

While 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 Karlsson et al., 2009; and Sicherman et al., 2016, for empirical evidence using data on online account logins). At the heart of these models lies the assumption that

[33] In unreported analyses, we 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., 2016). We explore this possibility in detail below.

information can have a direct impact on utility ("hedonic value") that is separate from its usefulness. In similar spirit, models with *realization utility* assume investors derive additional utility from realizing gains and losses over and above the utility derived from paper gain/losses (Barberis and Xiong, 2012; Ingersoll and Jin, 2013).[34]

Motivated by these theories, we study how the distraction effect for trading propensity interacts with the past return 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 in order to avoid facing the negative emotions associated with acknowledging a trading loss. Table 8 shows the results of this exercise. As indicated by the positive albeit weakly significant interaction coefficient shown in columns 1-2, the distraction effect is somewhat less pronounced for stock positions that trade at a gain. The flip side of this is that, consistent with an ostrich-type behavior, distracted investors are particularly unlikely to trade in stocks that have had low returns.

[Include Table 8 here.]

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

[34] Using measures of neural activity obtained from functional magnetic resonance imaging (fMRI), Frydman et al. (2014) find evidence consistent with such type of preferences.

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 we find a stronger distraction effect for sells following low returns hints at a connection to the disposition effect—i.e., 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). We explore this connection below.

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; 2015), 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.[35] 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.[36]

In this subsection, we explore how the distraction effect interacts with the salience of a stock position, which we proxy for using an *extreme rank dummy* as proposed by Hartzmark (2015). Specifically, we define the *extreme rank dummy* to be equal to one if the return of a watchlist

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[35] Bordelo 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.

[36] 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 also finds a similar rank effect for an economically meaningless ranking based on the alphabetical order of portfolio positions.

stock was the lowest or highest over the previous four weeks compared to all other watchlist stocks for a given investor. We then interact this variable with our distraction measure in the baseline specification. Since salient stocks subconsciously catch investors' attention (i.e., without involving a distraction-prone choice on the part of the investor), we expect the distraction effect to be mitigated for salient stock positions.

Our results, shown in Table 9, confirm this intuition: the distraction effect is almost fully reversed for stock positions that rank at the bottom or at the 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 at times when attention is scarce. This result is more pronounced for sell decisions, which Hartzmark (2015) shows to be more affected by the rank effect.[37] Finally, note that for sell decisions the level effect of the extreme rank dummy is significantly positive, confirming the existence of the rank effect in our sample.

[Include Table 9 here.]

D. Distraction and the Disposition Effect

The disposition effect describes the well-documented preference for selling stock positions trading at a gain compared to the ones trading at a loss.[38] In this subsection, we investigate the possibility that inattention can exacerbate the disposition effect. The literature suggests two reasons why this might be the case: First, there is evidence that investors can learn to avoid the

[37] Compare Table 4 in Hartzmark (2015) with Table IA.25 of the accompanying internet appendix.

[38] For individual investors, see Shefrin and Statman (1985), Odean (1998) and Grinblatt and Keloharju (2001). For institutional investors and asset pricing implications, see Grinblatt and Han (2005) and Frazzini (2006).

disposition effect with conscious effort and experience (Feng and Seasholes, 2005; Seru et al., 2010). In similar spirit, we argue that investors may require attention/mental effort in order to avoid succumbing to the disposition effect. Second, under realization utility preferences, a distracted investor may choose to forgo selling a loser (so as to further delay its realization) while still selling his winners. The evidence presented in Table 8 was consistent with this view.

Here, we directly measure the extent of the disposition effect at the manager-week level and explore how it correlates with our distraction proxy. Following Odean (1998), the disposition effect measure is calculated as the proportion of gains realized (PGR) minus the proportion of losses (PLR) realized. Specifically, for each manager, we keep track of the average purchase price for all open stock positions (based on that manager's trading history). Then, for each week in which the manager sells at least one stock, we calculate the PGR (PLR) as the number of positions sold at a gain (loss) over the total number of positions that could have been sold at a gain (loss) in that week and take the difference. Unconditionally, we find a statistically significant disposition effect of 1.18 percentage points (t-statistic of 3.5). To ensure that there is no mechanical relation between the dependent and the independent variable, we exclude earnings announcement stocks before calculating the disposition effect measure.

Table 10 shows the results from regressing the disposition effect measure on our distraction proxy aggregated at the manager-week level.[39] Manager and time fixed effects are included to soak up all time-invariant variation (controlling for, e.g., return seasonalities coinciding with the earnings season) and manager-invariant variation (controlling for, e.g., a manager-specific

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[39] The aggregated distraction measure simply equals the (weighted) fraction of watchlist stocks that have an earnings announcement in a given week.

predisposition to succumb to the disposition effect). We also control for managers' watchlist sizes and past trading volumes.

[Include Table 10 here.]

As shown in Table 10, column 1, we find a significantly positive association between distraction and the extent of the disposition effect for the specification with manager and month fixed effects: a one-standard deviation increase in distraction leads to an increase in the disposition effect of about 0.1 percentage points, representing a relative increase of about 8.5%. The effect shrinks and becomes less significant, however, when the month fixed effects are replaced by finer week fixed effects, suggesting that it was driven in part by variation of average distraction over time (which is itself driven by the earnings season). Overall, the results are weakly consistent with the idea that distracted managers are more prone to the disposition effect. At a broader level, they suggest that the impact of behavioral biases can be countered by devoting costly cognitive resources (i.e., attention/mental effort)—implying that those biases are exacerbated when attention constraints tighten.

VII. Robustness

In this section, we provide robustness checks for our results. We first experiment with alternative variable definitions and control for confounding factors in our baseline regression setting. We then also consider a regression setting at the daily frequency. Additional robustness checks and results are collected in the accompanying Internet Appendix. For example, we show in Internet Appendix B that we obtain consistent results when we use a trade-based watchlist definition (instead of the portfolio watchlist used in the paper).

A. Robustness of the Baseline Regression Setting

In this subsection, we present robustness checks for the effect of distraction on our four variables of interest. Table 11 Panels A-B contain the results. For brevity, we only show the coefficient on the distraction measure, although we always run the full specification with 13f controls or industry×quarter fixed effects.

[Include Table 11 here.]

In our first robustness check, we calculate the distraction measure as before, except that we now exclude from the calculation not only the stock in question, but all stocks in the same Fama-French 49 industry. 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-B, distraction continues to have a negative effect on the trading propensity (trade dummy) and trade profitability, a positive effect on transaction costs, and an insignificant effect on trading volume.

Second, we address the concern that our results could be driven by institutional capital or risk management constraints. When such constraints are binding, an institution's decision to trade upon the earnings announcement of a watchlist stock is certainly interlinked with its decision to trade in other stocks. We argue, however, that in its most straightforward interpretation, such an explanation will predict the opposite of what we 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 in order to finance this purchase. But then we would expect to find *more* and 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 our 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 will then mean there is less money (or risk-taking capacity) for buying other stocks.

To control for this possibility, we include the (logarithm of the) total amount traded in announcing stocks as a control variable (while excluding all stock-weeks with an earnings announcement). The idea is that, if our results are driven by capital constraints, then 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 the trading propensity (Table 11 Panel A columns 1-3), row 2 shows that both the statistical and economic significance of the measured distraction effect is largely increased by the inclusion of the control (which itself has a significantly positive coefficient, not shown for brevity). This could mean that the true distraction effect was indeed attenuated by the presence of capital/risk management constraints (as conjectured in the "straightforward" interpretation above). We 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, the inclusion of this control has virtually no effect.

Third, we examine the robustness of our distraction measure. Our baseline measure uses watchlist weights to calculate the average; row 3 in Table 11 Panels A-B show that we get similar albeit slightly weaker results when we 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, we modify the distraction measure to take into account the surprise of the earnings announcement. 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, we follow standard practice and calculate the earnings surprise as the difference between the actual earnings figure and the median earnings forecast reported in I/B/E/S, scaled by the stock's price five days prior to the announcement. We then take the absolute value of the earnings surprise and group the resulting measure into quintiles. Finally, we replace the earnings announcement dummy in our definition of the distraction measure by the absolute earnings surprise quintile:[40]

$$distraction(EA\ surprise)_{imt} = \frac{\sum_{j \neq i} w_{jmt}^{x} \times |EA\ surprise\ quintile|_{jt}}{\sum_{j \neq i} w_{jmt}^{x}}$$

When we repeat our analyses with this modified distraction measure, we again find a significantly negative distraction effect for the trading propensity and trade profitability, as well as an increase in transaction costs (see row 4 in Panels A-B). In terms of economic magnitude, a one-standard deviation increase in distraction (24%), causes a drop in the propensity to trade by about 2.5%, which is comparable to what we found in our baseline case (see Table 2). For the other outcome variables, the economic magnitudes are again similar to those from before.

[40] In order not to lose all the observations when there is no earnings announcement, we set the absolute earnings surprise quintile to zero for stock-weeks in which there was no earnings announcement. The absolute earnings surprise quintile thus takes on values from 0 (no announcement), over 1 (small earnings surpise) to 5 (large earnings surprise).

In our final robustness check, we redefine once more our distraction measure in order to remedy one unappealing feature of the original definition. To see the issue, suppose a manager has three 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 would be 0 for stock A, 2/3 for stock B and 1/2 for stock C. Thus, it would appear 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 our original distraction measure is not meaningful.

For this reason, we now calculate distraction as:

$$distraction(alternative)_{imt} = \sum_{j \neq i} w_{jmt} \times EA\ dummy_{jt}$$

In the example above, 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. We repeat our regressions with this new measure, after excluding earnings announcements from the sample. Now distraction *only* varies across manager-weeks (while being the same for all stocks of the same manager). As shown in row 5 of Panels A-B, our results for all four outcome variables are qualitatively and quantitatively similar to those from before.

B. Daily Regression Setting

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

Specifically, we construct a panel dataset that joins earnings announcement stocks with all managers that hold them in their portfolios (and thus have them on their watchlists). We then construct all dependent and independent variables at the daily frequency and re-run our regression specification (1). The results are shown in Table 12. As before, we find a strongly significant effect on the propensity to trade (extensive margin), but no effect on trading volume (intensive margin). In terms of economic magnitude, a one-standard deviation increase in distraction leads to a reduction in the trading propensity of about 4%,[41] which is roughly 20% larger than the corresponding magnitude from the weekly regression (column 1 of Table 2 Panel A). As shown in Panel B, we again find that both trade profitability and transaction costs suffer when managers are distracted by other announcements. For example, a one-standard deviation increase in distraction decreases the trade profitability by about 22 basis points, which is 10% larger than the corresponding effect at 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-absorbing (and thus most distracting) on the day of the announcement. At the same time, the increase in magnitude is quite modest for at least two reasons. First, as documented in Table 7, the distraction effect is somewhat mitigated when the stock in question has an earnings announcement itself—and this is the case for our daily subsample. Second, the distraction effect is not confined to the announcement date. In fact, we show in Internet Appendix C.4 that the distraction effect for the

[41] 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 of Table 12 Panel A, we thus have a 4% drop in the probability to trade.

trade propensity sets in on the day of the announcements and persists until up to 4 days after the announcement.

VIII. Conclusion

Exploring detailed transaction records, we 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. We find that, despite of these efforts, attention constraints occasionally appear to be binding. Specifically, we find that managers with a large fraction of watchlist stocks exhibiting an earnings announcement are significantly less likely to trade in other stocks, but—conditional on trading—they do not trade in smaller amounts. We further document that distracted managers trade less profitably and incur higher transaction costs, although both of these effects are economically modest.

Next, we analyze how institutional investors cope with their attention constraints. Our findings suggest that attention allocation decisions have multiple facets. On the one hand, we find that investors are less distracted from trading in stocks with an immanent earnings announcement or stocks in which they have a large portfolio stake, consistent with them understanding that it is in their interest to remain relatively more attentive to these stocks. On the other hand, we 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. This behavior tends to exacerbate the disposition effect, and therefore hurts managers' performance. Finally, we show that the distraction effect is mediated by a stock's salience.

Our findings have important implications for the development of a *positive* theory of attention choice—which, to the best of our 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 also governs the quality of trade execution. They are less consistent, however, with the common approach of modelling attention as the choice of signal precisions, at least as long as one insists that signal precisions also affect the choice of position sizes. In terms of attention allocations across stocks, such a model should combine both rational and behavioral elements. Indeed, our 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. We hope that these results can guide future theoretical work in this area.

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Figure 1: Distraction and Trading Propensity

This figure shows the economic magnitude of the distraction effect for different distraction quartiles. The economic magnitude is measured as the reduction in the propensity to trade, relative to its unconditional mean. The numbers come from regressions similar to the ones in Table 2 Panel A, 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). Panel A shows the quartile results for the overall sample that includes all managers. Panel B shows the quartile results for the subset of managers that are classified as non-indexers according to Bushee and Noe (2000) and Bushee (2001).

Panel A: All managers *Panel B: Non-indexers only*

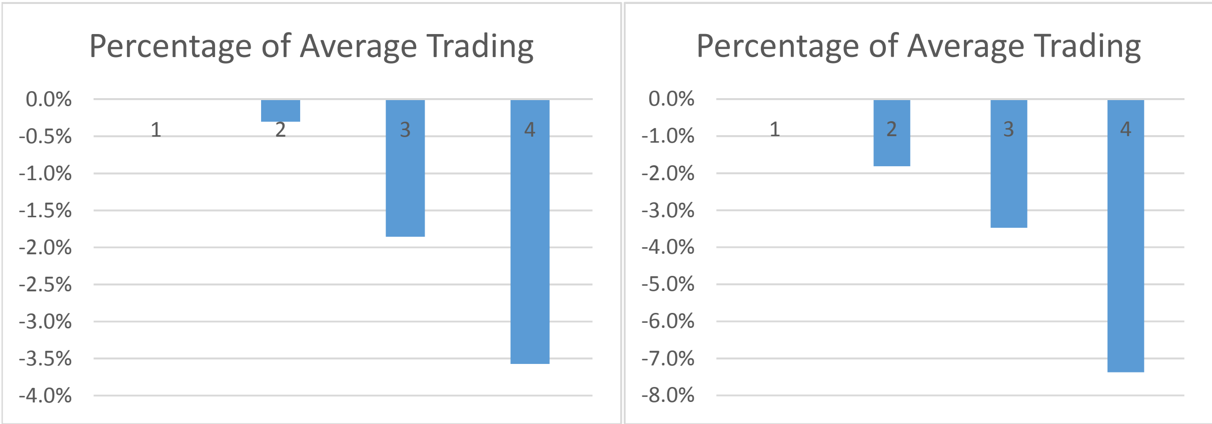

Table 1: Summary Statistics

This table describes the data used in this study. In Panel A, we show summary statistics for all variables used in our panel regressions. *Trade volume* is the weekly trading volume in the stock (if it is positive) in million \$. *Buy volume* is the weekly buy volume in the stock (if it is positive) in million \$. *Sell volume* is the weekly sell volume in the stock (if it is positive) in million \$. *Trade (dummy)* is a dummy variable equal to one if the manager trades the stock in that week. *Buy (dummy)* is a dummy variable equal to one if the manager bought the stock at least once in the week. *Sell (dummy)* is a dummy variable equal to one if the manager sold the stock at least once in the week. *Trade profitability* is the post-trade return (in %) for buys and the post-trade return times minus one for sells. Post-trade returns are calculated over the subsequent 4 weeks of the transaction week. *Relative transaction spread* (in %) is the difference between the transaction price and the previous day closing price for buys (and the same difference times minus one for sells), scaled by the previous day closing price. *Distraction* is defined as the weighted fraction (in %) of a manager's watchlist stocks that have an earnings announcement. The weights correspond to the fraction of portfolio holdings in a particular stock at the end of the previous quarter. *Stocks on watchlist* is the logarithm of the number of stocks on the manager's watchlist. *Trade number* is the number of days on which the stock was traded in the last 12 weeks. *Trade volume manager* is the total trading volume of the manager in the past 12 weeks (in m\$). *Assets under Management* is the amount of assets under management according to 13f (in b\$). *Change in AuM* is the percentage change in assets under management of the manager in the preceding quarter. All variables are defined in the Appendix. In Panel B, we report results of a comparison of the propensity to trade between the portfolio watchlist and a "placebo" watchlist. For the portfolio watchlist, it shows 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. 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. ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Summary statistics for all variables

	Mean	Median	StD	1st perc.	25th perc.	75th perc.	99th perc.
Dependent variables							
Trade volume (m\$)	0.7645	0.0849	4.6140	0.0007	0.0210	0.3883	11.5189
Buy volume (m\$)	0.5902	0.0612	3.1035	0.0006	0.0165	0.2599	9.3704
Sell volume (m\$)	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
Relative 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 (log)	6.9037	7.1770	1.1246	4.0073	6.1181	7.9095	8.2953
Trade number (t-12,t-1)	3.1149	0.0000	9.0043	0.0000	0.0000	1.0000	53.0000
Trade volume manager (t-12,t-1) (b\$)	6.0621	0.1559	19.0157	0.0000	0.0336	1.0845	92.5586
Assets under Management (b\$)	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
Number of observations	40,436,795						

Panel B: Sanity check – are watchlist stocks more likely to be traded compared to other stocks?

Mean # stocks on watchlist	Mean # traded stocks on watchlist	Fraction traded (in %)	Placebo: Fraction traded (in %)	t-statistic of difference
486.83	64.58	13.30	3.45	(11.87)***

Table 2: Distraction and Trading Activity

This table shows results of stock-manager-week level regressions of managers' trading activity on the distraction measure (specification (1) in the text). 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 takes the value one if the manager trades a given stock in a given week and zero otherwise. In Panel B, trading activity is measured at the intensive margin; that is, as the logarithm of the 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 show 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 below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Extensive margin

Dependent Variable:	Trade dummy								
Sample:	All trades			Buys			Sells		
	(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 (log)	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 (t-12,t-1) (log)	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 (t-12,t-1)	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)
Assets under Management (log)		-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)	
Number of Observations	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
Adjusted-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

Panel B: Intensive margin

Dependent Variable:	Trade volume (log)								
Sample:	All trades			Buys			Sells		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Distraction	-0.0838	-0.0874	0.0783	0.0048	0.0064	0.1816	-0.2493	-0.2514	-0.0791
	(-0.35)	(-0.36)	(0.39)	(0.02)	(0.02)	(0.76)	(-0.98)	(-0.99)	(-0.34)
Stocks on watchlist (log)	-0.0870	-0.1453	-14.1346**	-0.1520	-0.3016*	-14.9132**	-0.0102	-0.0083	-28.6154**
	(-0.99)	(-1.09)	(-2.10)	(-1.43)	(-1.86)	(-2.17)	(-0.14)	(-0.09)	(-2.15)
Trade volume manager (t-12,t-1) (log)	0.1747***	0.1743***	-0.1760***	0.1742***	0.1737***	-0.1129**	0.1407***	0.1402***	-0.2040***
	(6.20)	(6.12)	(-3.72)	(7.00)	(7.01)	(-2.19)	(4.62)	(4.57)	(-4.35)
Trade number (t-12,t-1)	0.0065***	0.0061***	0.0124***	-0.0056**	-0.0066***	-0.0025	-0.0005	-0.0005	0.0113***
	(3.08)	(2.72)	(4.07)	(-2.13)	(-2.76)	(-1.00)	(-0.17)	(-0.16)	(3.57)
Assets under Management (log)		0.0967			0.2395*			0.0015	
		(0.86)			(1.89)			(0.02)	
Change in AuM (%)		0.0715			0.2643*			-0.0070	
		(0.55)			(1.76)			(-0.04)	
Number of Observations	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
Adjusted-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

Table 3: Trading Activity – Excluding Quasi-Indexers

This table shows a sample split by whether a manager is a quasi-indexer or not. We run stock-manager-week level regressions of managers' trading activity on the distraction measure (specification (1) in the text). 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 takes the value one if the manager trades a given stock in a given week and zero otherwise. In Panel B, trading activity is measured at the intensive margin; that is, as the logarithm of the 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 1-2 show results for the specification with AuM controls; columns 3-4 show results for the specification with manager×quarter fixed effects (which subsume the AuM controls). In columns 1 and 3, we include only managers that are identified as quasi-indexers according to the classification by Bushee and Noe (2000) and Bushee (2001), while we exclude those managers in columns 2 and 4. The statistical significance of the difference between the two 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 below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Extensive margin

Dependent Variable:	Trade (dummy)			
Subsample:	Quasi-indexer	Other	Quasi-indexer	Other
	(1)	(2)	(3)	(4)
Distraction	-0.0033	-0.0737***	-0.0006	-0.0743***
	(-0.32)	(-3.04)	(-0.06)	(-3.12)
Stocks on watchlist (log)	0.0231	0.0069	-1.0397***	-0.5832**
	(1.64)	(0.92)	(-3.07)	(-2.31)
Trade volume manager (t-12,t-1) (log)	0.0109***	0.0138***	-0.0089***	-0.0210***
	(3.59)	(4.50)	(-3.94)	(-4.84)
Trade number (t-12,t-1)	0.0141***	0.0174***	0.0143***	0.0157***
	(8.37)	(12.58)	(10.77)	(14.93)
Assets under Management (log)	-0.0097	-0.0020		
	(-1.43)	(-0.30)		
Change in AuM (%)	-0.0052	0.0028		
	(-0.63)	(0.32)		
Number of Observations	21,605,783	10,731,575	21,662,609	10,834,767
Adjusted-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 (log)			
Subsample:	Quasi-indexer	Other	Quasi-indexer	Other
	(1)	(2)	(3)	(4)
Distraction	-0.3457	0.1158	-0.1371	0.1665
	(-1.30)	(1.08)	(-0.61)	(1.19)
Stocks on watchlist (log)	-0.3831***	0.0108	-0.2396***	-0.5126
	(-2.60)	(0.11)	(-2.84)	(-0.29)
Trade volume manager (t-12,t-1) (log)	0.1397***	0.1122***	-0.2435***	-0.2034***
	(6.01)	(2.84)	(-6.26)	(-7.05)
Trade number (t-12,t-1)	0.0035	0.0055	0.0095***	0.0165***
	(1.18)	(1.15)	(2.89)	(4.25)
Assets under Management (log)	0.2435***	0.0303		
	(2.85)	(0.19)		
Change in AuM (%)	0.0396	0.1426		
	(0.16)	(0.91)		
Number of Observations	1,767,950	912,637	1,773,262	919,167
Adjusted-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

Table 4: Trading Propensity – Sample Splits by Manager Characteristics

This table shows results for sample splits for the stock-manager-week level regressions of a manager's trading propensity (trade dummy) on the distraction measure (specification (1) in the text). Each row represents a different sample split as indicated in the row header (and explained in Subsection IV.C). For brevity, the table only shows the coefficient on the distraction measure. Columns 1-3 show results for the specification with AuM controls; columns 5-7 show results for the specification with manager×quarter fixed effects (which subsume the AuM controls). Columns 4 and 8 show the test statistics of 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 one if an observation is in the high/yes group and zero if it is in the low/no group. The split variables are defined as follows: 1) turnover is the manager's average dollar trading volume in watchlist stocks over their market capitalization. 2) trade activeness is defined as the minimum of a manager's dollar buys and dollar sells, divided by his total trading volume. 3) industry concentration is defined as the Herfindahl concentration index of a manager's reported stock holdings across Fama-French 49 industries. 4) Institution AuM is the institution's average assets under management. 5) Watchlist size is the average number of stocks on the institution's watchlist. 6) Average profits is the average 48-weeks ahead portfolio return of the watchlist portfolio. 7) Hedge fund is a dummy variable equal to one for hedge funds and zero otherwise (obtained from Russell Jame; explained in Jame, 2017). All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable:	Trade (dummy)							
Sample Split by:	Low/No	Medium	High/Yes	t-stats Difference	Low/No	Medium	High/Yes	t-stats Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1) 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)	
2) 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)	
3) Industry concentr.	-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)	
4) 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)	
5) 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)	
6) 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)	
7) 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 & change in AuM	Yes	Yes	Yes		No	No	No	
Stock×Week f.e.	Yes	Yes	Yes		Yes	Yes	Yes	
Manager×Stock f.e.	Yes	Yes	Yes		Yes	Yes	Yes	
Manager×Quarter f.e.	No	No	No		Yes	Yes	Yes	

Table 5: Distraction and Trade Profitability

This table shows results of stock-manager-week level regressions of trade profitability on the distraction measure (specification (1) in the text). The dependent variable is the post-trade return for buys and the post-trade return times minus one 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 shows the results for the whole sample. Panel B shows a sample split by whether a manager is a quasi-indexer or not. In Panel B columns 1 and 3, we include only managers that are identified as quasi-indexers according to the classification by Bushee and Noe (2000) and Bushee (2001), while we exclude those managers in columns 2 and 4. The statistical significance of the difference between the two 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 below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Overall

	Trade Profitability		
	(1)	(2)	(3)
Distraction	-0.0185**	-0.0199**	-0.0225**
	(-2.14)	(-2.26)	(-2.58)
Stocks on watchlist (log)	0.0004	-0.0006	-0.0978
	(0.34)	(-0.31)	(-1.32)
Trade volume manager (t-12,t-1) (log)	-0.0007	-0.0007	-0.0009
	(-1.27)	(-1.33)	(-0.69)
Trade number (t-12,t-1)	-0.0001***	-0.0001**	-0.0001***
	(-2.83)	(-2.55)	(-3.23)
Assets under Management (log)		0.0015	
		(1.04)	
Change in AuM (%)		-0.0038	
		(-1.00)	
Number of Observations	3,936,029	3,921,673	3,935,910
Adjusted-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			
Subsample:	Quasi-indexer	Other	Quasi-indexer	Other
	(1)	(2)	(3)	(4)
Distraction	0.0046	-0.0290***	0.0033	-0.0316***
	(0.66)	(-3.06)	(0.48)	(-3.02)
Stocks on watchlist (log)	0.0030	-0.0036	-0.0955	-0.0614
	(1.59)	(-1.31)	(-0.96)	(-0.38)
Trade volume manager (t-12,t-1) (log)	0.0003	-0.0005	0.0002	0.0006
	(0.62)	(-0.64)	(0.24)	(0.30)
Trade number (t-12,t-1)	-0.0001***	-0.0001	-0.0001***	-0.0001
	(-4.67)	(-0.93)	(-4.85)	(-1.60)
Assets under Management (log)	-0.0028**	0.0013		
	(-2.16)	(0.99)		
Change in AuM (%)	0.0012	-0.0084		
	(0.43)	(-1.10)		
Number of Observations	1,759,716	909,281	1,765,021	915,790
Adjusted-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

Table 6: Distraction and Transaction Costs

This table shows results of stock-manager-week level regressions of transaction costs on the distraction measure (specification (1) in the text). 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 minus one 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 shows the results for the whole sample. Panel B shows a sample split by whether a manager is a quasi-indexer or not. In Panel B columns 1 and 3, we include only managers that are identified as quasi-indexers according to the classification by Bushee and Noe (2000) and Bushee (2001), while we exclude those managers in columns 2 and 4. The statistical significance of the difference between the two 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 below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Overall

	Relative Transaction Spread		
	(1)	(2)	(3)
Distraction	0.0012***	0.0011**	0.0013**
	(2.83)	(2.36)	(2.10)
Stocks on watchlist (log)	0.0001	0.0002	-0.0003
	(0.59)	(0.89)	(-0.06)
Trade volume manager (t-12,t-1) (log)	0.0000	0.0000	0.0001*
	(0.80)	(0.76)	(1.81)
Trade number (t-12,t-1)	-0.0000***	-0.0000***	-0.0000***
	(-5.13)	(-5.02)	(-4.68)
Assets under Management (log)		-0.0001	
		(-1.07)	
Change in AuM (%)		-0.0002	
		(-0.44)	
Number of Observations	3,950,989	3,936,599	3,950,871
Adjusted-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:	Relative Transaction Spread			
Subsample:	Quasi-indexer	Other	Quasi-indexer	Other
	(1)	(2)	(3)	(4)
Distraction	0.0009*	0.0010	0.0011*	0.0008
	(1.81)	(1.62)	(1.72)	(1.58)
Stocks on watchlist (log)	0.0005***	0.0002	-0.0008	0.0005
	(4.35)	(1.43)	(-0.09)	(0.07)
Trade volume manager (t-12,t-1) (log)	0.0001	0.0000	0.0001	0.0002**
	(1.15)	(0.65)	(0.77)	(2.60)
Trade number (t-12,t-1)	-0.0000***	-0.0000***	-0.0000***	-0.0000***
	(-3.62)	(-6.16)	(-6.92)	(-6.49)
Assets under Management (log)	-0.0002	0.0000		
	(-1.64)	(0.08)		
Change in AuM (%)	0.0007	-0.0005		
	(0.82)	(-1.34)		
Number of Observations	1,767,715	912,207	1,773,026	918,738
Adjusted-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

Table 7: Trading Propensity – Rational Attention Allocation?

This table shows results of stock-manager-week level regressions of a manager's trading propensity (trade dummy) on the distraction measure (specification (1) in the text). 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 (*WatchlistWeight*) 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 one if there is an earnings announcement for the given stock×week and zero otherwise. In this case, the level effect of the interaction variable is subsumed by the stock×week fixed effects. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable:	Trade (dummy)					
	(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×WatchlistWeight	0.5364***	0.5423***	0.4844***			
	(3.64)	(3.58)	(3.53)			
WatchlistWeight	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 (log)	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 (t-12,t-1) (log)	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 (t-12,t-1)	0.0158***	0.0158***	0.0153***	0.0158***	0.0159***	0.0155***
	(8.80)	(9.04)	(14.50)	(8.72)	(8.96)	(14.20)
Assets under Management (log)		-0.0082			-0.0087	
		(-1.31)			(-1.37)	
Change in AuM (%)		-0.0039			-0.0040	
		(-0.62)			(-0.63)	
Number of Observations	39,093,780	38,921,958	39,093,780	39,093,780	38,921,958	39,093,780
Adjusted-R^2	0.48	0.48	0.49	0.48	0.48	0.49
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

Table 8: Trading Propensity – Exacerbating Behavioral Biases?

This table shows results of stock-manager-week level regressions of a manager's trading propensity on the distraction measure (specification (1) in the text). 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 the trade propensity; i.e., a dummy variable that takes the value one if the manager buys or sells the stock in a given week and zero otherwise. In columns 4-6, the dependent variable is the buy propensity; i.e., a dummy that takes the value one if the manager buys a given stock in a given week and zero otherwise. In columns 7-9, the dependent variable is the sell propensity; i.e., a dummy variable that takes the value one if the manager sells the stock in a given week and zero otherwise. In all columns, the distraction measure is interacted with the stock's return in the previous four weeks (*PastReturn*). The level effect of the interaction variable is subsumed by the stock×week fixed effects. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable:		Trade (dummy)			Buy (dummy)			Sell (dummy)	
	(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×PastReturn	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 (log)	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 (t-12,t-1) (log)	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 (t-12,t-1)	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)
Assets under Management (log)		-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)	
Number of Observations	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
Adjusted-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

Table 9: Trading Propensity – Interaction with Salience

This table shows results of stock-manager-week level regressions of a manager's trading propensity on the distraction measure (specification (1) in the text). 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 the trade propensity; i.e., a dummy variable that takes the value one if the manager buys or sells the stock in a given week and zero otherwise. In columns 4-6, the dependent variable is the buy propensity; i.e., a dummy that takes the value one if the manager buys a given stock in a given week and zero otherwise. In columns 7-9, the dependent variable is the sell propensity; i.e., a dummy variable that takes the value one if the manager sells the stock in a given week and zero 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 four weeks (*ExtremeRank*), 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 below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable:	Trade (dummy)			Buy (dummy)			Sell (dummy)		
	(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×ExtremeRank	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)
ExtremeRank	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 (log)	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 (t-12,t-1) (log)	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 (t-12,t-1)	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)
Assets under Management (log)		-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)	
Number of Observations	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
Adjusted-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

Table 10: Distraction and the Disposition Effect

This table shows results of manager-week level regressions of the disposition effect measure on the distraction measure (aggregated at the manager-level, as described in Section VI). Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. Following Odean (1998), the disposition effect measure is calculated as the proportion of gains realized (PGR) minus the proportion of losses (PLR) realized. Specifically, for each manager, we keep track of the average purchase price for all open positions (based on the manager's trading history). Then, for each week in which the manager sells at least one stock, we calculate the PGR (PLR) as the number of positions sold at a gain (loss) over the total number of positions that could have been sold at a gain (loss) in that week and take the difference. Unconditionally, we find a disposition effect of 1.18 percentage points (*t*-statistic = 3.5; based on standard errors clustered at the manager level). To ensure that there is no mechanic relation between the dependent and independent variable, we exclude earnings announcement stocks before calculating the disposition effect measure. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable:	Disposition Effect = Proportion of Gains Realized – Proportion of Losses Realized			
	(1)	(2)	(3)	(4)
Distraction	1.2068***	0.9624**	0.8412*	0.6864
	(3.91)	(2.02)	(1.72)	(1.42)
Stocks on watch list (log)	-0.3784	0.3821	-0.3676	0.4003
	(-1.19)	(0.52)	(-1.16)	(0.55)
Trade volume manager (t-12,t-1) (log)	-0.0532	-0.2505	-0.0650	-0.2753
	(-0.38)	(-0.92)	(-0.46)	(-1.00)
Assets under Management (log)		0.8392		0.8512
		(1.28)		(1.29)
Change in AuM (%)		1.5901		1.6046
		(1.01)		(1.02)
Number of Observations	195,613	69,002	195,613	69,002
Adjusted-R^2	0.32	0.33	0.33	0.35
Manager fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	No	No
Week fixed effects	No	No	Yes	Yes

Table 11: Robustness Checks

This table shows robustness checks for stock-manager-week level regressions of trading activity and performance measures on the distraction measure (specification (1) in the text). Panel A shows the results for trading activity, Panel B those for performance. In Panel A, the dependent variables are the trade dummy (columns 1-3) and the logarithm of trading volume (columns 4-6). In Panel B, the dependent variables are the trade profitability (columns 1-3) and the relative transaction spread (columns 4-6). Each row represents a different robustness check as indicated in the row header (and explained in Subsection VII.A). For brevity, the table only shows the coefficient on the distraction measure. All variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Trading Activity

Dependent Variable:	Trade (dummy)			Trading Volume		
	(1)	(2)	(3)	(4)	(5)	(6)
1) 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)
2) 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)
3) 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)
4) 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)
5) 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 & 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			Relative Transaction Spread		
	(1)	(2)	(3)	(4)	(5)	(6)
1) 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)
2) 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)
3) 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)
4) 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)
5) 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 & 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

Table 12: Daily Regression Setting

This table shows results of stock-manager-date level regressions of trading activity and performance measures on the distraction measure (specification (1) in the text) for the *sample of earnings announcements*. Panel A shows the results for trading activity, Panel B those for performance. In Panel A, the dependent variables are the trade dummy (columns 1-3) and the logarithm of trading volume (columns 4-6). In Panel B, the dependent variables are the trade profitability (columns 1-3) and the relative transaction spread (columns 4-6). Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement on a given day. *Trade volume manager (t-60,t-1) (log)* is the logarithm of the manager's dollar trading volume over the previous 60 trading days. *Trade number (t-60,t-1)* is the number of days out of the previous 60 trading days on which the manager was trading a given stock. All other variables are defined in the Appendix. Standard errors are clustered at the manager level. *t*-statistics are below the parameter estimates in parenthesis; ***, **, * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Trading Activity

Dependent Variable:		Trade (dummy)			Trading Volume	
	(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 (log)	-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 (t-60,t-1) (log)	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 (t-60,t-1)	0.0002***	0.0002***	0.0001***	0.0002*	0.0002*	0.0002*
	(2.73)	(2.75)	(3.37)	(1.72)	(1.74)	(1.92)
Assets under Management (log)		0.0148			0.1567	
		(1.09)			(0.93)	
Change in AuM (%)		0.0126			0.0506	
		(1.22)			(0.20)	
Number of Observations	2,427,358	2,415,715	2,427,314	92,244	92,096	90,471
Adjusted-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			Relative Transaction Spread	
	(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 (log)	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 (t-60,t-1) (log)	-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 (t-60,t-1)	-0.0000	-0.0000	-0.0000	0.0000	0.0000	0.0000
	(-1.48)	(-1.46)	(-1.42)	(0.82)	(0.77)	(0.85)
Assets under Management (log)		0.0002			0.0002*	
		(0.05)			(1.91)	
Change in AuM (%)		-0.0308**			0.0006	
		(-2.16)			(1.59)	
Number of Observations	92,107	91,959	90,338	92,244	92,096	90,471
Adjusted-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

Appendix A: Variable Definitions

This table shows the definitions of the variables used in our study. All continuous variables are winsorized at the 1% threshold on both sides.

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 trade-based watchlist, for which results are reported in the Internet Appendix, the weight corresponds to the fraction of the manager's trading volume in the stock over the past 12 weeks.]	Manager-Stock-Week	ANcerno, 13f, I/B/E/S
Stocks on watchlist (log)	Logarithm of number of stocks that the manager held at the end of the previous quarter. [For the trade-based watchlist, the number of stocks that the manager traded in the past 12 weeks.]	Manager-Week	ANcerno, 13f
Trade volume manager (t-12,t-1) (log)	Logarithm of the dollar trading volume of the manager in the preceding 12 weeks. (Ancerno)	Manager-Week	ANcerno
Trade number (t-12,t-1)	How many days in the last 12 weeks the manager traded the stock.	Manager-Week-Stock	ANcerno
Assets under Management (log)	Logarithm of the dollar amount of assets under management at the end of the previous quarter (13f)	Manager-Week	13f
Change in AuM	Relative change in assets under management from beginning to the end of the previous quarter (13f)	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
Trading Volume (log)	Logarithm of the dollar trading volume of the manager in the stock in that week	Manager-Week-Stock	ANcerno
Trade Profitability	Compounded stock return over the subsequent four weeks for buys, minus one times the compounded stock return over the subsequent four weeks for sells.	Manager-Week-Stock	ANcerno, CRSP
Transaction Costs	The volume-weighted average relative transaction spread, which is 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