Distracted Institutional Investors

Daniel SCHMIDT*

March 3, 2015

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. Hence, attention constraints can be binding even among this elite group of traders. Finally, we investigate the impact of distraction on trade performance and find tentative, albeit weak, evidence that distraction hurts managers' trade execution quality and stock-picking ability.

---

* Preliminary and incomplete, please do not cite without permission. Daniel Schmidt is at HEC Paris, 1 rue de la Liberation, 78350 Jouy-en-Josas, France. Email: schmidt@hec.fr. We acknowledge support by a public grant overseen by the French National Research Agency (ANR) as part of the « Investissements d'Avenir » program (Idex Grant Agreement No. ANR -11- IDEX-0003-02 / Labex ECODEC No. ANR - 11-LABEX-0047).

Limited attention is a fact of life. Yet, we know very little about how limited attention affects the trading behavior and performance of institutional asset managers—arguably the most important class of investors in financial markets today.[1] This lack of knowledge may arise 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? Through exactly which channel do they operate? And how does inattention affect trade 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 that it provides substantial cross-sectional and time-series variation in investor distraction—variation that we exploit to our advantage in the regression approach. In particular, through high-dimensional fixed effects, we can 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. Through the inclusion of *stock×date* fixed effects, we ensure 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. By including *stock×manager* fixed effects, we 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 different points 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]

Specifically, by following institutional investors over time, we construct two different versions of institutional "watchlists"; i.e., stocks that these investors pay attention to. Our first version, which we label *ANcerno watchlist*, is based on past trading: we simply assume that all stocks that the investor traded in the previous 12 weeks are on the investor's watchlist. The second version, which we label *13f watchlist*, is based on portfolio holdings at the end of the previous quarter as reported on form 13f. We verify that both watchlists highly predict future trading, and much better so than randomly-assigned placebo watchlists. Hence, both watchlists capture investor attention as intended. At the same time, since there is only limited overlap between them,[3] showing results for both watchlists provides 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 Verreccia, 1994). Institutional investors have the professional mandate to keep their

[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] Two reasons are responsible for the limited overlap. First, since the 13f watchlist requires a valid link between ANcerno and 13f, we lose a significant number of institutional investors in this subsample. Second, even when there is a link, recent trading and prior holdings are not the same. A manager can make frequent round-trip trades in a stock (in which case it only appears in the ANcerno watchlist), and he can hold on to a stock bought long time ago (in which case it only appears in the 13f watchlist).

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—attention that we argue is missing for trading in other stocks. Our primary distraction proxy is thus the (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 3-4%. 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 increases to up to 8%. 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 flies in the face of standard models of information acquisition in which inattentive investors

[4] The weights correspond to the relative importance of a stock in the watchlist, where relative importance is measured by the fraction of dollar volume for the ANcerno watchlist and by the fraction of portfolio holdings for the 13f watchlist.

adjust at the intensive margin how much information to gather (e.g., Verrecchia, 1982; Van Nieuwerburgh and Veldkamp, 2010). Instead, our results suggest that, even among professional traders, attention is better modeled in terms of a fixed cost to searching and trading in a particular stock (akin to the recognition cost in Merton, 1987).

We then conduct two sample splits that help to reinforce our distraction interpretation. First, we show 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. Second, we 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 other watchlist stocks.

Finally, we investigate the distraction effect on trade performance—both over the very short-term and for longer holding horizons. Our examination of short-term performance serves to study the distraction effect on order execution quality. Microstructure models suggest that limited attention exposes limit order users to the risk of being "picked-off" or not executed (e.g., Dugast, 2014). Moreover, attention-constrained investors may spend less effort bargaining with dealers and/or searching for the best quotes. Hence, we expect distracted managers to incur higher transaction costs. The predictions for future returns are less clear, however. On the one hand, distracted managers may limit trading activity to their best trades, which would boost average trade profitability. On the other hand, distracted managers may spend less effort searching for good trade opportunities, which should hurt their trade performance. All in all, we find suggestive, albeit weak, evidence for a negative distraction effect on trade performance: compared to other managers trading in the same stock, distracted managers appear to be trading at slightly worse prices and are more likely to end up on the "wrong" side of a trade (i.e., buying a stock that subsequently goes down and vice versa).

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. (2015) collect a sample of marriage and divorce events for hedge fund managers and find that their performance suffers during those events. Kempf et al. (2014) 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 manifests itself in trading behavior and performance.

We also offer a methodological contribution by showing how novel distraction proxies can be constructed from trades or portfolio holdings data. For example, we imagine that aggregating our distraction measure across institutions holding a particular stock (similar to Kempf et al., 2014, but at daily or weekly rather than quarterly frequency) can lead to a stock-level distraction measure with rich cross-sectional and time-series variation. Importantly, such a measure would specifically capture the distraction of institutional investors, rather than the inattention by all market participants.[5] As such, this approach holds the promise of identifying the distinct role of institutional investors in financial markets.

The paper proceeds as follows. Section 1 describes the data. Section 2 presents our empirical hypotheses and introduces the identification approach. Section 3 considers the effect of institutional distraction on trading activity. Section 4 studies how distraction affects trade performance. Section 5 presents robustness checks and Section 6 concludes.

I. 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.[6] Puckett and Yan (2011) report that ANcerno trades represent approximately 10% of institutional trading volume in U.S. equities. While institutional investors subscribing to ANcerno are relatively large (they include plan sponsors like CalPERS and money managers such as Fidelity), their trades and stock holdings have been found to be comparable to those of the average investor

[5] In a related paper, Peress and Schmidt (2014) study the impact of sensational news episodes like the O.J. Simpson trial on retail trading behaviour und ultimately market liquidity. Such distraction events are not suited to separately identify the role of institutional investors, because any effect on institutional trading behavior could be the indirect effect of institutions responding to the reduced demand by retail investors.

[6] Previous papers using this data include Goldstein et al. (2009), Chemmanur et al. (2009), Puckett and Yan (2011), Anand et al. (2012), Franzoni and Plazzi (2013), Eisele et al. (2013), Hu et al. (2014), Jame (2014), Chakrabarty et al. (2014), Ben-Rephael and Israelson (2014) and Goetzmann et al. (2014).

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.[7] 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 over various holding periods, we map stock returns from CRSP onto the ANcerno trades.[8] 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

[7] 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 (2014) writes (see footnote 9): "discussions with ANcerno representatives indicate that different clientmgrcodes within a client-manager generally do not reflect different fund products."

[8] See Appendix A for details.

Using the manager names available to us, we hand-match ANcerno managers to institutional holdings data reported in 13f.[9] We are able to find corresponding 13f information for 670 out of the 835 managers in our sample. This match serves several purposes. First, we use it 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,[10] as we expect distraction effects to be weaker for managers following passive investment strategies. Second, we construct a matched sample between ANcerno and 13f which allows to control for the level and change of managers' assets under management. Third, as detailed below, we exploit holdings data to assemble the list of stocks held by each manager at the end of the previous quarter.

C. Watchlist Construction

Our identification rests on the assumption that investors pay more attention to some stocks than to others. This assumption is tested below. We will say that investors have a *watchlist* of stocks, and we call such stocks *watchlist stocks*. We construct two different watchlists for each manager-week pair. Our first version, labelled *ANcerno watchlist*, reflects past trading. More specifically, a given stock i enters the watchlist of manager m in week t when the manager was trading the stock in the previous 12 weeks. Let w_{imt}^{a} be the watchlist weight of stock i, defined as the share of past trading volume going to the stock:

$$w_{imt}^{a} = \frac{\text{trading volume in stock } i \text{ in the past 12 weeks}}{\text{total trading volume in the past 12 weeks}}$$

[9] See Appendix B for details.

[10] 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.

Below, we use this weight when we construct average distraction measures across watchlist stocks.

Our second watchlist, labelled *13f watchlist*, is based on portfolio holdings: 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}^h be the portfolio weight of stock i, defined as:

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

Note that there is only a limited overlap between the trade-based ANcerno watchlist and the holdings-based 13f watchlist. There are two reasons for this. First, we are simply not able to find 13f holdings data for approximately 65% of the manager-quarters in our ANcerno data. Second, even when there is a match, trades and holdings yield different watchlists because (i) a manager can quickly trade in and out of a stock (in which case the stock enters the trade-based watchlist), and (ii) a manager can report holdings for stocks which he did not trade recently (in which case the stock is in the holdings-based watchlist). As such, we prefer to report results using both watchlists.

D. Trade Persistence

If our watchlists capture stocks that managers are paying attention to, we expect those stocks to be traded with higher propensity than a random sample of stocks. To test this prediction, we construct randomly-assigned placebo watchlists in the following way. First, we randomly reshuffle the trades and holdings data, while maintaining differences in trade (holding) intensities across managers and stocks.[11] Second, we use this reshuffled data to construct new trade-based and holdings-based watchlists (called ANcerno-placebo and 13f-placebo). We then compare the fraction of watchlist stocks that are traded in a given week across the different watchlists. Figure 2 plots the evolution of this fraction over time. The important thing to notice is that, for the ANcerno and the 13f watchlists, the average fraction of traded watchlist stocks always exceeds the one for the placebo watchlists: whereas the average fraction for the ANcerno and the 13f watchlist equals 20.4% and 13.3%, respectively, it hovers around 3-4% for the placebo ones. Table 1 Panel B shows that these differences are highly statistically significant (with t-statistics of 40 and 12, respectively). These results give us confidence that our watchlists indeed capture what they are intended to capture.

E. Earnings Announcement Dates

We study how news events in some watchlist stocks affect trading in other watchlist stocks. To proxy for news events, we use earnings announcement dates from I/B/E/S and Compustat. Earnings announcements arguably constitute the most important recurring news releases for individual firms;[12] 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

[11] Specifically, when a manager was trading (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.

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

i had an earnings announcement in week t and zero otherwise.[13] Overall, we have 274,840 earnings announcement weeks, representing roughly 8% of all stock-week observations in our sample period.

II. Methodology and Hypotheses

A. Distraction Measure

The key idea that we exploit in this paper is that different managers are exposed to different news shocks over time. By comparing their trading activity in a given stock, we can isolate such distraction effects from any potential stock-specific reasons why managers may want to trade that stock (e.g., whether the stock is in the news itself).

We now explain how we construct our distraction measure. Recall that w_{jmt}^{a} and w_{jmt}^{h} are the weights of stock j in manager m's trade-based and holdings-based watchlist, respectively, 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:

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

where $x \in \{a, h\}$. 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

[13] 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.

has an earnings announcement. Note also that our distraction measure always lies between 0 and 1 by definition.

Table 1 Panel A shows descriptive statistics for our distraction measure and the other variables used in this study. We see that, for the median manager in our samples, roughly 3.1% 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 in the ANcerno sample trades a given watchlist stock approximately on three different days over the course of 12 weeks and has a weekly trading volume of 1.3$ billion. For the 13f sample, the median manager trades slightly less but has more assets under management.

B. Regression Methodology

Having defined the distraction measure, we now explain our regression approach. The main specification is

$$trade\ activity_{imt} = \alpha_{it} + \alpha_{im} + \beta\ distraction_{imt} + \gamma\ trade\ number_{imt} \\ +\delta\ managercontrols_{mt} + \varepsilon_{imt} \tag{1}$$

where $trade\ activity_{imt}$ is one of the outcome variables introduced below. 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 trading for a vast majority of observations). We therefore estimate specification (1) only on the subset of 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 seriously confounded. The inclusion of stock×date (α_{it}) and stock×manager (α_{im}) fixed effects in specification (1) immunize us against these and related concerns.[14]

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 with the inclusion of the latter two controls, our sample reduces to the subset of manager-quarters for which we could find corresponding 13f holdings. Hence, we show results with and without the inclusion of these controls. Finally, we note that standard errors are clustered at the manager level.

[14] As our identification draws on the comparison *across* managers with different levels of distraction, we cannot include fund×date fixed effects in our specification. Indeed, we can show that, 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.

C. Hypotheses

When it comes to the impact of attention (or the lack thereof) on investor behavior, trading activity should be affected in one way or another. We consider two ways how this can occur. First, there may be an *extensive margin* effect on the propensity to trade in a given stock. Such an effect would indicate that there are significant *search costs* for deciding in which stock to trade. When investors are attention constrained, they are less likely to incur this cost, leading them to forego trading. To capture such an extensive margin effect, we define $trade\ dummy_{imt}$ that takes on the value of one if manager m trades stock i in week t and zero otherwise. With short-sale constraints, the search costs may be larger for buy decisions, because for sell decisions the choice set is reduced to stocks that are currently held (Barber and Odean, 2008). It is not clear, however, whether the institutional investors in our sample are short-sale constrained.[15] Hence, we define dummy variables that separately flag buy and sell decisions.

Second, there may be an *intensive margin* effect due to inattention. Indeed, such an effect is a primitive prediction of models of rational attention choice (e.g., Verrecchia (1982); He and Wang, 1995; Vives, 1995; Van Nieuwerburgh and Veldkamp, 2010). The intuition is that investors trade less aggressively when they possess less precise information—such as when they are distracted. According to this line of work, we expect to see a reduction in $trading\ volume_{imt}$, defined as the logarithm of dollar trading volume, conditional on manager m trading stock i in week t.

[15] According to Jame (2014), the ANcerno data contains short-sales, but it is not possible to distinguish them from other sales.

The effect of inattention on manager's overall profitability is less obvious. For example, suppose that all trades are equally profitable and that distraction leads investors to make fewer trades. In this case, whether inattention hurts or benefits performance depends on the average profitability *per* trade. Here the evidence is mixed: while Puckett and Yan (2011) find that ANcerno clients posit interim trading skills, Chakrabarty et al. (2014) come to the opposite conclusion. Moreover, not all trades are equally profitable and hence the impact of investor distraction may ultimately depends on which type of trades are foregone. Two opposing effects are conceivable: on the one hand, managers could have in mind a clear order of trades. We then expect attention-constrained managers to cut back on the least profitable ones, which will boost their average trade profitability. On the other hand, identifying more profitable trades may itself require more attention. Under this scenario, distraction should reduce average trade profitability. In conclusion, how distraction affects performance is an empirical question—one that we intend to answer by studying how our distraction measure correlates with average trading returns over varying horizons.

Finally, conditional on having decided to trade in a certain stock, distraction may affect order execution quality. Anand et al. (2012) find that execution quality is an economically important contributor to relative performance. 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).[16] Attention-constrained managers may hence decide to use market orders instead of limit orders. In addition, distracted managers may spend less time looking for the best quotes and/or bargaining with brokers, which should lead to a

[16] Dugast (2014) 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).

further increase in transaction costs. We proxy for execution quality with the *intra-day transaction return*, defined as the relative difference between the execution price and the closing price on the day of the transaction. Using this proxy, we investigate whether distraction indeed lowers execution quality as predicted.

III. Distraction and Trading

A. Baseline Results

In this section, we examine how distraction affects trading activity. Table 2 shows the results for the propensity to trade (trade dummy)—first for all trades (columns 1-2) and then for buys and sells separately (columns 3-6). Panels A and B reveal a pervasive distraction effect for both the ANcerno and the 13f watchlist. Based on the exact specification, we find that a one standard deviation increase in our distraction measure reduces the probability to trade by 2.2% to 3.3% relative to its unconditional mean. While the effect may not appear very large, it is important to emphasize 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. Converted into numbers of trades, it implies that the average manager foregoes 9 trades in a week per one-standard deviation increase in distraction.

Analyzing the trading behavior of retail investors, Peress and Schmidt (2014) find a significant distraction effect for buys but not for sells. In contrast, we find a symmetric effect for the buy and sell decisions of institutional investors (columns 3-6). This difference likely stems from short-selling: contrary to retail investors, institutional investors have much larger portfolios and 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 3, 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. As argued above, rational attention models à la Van Nieuwerburgh and Veldkamp (2010) make a clear prediction: paying less attention implies having less precise information, leading investors to trade less aggressively. In contrast, we find no evidence that distracted managers curb their trading amounts (conditional on trading). Based on the estimated standard error, we can reject any intensive margin effect that exceeds 2.4% of the average dollar volume per standard deviation increase in distraction. Hence, even if such an effect exists, its economic magnitude would be small. The failure to support this basic prediction of rational attention models is noteworthy since, if these models are to explain any investor behavior, we expect them to explain best the behavior of professional money managers.

Our 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 (akin to Merton, 1987).

B. Sample Splits

If our results are due to investor distraction as we posit, we expect them to be concentrated for certain type of managers. For example, some managers may 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.[17] Table 4 shows the results. We see that the distraction effect is highly concentrated in the group of non-indexers: the effect for quasi-indexers is either very small (for the ANcerno watchlist, column 1) or non-existent (for the 13f watchlist, column 3). In contrast, the effect for the non-indexers is double the magnitude of the baseline effect documented in Table 2, with a one standard deviation increase in distraction leading to a 4.5%-7.2% reduction in the propensity to trade. As shown at the bottom of the table, the differences between the two subgroups are also statistically significant for both the ANcerno and the 13f sample.

Our next sample split intends 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.[18] We then take the average across weeks and call this measure *trade activeness*. Managers that score high on this measure buy and sell a lot 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

[17] Because we are only able to classify a manager when we can link him to 13f data, we don't lose additional observations by including the 13f controls (level and change in assets under management). Hence, we only show results that include these controls.

[18] 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.

run our analysis separately for managers in the bottom, middle and top tercile in terms of this trade activeness measure. Table 5 columns 1-3 show the results of this exercise: 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 4.7%-7.3% reduction in the propensity to trade, whereas there is no discernible distraction effect for the bottom group. The difference between the top and bottom group is highly statistically significant.

Finally, recall that our distraction measure essentially equals the fraction of watchlist stocks that exhibit an earnings announcement. Such announcements are not idiosyncratic news, however (e.g., Patton and Verardo, 2012). To the degree that they also pertain to other stocks—e.g., those in the same industry—an earnings announcement in one stock may precipitate rather than distract trading in other stocks. 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 portfolio holdings across the Fama-French 49 industries. Table 5 columns 4-6 show that, consistent with our expectation, the distraction effect is largest for the group of managers with low industry concentration.

Taken together, the results from this section support the notion that news events absorb attention that is missing for trading in other stocks, especially for managers that follow active investment strategies across different industries.

IV. Trade Profitability

Our profitability analysis proceeds in two parts. First, we investigate trade profitability on average, and for groups of managers sorted by distraction. The aim of this analysis is to examine whether managers' foregone trades benefit or hurt their performance. Second, returning to the manager-stock-week level, we ask whether distracted managers trade at worse prices and are more likely to be on the wrong side of a trade compared to non-distracted managers *trading in the same stock*. In other words, we test whether distraction affects execution quality and manager's ability to pick stocks that subsequently go up.

A. Overall Trade Profitability

We start with an examination of the average trade profitability of the ANcerno managers in our samples. To be consistent with our previous analyses, we focus on managers' trades in watchlist stocks only. The previous literature disagrees on whether ANcerno managers have trading skills: while Puckett and Yan (2011) answer in the affirmative, Charkrabarty et al. (2014) conclude the opposite. Jame (2014) finds a positive trade performance over a one-month horizon for the subsample of hedge funds in ANcerno, but also notes that this performance evaporates over the subsequent months. We add to this mixed evidence by showing that ANcerno managers have miniscule holding period returns—returns that may become negative once transaction costs are properly accounted for.

Specifically, Table 6 Panel A shows the (volume-weighted) average cumulated return for managers' buys and sells, respectively, and their difference. Like the overall market, stocks bought and sold are going up, while the difference remains small. For example, the buy-sell return difference after one month (~4 weeks) is only 4 basis points and less than 0.5% annualized. In Panel B, we sort managers into quartiles based on our distraction measure and separately show the buy-sell return differences for these groups. No clear pattern emerges, suggesting that the average trade by distracted managers does not differ from the average trade by less distracted ones. Hence, we conclude that, by foregoing a trade, distracted managers do not appear to lose much in terms of performance. We note, however, that the "raw-average" approach of Table 6 conceals a great deal of heterogeneity between managers and the stocks they hold.

B. Execution Quality and Stock-Picking

We now examine whether differences in distraction affect the performance for managers trading in the *same* stock. In other words, we return to specification (1), which includes stock×date and stock×manager fixed effects. While the stock return is obviously the same for all managers, the trade return is not because of (i) differences in the execution price and (ii) differences in the trade direction. Thus, we are essentially testing whether distracted managers trade at worse prices and are more likely to trade in the "wrong" direction; i.e., buying (selling) when the stock subsequently goes down (up).

Our proxy for order execution quality is the difference in average transaction-day returns of buys minus sells. Specifically, for each trade, we calculate the relative difference between the execution price and the day's closing price.[19] We then average all buys and sells (weighted by trading volume) for each manager-stock-week and take the difference. When a manager neither bought nor sold the stock in a given week, we set the buy-sell return difference to missing. The higher this measure, the more favorable was the order execution. Moreover, since trades have price impact (which goes against any intra-day stock return), we expect this measure to be close to zero or even negative on average. Table 1 Panel A confirms this

[19] We are essentially using the transaction-day return as a proxy for the effective spread of the trade. In future research, we will try to improve upon this admittedly crude approach.

intuition: the average buy-sell return difference on transaction days is -0.04% and -0.02% for the ANcerno and the 13f-based sample, respectively.

Using this measure as the dependent variable in Table 7, we find weak support for the notion that distraction hurts execution quality: for the ANcerno sample with full controls (column 2), the effect is significantly negative but economically small. A one standard deviation increase in distraction reduces the buy-sell return difference on transaction days by 1.8 basis points, representing 1.2% of its standard deviation. For the 13f watchlist, the point estimate is also negative but fails to be significant (with a t-statistic of -1.3). Recall that, because of the inclusion of stock×date fixed effects, these results are identified from comparing the average execution prices of different managers in the same stock. In this sense, the buy-sell return difference reflects managers' order execution quality and not, say, their stock-picking skills. We admit, however, that the measure is a relatively noisy proxy of managers' transaction costs, which may explain why we only find weak results.

To measure whether a manager is on the "right" side of a trade, we define a dummy variable that takes the value one if the manager is a net buyer (seller) of a stock that goes up (down) over the following 20 trading days (~1 month) and zero otherwise. We then run specification (1) with this dummy as the dependent variable. Table 8 shows the results: for the ANcerno sample, the effect of distraction is marginally significant in the baseline specification (column 1), but becomes insignificant when 13f-controls are not included (column 2). For the 13f-based sample, the effect is somewhat stronger. For example, the coefficient in column (4) implies that a one-standard deviation increase in distraction reduces the probability to be on the "right side" by 1.3%. We acknowledge, however, that the significance of this result is borderline and disappears when we consider holding periods of longer than 1 month.

Taken together, we find suggestive evidence for the prediction that distraction hurts performance: distracted managers appear to be trading at worse prices and are more likely to be on the sell- (buy-)side of stocks that subsequently go up (down).

V. Robustness

In this section, we present some robustness checks for the effect of distraction on trading propensity. Table 9 contains the results. For brevity, we only show the coefficient on the distraction measure, although we always run the full specification with and without 13f controls.

In our first robustness check, shown in Table 9 row 1, we cluster standard errors at the week rather than at the manager level. It turns out that this only increases the t-statistics. Second, 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 2, the results are not affected by this change. Third, we drop all stock-weeks in which the firm has an earnings announcement from the sample. Row 3 shows that the results are again very similar.

In our final robustness check, we employ an alternative distraction measure that remedies 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 likely to be very meaningful.

For this reason, we now calculate distraction as:

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

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 regression with this new measure, while again excluding earnings announcements from the sample. Importantly, with this analysis, distraction *only* varies across manager-weeks (while being the same for all stocks of the same manager). As shown in row 4, our results are qualitatively and quantitatively similar to those from before.

VI. Conclusion

Exploring detailed transaction records, we investigate and quantify attention constraints among professional asset managers. This is an important task because 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 compared to non-distracted managers, but—conditional on trading—do not trade in smaller amounts. These findings are consistent with models that feature a significant *fixed* search costs for deciding in which stocks to trade (akin to the recognition costs in Merton, 1987), but are inconsistent with models in which attention-constrained investors gather less precise information and curb their trading aggressiveness as a consequence (e.g., Verrecchia, 1982; Van Nieuwerburgh and Veldkamp, 2010).

Finally, we attempt to study the link between distraction and trade performance. While our results are rather weak at this stage, they point in the direction of a negative performance impact. In particular, compared to other managers trading in the same stock, distracted managers appear to be trading at slightly worse prices and are more likely to end up on the "wrong" side of a trade.

To the best of our knowledge, this is the first paper to provide trade-level evidence of distraction effects among professional asset managers. However, one need not stop here: our approach could be used to construct stock-level distraction measures by aggregating across institutions holding a particular stock (as in Kempf et al., 2014, but at higher frequency). Such a measure holds the promise of shedding light on the financial market implications of limited attention among institutional investors—a question of paramount importance. We plan to contribute to this question in future work.

REFERENCES

Anand, A., Irvine, P., Puckett, A., and Venkataraman, K., 2012, Performance of Institutional Trading Desks: An Analysis of Persistence in Trading Costs. *Review of Financial Studies* 25, 557-598.

Aharony, J., and Swary, I., 1980, Quarterly Dividend and Earnings Announcements and Stockholders' Returns: An Empirical Analysis. *Journal of Finance* 35, 1-12.

Beaver, W. H., 1968, The Information Content of Annual Earnings Announcements. *Journal of Accounting Research* 6, 67-92.

Ben-Rephael, A., and Israelsen, R. D., 2014, Are Some Clients More Equal than Others? Evidence of Price Allocation by Delegated Portfolio Managers. Working Paper.

Bernard, V. L., and Thomas, J. K., 1989, Post-Earnings-Announcement Drift: Delayed Price Response or Risk Premium? *Journal of Accounting Research* 27, 1-36.

Barber, B. M., and Odean, T., 2008, All that glitters: The effect of attention and news on the buying behavior of individual and institutional investors. *Review of Financial Studies* 21, 785-818.

Bushee, B. J., 2001, Do Institutional Investors prefer Near-Term Earnings over Long-Run Value?. *Contemporary Accounting Research* 18, 207-246.

Bushee, B. J., Jung, M. J., and Miller, G. S., 2011, Conference Participations and the Disclosure Milieu. *Journal of Accounting Research* 49, 1163-1192.

Bushee, B. J., and Noe, C. F., 2000, Corporate Disclosure Practices, Institutional Investors, and Stock Return Volatility. *Journal of Accounting Research* 38, 171-202.

Brunnermeier, M., and Nagel, S., 2004, Hedge Funds and the Technology Bubble. *Journal of Finance* 59, 2013-2040.

Chakrabarty, B., Moulton, P., and Trzcinka, C., 2014, Short-Term Institutional Trades. Working Paper.

Chemmanur, T., He, S., and Hu, G., 2009, The Role of Institutional Investors in Seasoned Equity Offerings. *Journal of Financial Economics* 94, 384-411.

Cohen, L., and Frazzini, A., 2008, Economic links and predictable returns. *Journal of Finance* 63, 1977-2011.

DellaVigna, S., and Pollet, J., 2009, Investor Inattention, Firm Reaction, and Friday Earnings Announcements. *Journal of Finance* 64, 709-749.

Dugast, J., 2014, Limited Attention and News Arrival in Limit Order Markets. Working Paper.

Eisele, A., Tamara, N., and Parise, G., 2014, Are Star Funds really Shining? Cross-trading and Performance Shifting in Mutual Fund Families. Working Paper.

Fang, L., Peress, J., and Zheng, L., 2014, Does Media Coverage of Stocks Affect Mutual Funds' Trading and Performance?, *Review of Financial Studies* 27, 3441-3466.

Franzoni, F., and Plazzi, A., 2013, Do Hedge Funds Provide Liquidity? Evidence from their Trades. Working Paper.

Goettler, R. L., Parlour, C. A., and Rajan, U., 2005, Equilibrium in a Dynamic Limit Order Market. *Journal of Finance* 60, 2149-2192.

Goettler, R. L., Parlour, C. A., and Rajan, U., 2009, Informed Trading in Limit Order Markets. *Journal of Financial Economics* 93, 67-87.

Goetzmann, W. N., Kim, D., Kumar, A., and Wang, Q., 2014, Weather-Induced Mood, Institutional Investors, and Stock Returns. Working Paper.

Goldstein, M., Irvine, P., Kandel, E., and Weiner, Z., 2009, Brokerage Commissions and Institutional Trading Patterns. *Review of Financial Studies* 22, 5175-5212.

Handa, P., and Schwartz, R. A., 1996, Limit Order Trading. *Journal of Finance* 51, 1835-1861.

He, H., and Wang, J., 1995, Differential Information and Dynamic Behavior of Stock Trading Volume. *Review of Financial Studies* 8, 919-972.

Hirshleifer, D., Lim, S. S., and Teoh, S. H., 2009, Driven to distraction: Extraneous events and underreaction to earnings news. *Journal of Finance* 64, 2289-2235.

Hu, G., McLean, R. D., Pontiff, J., and Wang, Q., 2014, The Year-End Trading Activities of Institutional Investors: Evidence from Daily Trades. *Review of Financial Studies* 27, 1593-1614.

Jame, R., 2014, Do Hedge Funds Create Value from Liquidity Provision?. Working Paper.

Kempf, E., Manconi, A., and Spalt, O., 2014, Distracted Shareholders and Corporate Actions. Working Paper.

Kim, O., and Verrecchia, R. E., 1994, Market Liquidity and Volume around Earnings Announcements. *Journal of Accounting and Economics* 17, 41-67.

Lu, Y., Ray, S., and Teo, M., 2014, Limited Attention, Marital Events and Hedge Funds. Working Paper.

Merton, R. C., 1987, A Simple Model of Capital Market Equilibrium with Incomplete Information. *Journal of Finance* 42, 483-510.

Patton, A. J., and Verardo, M., 2012, Does Beta Move with News? Firm-specific Information Flows and Learning about Profitability. *Review of Financial Studies* 25, 2789-2839.

Peress, J., and Schmidt, D., 2014, Glued to the TV: the Trading Behavior of Distracted Investors. Working Paper.

Puckett, Y., and Yan, X., 2011, The Interim Trading Skills of Institutional Investors. *Journal of Finance* 66, 601-633.

Stambaugh, R. F., 2014, Presidential Address: Investment Noise and Trends. *Journal of Finance* 69, 1415-1453.

Van Nieuwerburgh, S., and Veldkamp, L., 2010, Information Acquisition and Under-Diversification. *Review of Economic Studies* 77, 779-805*.*

Verrecchia, R. E., 1982. Information acquisition in a noisy rational expectations economy. *Econometrica* 50, 1415-1430.

Vives, X., 1995, Short-Term Investment and the Information Efficiency of the Market. *Review of Financial Studies* 8, 125-160.

Wermers, R., 2000, Mutual Fund Performance: An Empirical Decomposition into Stock-Picking Talent, Style, Transaction Costs, and Expenses. *Journal of Finance* 55, 1655-1695.

Appendix A: Match between ANcerno and CRSP

Matching procedure

We match *stockkeys* from ANcerno to permnos from CRSP using 8-digit *cusip*-day pairs. We drop from our sample all stockkeys that have more than one cusip on any particular day.

Since the cusip field in ANcerno is missing for several trades, there are often "holes" in this match. We fill this holes in the following way: Whenever a permno shows up for a stockkey two times without any other permno being present in between, we assign this permno for all dates in between. We also assign the first permno to all prior days of this stockkey and last permno to all following dates.

Quality assessment

On average we can match over 93% of stockkey-dates to permnos. In no month is the matching quota below 90%. As a comparison: Matching on stock symbols (*ticker*) only matches 63% of stockkey-dates. In those cases where we can match stockkey and permno using both ticker and cusip, they yield the same permno in 99.5% of the cases. In those cases where they yield different permnos, the match is better using our cusip method in 99% of the cases. We measure quality of the match as the difference in logs between the average trading price in ANcerno and the CRSP closing price. The match quality is also good in an absolute sense. In only 36 out of over 11 million stockkey-date pairs is the median trading price from ANcerno outside the low-high price range given by CRSP.

Appendix B: Match between ANcerno and 13f

Using the manager names available to us, we hand-match between ANcerno managers (identified by *managercode*) to institutional investors in 13f (identified by *mgrno*). In doing so, we follow a conservative matching approach in order to minimize erroneous matches. We first use a string-proximity algorithm to generate a set of potential matches and then manually select the correct match from these potential matches. We are able to find mgrnos in 13f for 670 out of the 835 managers in our ANcerno sample.

Given the name-matching table, we link each managercode-quarter pair in ANcerno with mgrno-quarter pairs from 13f where available. For the managercode-quarter pairs that we can match, the match is with a unique mgrno in 92% of the cases. For the remaining 8%, there appear multiple mgrnos in 13f in that quarter with a name that matches to ANcerno. It appears that in those cases the different mgrnos represent different state branches of the same manager. We therefore aggregate the 13f holdings across those different mgrnos in those quarters. With this approach, we are able to find holding reports for 6,830 out of 19,686 managercode-quarter pairs in our ANcerno sample.

Appendix C: Variable Definitions

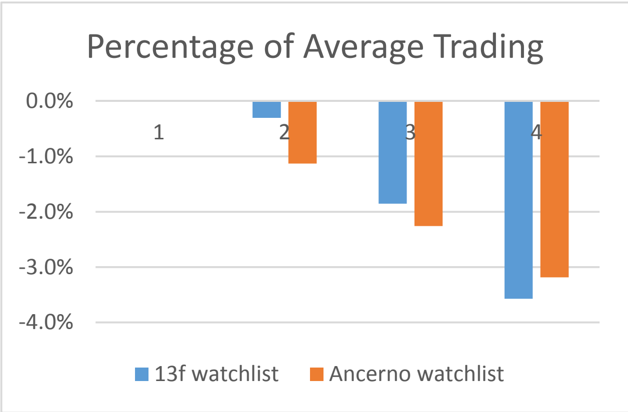
This table shows the definitions of the variables used in our study. All continuous variables are winsorized at the 1% threshold.

Variable Name	Definition	Level
Distraction (ANcerno)	Weighted fraction of a manager's watchlist stocks that have an earnings announcement, where the weights correspond to the fraction of the manager's trading volume in the stock over the past 12 weeks (ANcerno)	Manager-Stock-Week
Distraction (13f)	Weighted fraction of a manager's watchlist stocks that have an earnings announcement, where the weights correspond to the fraction of the manager's holdings in the stock at the end of the previous quarter (13f)	Manager-Stock-Week
Stocks on watchlist (ANcerno) (log)	Logarithm of number of stocks that the manager traded in the past 12 weeks. (ANcerno)	Manager-Week
Stocks on watchlist (13f) (log)	Logarithm of number of stocks that the manager held at the beginning of the quarter (ANcerno)	Manager-Week
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
Trade number (t-12,t-1)	How many days in the last 12 weeks the manager traded the stock	Manager-Week-Stock
Assets under Management (log)	Logarithm of the dollar amount of assets under management at the end of the previous quarter (13f)	Manager-Week
Change in AuM	Relative change in assets under management from beginning to the end of the previous quarter (13f)	Manager-Week
Trade (dummy)	Dummy variable equal to 1 if the manager traded the stock in that week	Manager-Week-Stock
Buy (dummy)	Dummy variable equal to 1 if the manager bought the stock in that week	Manager-Week-Stock
Sell (dummy)	Dummy variable equal to 1 if the manager sold the stock in that week	Manager-Week-Stock
Trading Volume (log)	Logarithm of the dollar trading volume of the manager in the stock in that week	Manager-Week-Stock
Buy-sell Return Difference on transaction days	Difference between the volume-weighted transaction-day return of buys and sells, where the transaction-day return is the relative price change from the execution price (in %) to the day's closing price (ANcerno & CRSP)	Manager-Week-Stock
Right Side of Trade (dummy)	Dummy that takes on the value one if the manager is buying (selling) a stock that goes up (down) over the next month and zero otherwise (ANcerno & CRSP)	Manager-Week-Stock

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, 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). The orange and blue bars show results for the ANcerno trade-based and 13f holdings-based watchlists, respectively.

Panel A: All managers



Panel B: Non-indexers only

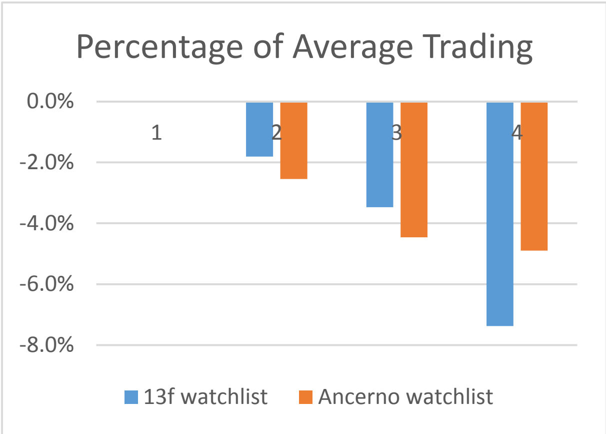


Figure 2: Trade persistence for ANcerno and 13f watchlists

This figure shows time-series plots of the fraction of stocks on different watchlists that are traded over the following week. The blue line is for the ANcerno-based watchlist. The red line is for the 13f-based watchlist. The green line is for a placebo (i.e., randomly assembled) watchlist based on ANcerno trade data. The yellow line is for a placebo watchlist based on reshuffled 13f holdings data.

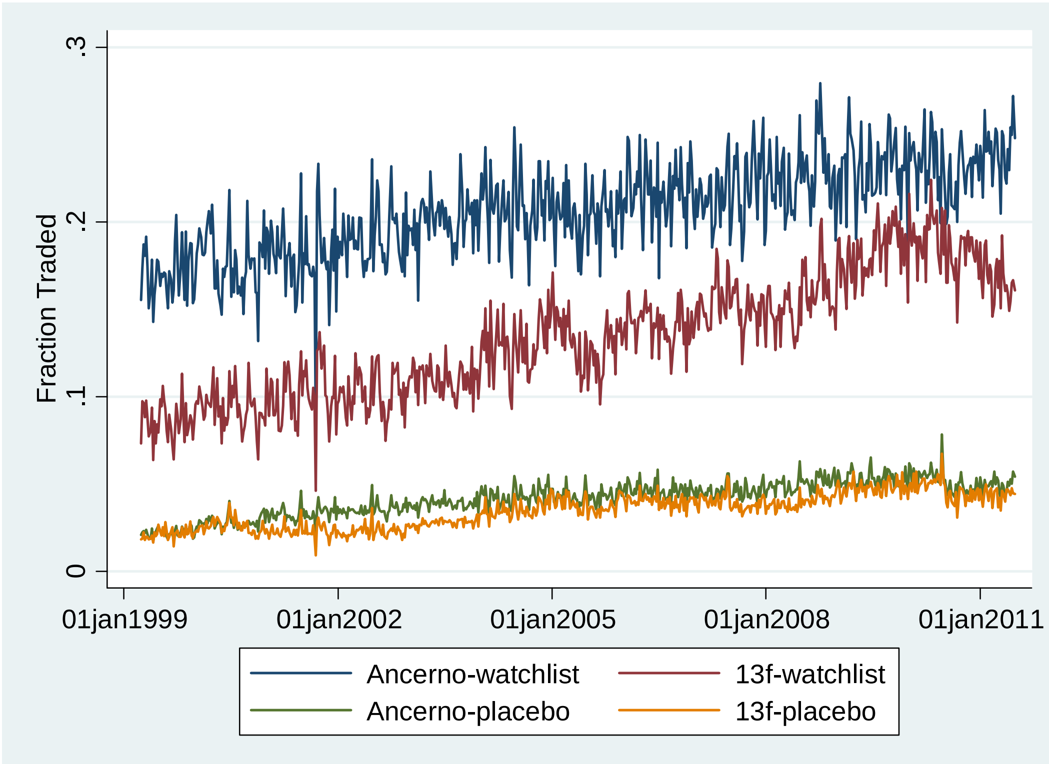


Table 1: Summary Statistics

This table describes the data used in our study. In Panel A, we show summary statistics for all variables used in our panel regressions. The "ANcerno sample – full" contains all stock-manager-week combinations that are part of the ANcerno-based watchlist. The "ANcerno sample – 13f match" contains the subsample of "ANcerno sample – full" for which we could match 13f holdings data at the end of the previous quarter. The "13f based sample" contains all stock-manager-week combinations that are part of the 13f-based watchlist. *Distraction (ANcerno)* is defined as the weighted fraction (in %) of a manager's watchlist stocks that have an earnings announcement, where the weights correspond to the fraction of trading volume in the particular stock over the past 12 weeks. *Distraction (13f)* is defined as the weighted fraction (in %) of a manager's watchlist stocks that have an earnings announcement, where the weights correspond to the fraction of portfolio holdings in the particular stock at the end of the previous quarter. *Stocks on watchlist (ANcerno)* is the number of stocks on the manager's ANcerno watchlist. *Stocks on watchlist (13f)* is the number of stocks on the manager's 13f watchlist. *Trade volume* is the weekly trading volume in the stock (if it is positive) in million $. *Trade number* is the number of days on which the stock was traded in the last 12 weeks. *Trade volume manager* is the total trading volume of the manager in the past 12 weeks (in m$). *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. *Buy-sell return difference on transaction days* is the difference between the volume-weighted transaction-day return of buys and sells, where the transaction-day return is the relative price change (in %) from the execution price to the day's closing price. *Right Side of Trade* is a dummy that takes on the value one if the manager is buying (selling) a stock that goes up (down) over the next month and zero otherwise. *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 within the preceding quarter. In Panel B, we report results of a trade persistence analysis at the manager-week level. For each watchlist (ANcerno and 13f), 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 a Placebo watchlist; ie., a randomly-assembled 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

	ANcerno sample - full			ANcerno sample - 13f match			13f based sample		
	Median	Mean	StD	Median	Mean	StD	Median	Mean	StD
Distraction (ANcerno)	3.1	7.9	10.3	3.1	7.8	10.3			
Distraction (13f)							3.1	8.1	10.8
Trade volume (m$) (if pos.)	0.12	1.87	11.04	0.15	2.03	12.11	0.16	2.16	12.80
Trade number (t-12,t-1)	3	7.5	10.6	3	8.3	12.3	0	3.1	9.0
Trade volume manager (t-12,t-1) (b$)	1.3	10.7	24.7	1.3	12.7	26.3	0.2	6.1	19.0
Trade (dummy)	0.00	0.28	0.45	0.00	0.29	0.46	0.00	0.11	0.32
Buy (dummy)	0.00	0.18	0.38	0.00	0.20	0.40	0.00	0.08	0.27
Sell (dummy)	0.00	0.18	0.38	0.00	0.18	0.38	0.00	0.07	0.25
Buy-sell return difference on transaction days (%)	0.00	-0.04	1.59	0.00	-0.02	1.38	0.00	-0.02	1.33
Goodtrade (dummy)	1.00	0.50	0.50	1.00	0.50	0.50	1.00	0.50	0.50
Assets under Management (b$)				10.1	90.2	149.0	19.1	99.5	154.6
Change in AuM (%)				2.8	2.2	14.2	2.6	2.5	13.8
Number of Observations		57,382,705			17,900,548			40,436,769	

Panel B: Trade persistence for ANcerno and 13f watchlists

	Mean # stocks on watchlist	Mean # traded stocks on watchlist	Fraction traded (in %)	Placebo: Fraction traded (in %)	t-statistic of difference
ANcerno watchlist	275.75	82.36	20.43	4.16	(39.93)***
13f watchlist	486.83	64.58	13.30	3.45	(11.87)***

Table 2: Distraction and Trading Volume – Extensive Margin

This table shows results of stock-manager-week level regressions of managers' trading propensity on the distraction measure. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In columns 1-2, trading propensity is measured by a dummy that takes the value one if the manager trades a given stock in a given week and zero otherwise. Columns 3-4 and 5-6 separate between the buy and sell propensity, respectively. In Panel A, we conduct the analysis for the ANcerno watchlist and in Panel B for the 13f watchlist. All variables are defined in Appendix C. 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: ANcerno watchlist

Dependent Variable:	Trade (dummy)		Buy (dummy)		Sell (dummy)	
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction (ANcerno)	-0.0611***	-0.0837***	-0.0473***	-0.0486***	-0.0319***	-0.0660***
	(-4.57)	(-4.34)	(-4.26)	(-3.78)	(-3.24)	(-3.38)
Stocks on watchlist (ANcerno) (log)	0.0175***	0.0089	0.0111***	0.0031	0.0136***	0.0015
	(4.46)	(1.37)	(3.47)	(0.64)	(4.33)	(0.21)
Trade volume manager (t-12,t-1) (log)	0.0167***	0.0191***	0.0080***	0.0109***	0.0109***	0.0128***
	(6.82)	(4.99)	(3.74)	(2.84)	(6.04)	(3.62)
Trade number (t-12,t-1)	0.0154***	0.0135***	0.0135***	0.0123***	0.0144***	0.0138***
	(20.74)	(10.15)	(46.23)	(21.04)	(56.48)	(52.17)
Assets under Management (log)		0.0053		0.0073**		0.0024
		(1.32)		(2.24)		(0.64)
Change in AuM (%)		-0.0107		0.0084		-0.0244**
		(-0.75)		(0.75)		(-2.35)
Number of Observations	57,313,471	17,701,215	57,313,471	17,701,215	57,313,471	17,701,215
Adjusted-R^2	0.32	0.37	0.32	0.41	0.29	0.31
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: 13f watchlist

Dependent Variable:	Trade (dummy)		Buy (dummy)		Sell (dummy)	
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction (13f)	-0.0354***	-0.0353***	-0.0207***	-0.0203***	-0.0227**	-0.0228**
	(-3.25)	(-3.22)	(-3.25)	(-3.19)	(-2.32)	(-2.32)
Stocks on watchlist (13f) (log)	0.0085	0.0158	0.0044	0.0061	-0.0002	-0.0032
	(1.29)	(1.57)	(1.28)	(1.20)	(-0.05)	(-0.42)
Trade volume manager (t-12,t-1) (log)	0.0117***	0.0119***	0.0049***	0.0049***	0.0055***	0.0054***
	(4.42)	(4.39)	(3.79)	(3.78)	(4.13)	(4.18)
Trade number (t-12,t-1)	0.0158***	0.0159***	0.0140***	0.0140***	0.0147***	0.0147***
	(8.68)	(8.92)	(25.00)	(25.56)	(49.88)	(46.76)
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)		(1.52)		(-1.26)
Number of Observations	39,413,266	39,241,617	39,413,266	39,241,617	39,413,266	39,241,617
Adjusted-R^2	0.48	0.48	0.49	0.49	0.38	0.38
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 3: Distraction and Trading Volume – Intensive Margin

This table shows results of stock-manager-week level regressions of managers' trading intensity on the distraction measure. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. Trading intensity is measured by the logarithm of the dollar trading volume (buys plus sells) by the manager in a given stock and week. The measure is set to missing if the manager does not trade in the stock in a given week. All variables are defined in Appendix C. 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:	Trading Volume (log)			
	(1)	(2)	(3)	(4)
Distraction (ANcerno)	-0.0580	-0.1244		
	(-0.51)	(-0.50)		
Distraction (13f)			-0.0624	-0.0649
			(-0.23)	(-0.23)
Stocks on watchlist (ANcerno) (log)	-0.5810***	-0.4582***		
	(-12.63)	(-6.72)		
Stocks on watchlist (13f) (log)			-0.1100	-0.2050
			(-1.06)	(-1.24)
Trade volume manager (t-12,t-1) (log)	0.3940***	0.3614***	0.1749***	0.1745***
	(15.90)	(8.66)	(4.83)	(4.78)
Trade number (t-12,t-1)	0.0312***	0.0262***	0.0253***	0.0247***
	(9.40)	(11.08)	(9.97)	(9.65)
Assets under Management (log)		-0.0112		0.1524
		(-0.19)		(1.15)
Change in AuM (%)		0.1428		0.0565
		(0.97)		(0.36)
Number of Observations	16,293,088	5,249,252	4,609,571	4,595,689
Adjusted-R^2	0.46	0.46	0.46	0.46
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes

Table 4: Excluding Quasi-Indexers

This table shows sample splits 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. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In columns 1-2, we conduct the analysis for the ANcerno watchlist and in columns 3-4 for the 13f watchlist. 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 Appendix C. 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:	ANcerno Sample		13f Sample	
Subsample:	Quasi-indexer	Other	Quasi-indexer	Other
	(1)	(2)	(3)	(4)
Distraction (ANcerno)	-0.0470**	-0.1272***		
	(-2.23)	(-4.36)		
Distraction (13f)			-0.0033	-0.0737***
			(-0.32)	(-2.97)
Stocks on watch list (ANcerno) (log)	0.0085	0.0202**		
	(1.10)	(1.98)		
Stocks on watch list (13f) (log)			0.0231	0.0069
			(1.63)	(0.90)
Trade volume manager (t-12,t-1) (log)	0.0171***	0.0132**	0.0109***	0.0138***
	(3.12)	(2.36)	(3.56)	(4.40)
Trade number (t-12,t-1)	0.0115***	0.0159***	0.0141***	0.0174***
	(8.30)	(21.90)	(8.30)	(12.29)
Assets under Management (log)	0.0097**	-0.0031	-0.0097	-0.0020
	(2.18)	(-0.65)	(-1.42)	(-0.29)
Change in AuM (%)	-0.0393	-0.0032	-0.0052	0.0028
	(-1.58)	(-0.17)	(-0.63)	(0.31)
Number of Observations	8,028,495	6,715,463	21,990,021	11,254,609
Adjusted-R^2	0.48	0.25	0.56	0.36
Difference in Distraction (t-stat)	2.23**		2.69***	
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes

Table 5: Sample Splits by Industry Concentration and Trade Activeness

This table shows sample splits by terciles of managers' trading activeness and industry concentration of portfolio holdings. We run stock-manager-week level regressions of managers trading activity on the distraction measure. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. In Panel A, we conduct the analysis for the ANcerno watchlist and in Panel B for the 13f watchlist. Trade activeness is defined as the minimum of a manager's dollar buys and dollar sells, divided by his total trading volume. Industry concentration is defined as the Herfindahl concentration index of a manager's reported stock holdings across Fama-French 49 industries. The statistical significance of the difference between the top and bottom tercile 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 an observation is in the top tercile and zero if it is in the bottom tercile. All variables are defined in Appendix C. 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: ANcerno watchlist

Dependent Variable:	Trade (dummy)					
	Trade Activeness			Industry Concentration		
Subsample:	Low	Medium	High	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction (ANcerno)	-0.0148	-0.0453*	-0.1321***	-0.1481***	-0.0251	-0.0446*
	(-0.46)	(-1.77)	(-4.02)	(-3.44)	(-0.98)	(-1.72)
Stocks on watch list (ANcerno) (log)	0.0099	0.0259**	0.0037	0.0087	0.0234***	0.0236***
	(0.80)	(2.54)	(0.43)	(0.91)	(2.66)	(2.59)
Trade volume manager (t-12,t-1) (log)	-0.0048	0.0033	0.0296***	0.0206***	0.0135***	0.0115**
	(-0.74)	(0.76)	(9.13)	(4.00)	(2.73)	(2.05)
Trade number (t-12,t-1)	0.0046	0.0152***	0.0133***	0.0132***	0.0133***	0.0111***
	(1.17)	(15.59)	(10.10)	(9.41)	(15.36)	(6.70)
Assets under Management (log)	-0.0054	0.0152***	0.0034	0.0042	0.0080**	0.0154**
	(-1.54)	(4.00)	(0.51)	(0.66)	(2.36)	(2.51)
Change in AuM (%)	0.0192	-0.0146	-0.0086	-0.0005	-0.0269	-0.0390*
	(0.78)	(-0.66)	(-0.42)	(-0.03)	(-1.49)	(-1.73)
Number of Observations	1,055,302	3,200,167	13,445,746	12,703,198	3,763,086	1,234,931
Adjusted-R^2	0.01	0.27	0.37	0.40	0.29	0.29
Difference in Distraction (High vs. Low) (t-stat)		2.93***			2.12**	
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: 13f watchlist

Dependent Variable:	Trade (dummy)					
	Trading Activity			Industry Concentration		
Subsample:	Low	Medium	High	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction (ANcerno)	0.0112	-0.0048	-0.0739***	-0.0418*	-0.0359***	-0.0148
	(1.05)	(-0.50)	(-3.42)	(-1.76)	(-2.62)	(-1.23)
Stocks on watch list (ANcerno) (log)	-0.0031	-0.0039	0.0232	0.0456**	-0.0087	0.0056
	(-0.33)	(-0.78)	(1.52)	(2.29)	(-1.12)	(0.73)
Trade volume manager (t-12,t-1) (log)	0.0004	0.0047***	0.0187***	0.0120***	0.0091***	0.0107***
	(0.20)	(4.45)	(3.70)	(3.83)	(5.00)	(3.68)
Trade number (t-12,t-1)	0.0157***	0.0189***	0.0152***	0.0154***	0.0158***	0.0137***
	(8.10)	(20.91)	(8.43)	(8.72)	(11.60)	(12.71)
Assets under Management (log)	0.0054	0.0022	-0.0196	-0.0143	0.0008	0.0093
	(1.27)	(0.44)	(-1.59)	(-1.26)	(0.18)	(1.51)
Change in AuM (%)	0.0013	0.0034	0.0005	-0.0082	0.0006	-0.0112
	(0.15)	(0.57)	(0.04)	(-0.72)	(0.06)	(-1.64)
Number of Observations	4,820,428	13,210,039	21,211,150	24,035,011	10,637,649	4,568,957
Adjusted-R^2	0.02	0.30	0.49	0.53	0.39	0.32
Difference in Distraction (High vs. Low) (t-stat)		3.54***			1.04	
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Average Trade Profitability

This table shows summary statistics for the average trade performance. In Panel A, the average cumulated trade returns for buys and sells of watchlist stocks and their respective difference are reported for the ANcerno sample in columns 1-3 and for the 13f sample in columns 4-6. We first take the volume-weighted average cumulated trade return across all buys and sells in the same stock by a given manager and week, and then across all stock-manager-week observations for buys and sells, respectively. The buy-sell return difference (shown in Panel A columns 3 and 6) is defined as the difference between the average cumulated returns for buys and sells. In Panel B, we only report this buy-sell return difference, but for groups of managers sorted by our distraction measure. All numbers are given in %.

Panel A: Overall

Dependent Variable:	Average Cumulated Return					
Sample:	ANcerno Sample			13f Sample		
	Buys	Sells	Buy-Sell Difference	Buys	Sells	Buy-Sell Difference
Holding Horizon	(1)	(2)	(3)	(4)	(5)	(6)
1 Day	0.06	0.06	0.00	0.04	0.06	-0.01
2 Days	0.10	0.08	0.01	0.08	0.09	0.00
3 Days	0.14	0.11	0.02	0.12	0.13	0.00
1 Week	0.22	0.18	0.03	0.20	0.23	0.00
2 Weeks	0.41	0.35	0.04	0.39	0.39	0.03
3 Weeks	0.57	0.53	0.03	0.59	0.63	0.02
4 Weeks	0.76	0.70	0.04	0.79	0.84	0.04
6 Weeks	1.06	0.98	0.06	1.07	1.11	0.06
12 Weeks	1.86	1.80	0.05	2.03	2.05	0.15

Panel B: By Distraction Groups

Dependent Variable:	Buy-Sell Difference in the Average Cumulated Return							
Sample:	ANcerno Sample				13f Sample			
Distraction Group:	Quartile 1	Quartile 2	Quartile3	Quartile 4	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Holding Horizon	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 Day	0.02	-0.01	-0.01	0.02	0.01	-0.06	-0.01	0.03
2 Days	0.03	-0.01	0.00	0.03	0.03	-0.08	0.01	0.05
3 Days	0.03	0.00	0.00	0.04	0.02	-0.09	0.01	0.05
1 Week	0.05	0.01	-0.01	0.04	0.04	-0.09	0.01	0.06
2 Weeks	0.07	0.04	0.00	0.06	0.05	0.00	0.02	0.05
3 Weeks	0.09	0.03	-0.04	0.05	0.06	-0.01	-0.03	0.04
4 Weeks	0.11	0.05	-0.05	0.06	0.08	0.03	-0.05	0.07
6 Weeks	0.14	0.07	-0.07	0.08	0.07	0.11	-0.03	0.12
12 Weeks	0.13	0.09	-0.12	0.10	0.03	0.17	0.37	0.05

Table 7: Trade Execution Quality

This table shows results of stock-manager-week level regressions of manager's trade execution quality on the distraction measure. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. Execution quality is measured by the difference between the volume-weighted transaction-day return of buys and sells, where the transaction-day return is the relative price change from the execution price to the day's closing price (in %). All variables are defined in Appendix C. 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:	Buy-sell Return Difference on transaction days			
Sample:	ANcerno Sample		13f Sample	
	(1)	(2)	(3)	(4)
Distraction (Ancerno)	-0.0712	-0.1715***		
	(-1.62)	(-2.73)		
Distraction (13f)			-0.0939	-0.0905
			(-1.37)	(-1.31)
Stocks on watch list (Ancerno) (log)	-0.0020	-0.0109		
	(-0.34)	(-0.89)		
Stocks on watch list (13f) (log)			0.0060	-0.0052
			(0.46)	(-0.33)
Trade volume manager (t-12,t-1) (log)	-0.0042	-0.0060	-0.0104**	-0.0105**
	(-1.32)	(-0.94)	(-2.03)	(-2.04)
Trade number (t-12,t-1)	-0.0005***	-0.0002	-0.0002	-0.0003
	(-2.82)	(-1.31)	(-1.02)	(-1.32)
Assets under Management (log)		0.0063		0.0176
		(0.94)		(1.20)
Change in AuM (%)		0.0063		-0.0295
		(0.21)		(-0.61)
Number of Observations	16,271,218	5,241,770	4,605,431	4,591,678
Adjusted-R^2	0.12	0.14	0.14	0.14
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes

Table 8: Trade Performance

This table shows results of stock-manager-week level regressions of manager's trade performance on the distraction measure. Distraction is defined as the fraction of a manager's watchlist stocks that have an earnings announcement. Trade performance is measured by a dummy that takes on the value one if the manager is buying (selling) a stock that goes up (down) over the next month and zero otherwise. All variables are defined in Appendix C. 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:	Right Side of Trade (dummy)			
Sample:	ANcerno Sample		13f Sample	
	(1)	(2)	(3)	(4)
Distraction (Ancerno)	-0.0311*	-0.0107		
	(-1.80)	(-0.40)		
Distraction (13f)			-0.0543*	-0.0583**
			(-1.88)	(-2.00)
Stocks on watch list (Ancerno) (log)	0.0008	0.0070		
	(0.25)	(1.14)		
Stocks on watch list (13f) (log)			0.0040	0.0016
			(0.65)	(0.18)
Trade volume manager (t-12,t-1) (log)	-0.0011	-0.0029	-0.0018	-0.0020
	(-0.71)	(-0.82)	(-0.95)	(-1.00)
Trade number (t-12,t-1)	-0.0002***	-0.0002**	-0.0001	-0.0001
	(-3.07)	(-2.58)	(-1.35)	(-1.28)
Assets under Management (log)		0.0020		0.0041
		(0.78)		(0.72)
Change in AuM (%)		-0.0058		-0.0147
		(-0.46)		(-0.73)
Number of Observations	16,262,848	5,240,433	4,600,914	4,587,165
Adjusted-R^2	0.07	0.08	0.07	0.07
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes

Table 9: Distraction and Trading Propensity – Robustness

This table shows robustness checks for stock-manager-week level regressions of managers trading propensity on the distraction measure. Each row represents a different robustness check as indicated in the row header. The specification is otherwise the same as the one from Table 2. For brevity, the table only shows the coefficient on the distraction measure. All variables are defined in Appendix C. Except for row one, 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:	ANcerno Sample		13f Sample	
	(1)	(2)	(3)	(4)
1) Clustering by week	-0.0611***	-0.0837***	-0.0354***	-0.0353***
	(-4.26)	(-4.70)	(-3.86)	(-3.86)
2) Exclude same industry	-0.0581***	-0.0795***	-0.0340***	-0.0338***
	(-4.49)	(-4.26)	(-3.21)	(-3.18)
3) Drop Earnings Announcements	-0.0634***	-0.0837***	-0.0395***	-0.0392***
	(-4.62)	(-4.15)	(-3.55)	(-3.51)
4) Alternative Distraction Measure	-0.0629***	-0.0834***	-0.0419***	-0.0416***
	(-4.49)	(-4.06)	(-3.73)	(-3.69)
Past Trade Controls	Yes	Yes	Yes	Yes
Manager×Week & Stock×Week fixed effects	Yes	Yes	Yes	Yes
AuM and Change in AuM	No	Yes	No	Yes