Vanity in Teams

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Abstract

We hypothesize that vanity amplifies realization utility in teams; admitting mistakes is particularly painful when mistakes have to be admitted to self *and* colleagues. Consistent with the Vanity hypothesis, U.S. stock funds run by teams hold on to losers when losers were initiated by a subset of the team (to avoid admitting a mistake to their non-initiating colleagues), when initiators of loser positions are more experienced (to avoid losing authority by admitting mistakes to junior colleagues), and when all colleagues agree that a position is a loser. Vanity is costly – losers held underperform by a risk-adjusted 1% annually.

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Important investment decisions today tend to be made by teams, be it teams of executives, directors, bankers, or fund managers. Across the sample of actively managed U.S. stock funds studied in this paper, for example, the proportion of funds run by teams has increased from 47% in 1998 to 76% in 2018. This shift points to benefits of fund teams such as their ability to generate new ideas (Harvey, Liu, Tan, and Zhu, 2021), handle managerial turnover (Massa, Reuter, and Zitzewitz, 2010), police illegal activities (Sarkissian and Patel, 2021), and alleviate extrapolation biases (Barahona, Cassella, and Jansen, 2021) outweighing costs of moral hazard (Almazan, Brown, Carlson, and Chapman, 2004) and coordination costs (e.g., Chen, Hong, Huang, and Kubik, 2004, and Dass, Nanda, and Wang, 2013).

In this paper, we hypothesize that, the benefits of teams notwithstanding, team decision making can amplify costly behavioral biases. We propose to study such amplification in the context of the disposition effect, one of the most robust behavioral phenomena documented in the literature (see, e.g., the survey by Kaustia, 2010). The main contribution of our paper is to identify a behavioral mechanism driving greater disposition effects in teams and to show that such effects are costly in a large sample of actively managed U.S. stock funds from 1998-2018.

Specifically, we develop and test the Vanity hypothesis and the Groupthink hypothesis to explain why teams may exhibit costly disposition behavior. According to the Vanity hypothesis, a team member who is primarily responsible for having picked a winner that is subsequently sold not only receives utility from feeling smart himself (pride as in Shefrin and Statman, 1985, or realization utility as in Barberis and Xiong, 2012, and Ingersoll and Jin, 2013), but also from looking good in front of his team members. By the same token, however, realizing a loser not only requires admitting a mistake to himself, but also to his colleagues. Put differently, a team setting

amplifies realization utility received or disutility suffered by the team members responsible for the decision, which in turn produces greater disposition effects.

In contrast, according to the Groupthink hypothesis (Janis, 1982), costly disposition behavior should arise when decisions are made collectively, that is, when all team members bear responsibility. In the presence of groupthink, such a setting could translate into a greater reluctance to sell losers by promoting excessive conformity to the initial view that a position is a good investment. When an investment turns out to be a loser, team members subject to groupthink may optimally choose to suppress information contrary to the shared initial view (see Bénabou, 2013). Groupthink is opposed to vanity, in a sense; Janis (1982, p207) states that "one obvious way to prevent groupthink is simply to make one person responsible for every important decision, eliminating all the problems of group dynamics from the outset."

Measuring the allocation of responsibility of team members with respect to a particular stock in the portfolio is key to testing our hypotheses. We start by identifying "bona-fide" team funds during our 1998-2018 sample as actively managed U.S.-based stock funds that (i) have multiple managers listed in Morningstar Direct and (ii) report only having one advisor in their corresponding N-SAR or N-CEN filings. This is a secondary, methodological, contribution of our paper to the literature as prior work tends to infer the existence of a team solely from multiple managers being listed for a given fund. Our methodology allows us to avoid incorrectly classifying a fund as team-managed when its N-SAR filing reveals multiple (sub-)advisors, implying that the listed managers may not interact as team members at all. Next, we classify a team member as being responsible for a stock if that member was present when the position in the stock was initiated in the portfolio – that is, the most recent instance when the fund went from not holding the stock to holding the stock. Finally, we classify a position as having "collective" responsibility

if all team members are responsible for it or having "specialized" responsibility if only a strict subset of team members is responsible for it.[1] A necessary condition to observe specialized positions is that one or more fund managers join a solo manager or an existing team at some point; this is the case for 4 out of 5 team funds in our sample. Across the almost 5 million team holdings in our sample, 18% are classified as specialized positions and the remainder as collective.

We follow the prior literature and classify a position as a winner or a loser based on the difference between the stock's current price and the volume-weighted average price across all purchases by the fund. Combined with the responsibility considerations above, we classify team positions as either collective winners, specialized winners, collective losers, or specialized losers.

To illustrate this classification, consider the following example of a specialized loser. Suppose that a solo fund manager (A) initiates a position in a stock by buying 100 shares at $70 per share. Further suppose that, after manager B joins the fund, the fund purchases an additional 100 shares at $50 and that the stock is currently trading at $55. The stock would then be considered a specialized loser because only manager A was present at initiation (from which we infer that A has special responsibility for that position) and because the stock trades below its volume-weighted average purchase price of $60 per share.

The Vanity hypothesis predicts that teams should be quick to sell specialized winners (allowing those responsible to reap the double benefits of having been right and showing off to their team peers) and reluctant to sell specialized losers (allowing those responsible to avoid admitting a mistake both to self and to peers). In contrast, the Groupthink hypothesis predicts disposition

[1] We do not require that a team member who initiated a position holds all the responsibility for that position and that non-initiating team members are just bystanders; rather, we posit that the initiating team member will have a greater share of responsibility for the position than a non-initiating colleague. Also, responsibility need not be shared equally among team members for collective positions. Our premise is simply that team member responsibilities will be distributed less equally in specialized positions than in collective positions.

effects for collective positions. Although vanity and groupthink are conceptually opposed, they are not mutually exclusive even within a given team. For example, team members may have individual responsibility for some decisions (potentially giving rise to the effects of vanity) and collective responsibility for others (potentially giving rise to the effects of groupthink). To examine the predictions of the Vanity and Groupthink hypotheses and to discipline our empirical analysis, we start with Hartzmark and Solomon's (2019) linear probability model of an investor's decision to sell a position during a given period as a function of time-varying fund and stock attributes as well as fund- and position size-fixed effects and interactions between holding period and fund fixed effects. We augment this specification by replacing the standard distinction between a winner and loser with indicator variables to accommodate differences between teams and solo managers as well as to test our hypotheses about team behavior: specialized winner, specialized loser, collective winner, collective loser, solo winner, and solo loser.

Consistent with the Vanity hypothesis, we find a strong disposition effect for specialized positions; other things equal, teams are significantly more likely to sell a specialized winner than a specialized loser. In terms of economic magnitude, we find that funds are 2.9% more likely to sell a specialized winner than a specialized loser, corresponding to a 9% increase in the unconditional selling probability.[2] Moreover, the disposition behavior for specialized positions is significantly greater than the disposition behavior in collective positions as well as the disposition

[2] To put this magnitude in the context of existing research, Hartzmark and Solomon (2019, Panel A, Column (4) of Table II, p. 2163) find that mutual funds subject to a disposition effect are 3.6% more likely to sell an unambiguous winner (a stock that is a winner both in terms of total returns and price returns since purchase) than an unambiguous loser (a stock that is a loser both in terms of total returns and price returns since purchase) and 1.1% more likely to sell an unambiguous winner than an ambiguous winner (a stock that is a winner in terms of total returns, but not price returns, since purchase). The economic magnitude of our results regarding the disposition effect is thus similar to the impact of Hartzmark and Solomon's "dividend disconnect" for funds.

behavior in solo positions.[3] The results are robust to adding interactions between holding-period fixed effects and return variables to address the concern that the disposition effect in specialized positions could be the artifact of a complex relation between the propensity to sell and holding periods as well as returns since purchase (see Ben-David and Hirshleifer, 2012).

We then develop and test further implications of the Vanity hypothesis. The first is based on the seminal work on the measurement of narcissism in social psychology by Raskin and Terry (1988) which suggests that vanity is related to a sense of authority and superiority. In the absence of direct measures of a fund manager's vanity, we use a manager's length of tenure in the fund industry relative to peers on his team as a proxy for vanity. Presumably, relative industry experience confers a sense of authority and superiority which are associated with heightened vanity; an experienced manager who enjoys authority will not want to jeopardize it by admitting mistakes to junior peers. Consistent with this conjecture, we find significantly stronger disposition effects in specialized positions when responsible managers have above-median industry experience relative to their colleagues than when they have below-median industry experience relative to their colleagues on the team.[4]

Another test of the Vanity hypothesis is based on the intuition that it should be easier for initiating managers to admit a mistake (i.e., sell a loser) when their non-initiating peers do not consider it to be a mistake (because they do not consider the position to be a loser); similarly, it should be harder for initiating managers to claim credit for selling a winner when their non-

[3] Our premise is that managers with greater responsibility for a position also have a greater say over whether to hold or sell that position. One might thus be concerned that concentration of decision rights, rather than vanity, drives disposition effects. By this logic, however, one would predict disposition effects to be greatest in solo positions and smallest in collective positions; both predictions are at odds with the data.

[4] Longer relative asset management experience may also proxy for lesser career concerns (Chevalier and Ellison, 1999) or greater effects of learning (Seru, Shumway, and Stoffman, 2010). Neither career concerns nor learning, however, predict that the disposition effect should unambiguously increase as a function of relative experience.

initiating peers do not consider it to be a winner. The example above illustrates such a situation. Non-initiating manager B was only present for the purchase at $50 and, unlike initiating manager A, not for the purchase at $70. Given a current price of $55, manager B may thus not think of the position as a loser or mistake. We conjecture that the disposition effect in specialized positions should be greater when all team members agree with the position's classification as either a winner or a loser. This is precisely what we find.

Overall, there is less robust support for the Groupthink hypothesis. Findings of a disposition effect for collective positions depend on the specification and are relatively small in terms of economic magnitude. We then develop and test further implications of the Groupthink hypothesis based on Janis's (1982) conjectures that (i) group cohesiveness is the primary condition giving rise to groupthink and (ii) independent experts may help remedy the effects of groupthink. We proxy for group cohesiveness by the length of time team members have managed a fund together (inspired by "director overlap," a measure proposed by Coles, Daniel, and Naveen, 2020, to capture cohesiveness of corporate boards). We find no evidence, however, that disposition behavior is more pronounced for more cohesive groups. We also find no evidence that disposition behavior weakens significantly after the arrival of new team members (who could be considered as relatively independent experts as they are not prejudiced by prior purchase decisions, at least initially). If anything, disposition effects are significantly weaker during the quarter before a new team member arrives which could be viewed as further support for the Vanity hypothesis – incumbent fund managers anticipate having to admit mistakes to new team members and sell losers to avoid having to do so.

Given that specialized positions require a past change in managerial structure, one might be concerned that our results reflect omitted variables related to past team additions, such as changes

in incentives, effort, or style migration. Theoretically, however, such changes might be expected to work in the opposite direction of our results. For example, it is reasonable to think that specialized positions involve more effort from the responsible parties (because there is less scope for those responsible to free-ride or shift blame) or that specialized positions are subject to greater style drift considerations (e.g., because the team addition proxies for a strategy adjustment). To the extent that effort mitigates bias, one would expect lower, not greater, disposition effects in specialized positions. Similarly, greater style drift in specialized positions would imply that selling decisions are more mechanical which should also lower the disposition effect. Moreover, it is unclear why an omitted variables problem would produce the subsample results discussed above.

Still, to address omitted variables concerns empirically, we re-estimate our model (i) by focusing on "switcher funds" and (ii) by including fund × period fixed effects and conditioning on fund-periods that feature both specialized positions and collective positions for switcher funds.

We define switcher funds as funds that switch to a new management structure by adding one or more managers while keeping the incumbent manager(s). That is, we effectively compare decisions made by just the incumbent manager(s) with decisions jointly made by incumbent(s) and new team member(s). The results are similar.

Including fund × period fixed effects and conditioning on the existence of both specialized and collective positions in a given fund × period allows us to compare, at the same time, the decision of a team to sell a position initiated by all members with the decision of the same team to sell a position initiated only by some members. We continue to find a significant disposition effect in specialized positions in this much smaller sample.

Having established vanity as a candidate explanation for disposition behavior in teams, we ask whether such vanity is costly, that is, whether it is costly for team funds to hold their specialized losers and sell their specialized winners. To answer this question, we calculate the future risk-adjusted performance of specialized losers held and compare it to the future risk-adjusted performance of specialized winners sold. Specifically, we calculate a specialized position's future DGTW Alpha (see Daniel, Grinblatt, Titman, and Wermers, 1997) as the stock's return in excess of a portfolio of stocks with similar size, value, and momentum characteristics.[5] Consistent with vanity being costly, we find that specialized losers held average a future DGTW Alpha of -1.0% per year whereas specialized winners sold average a future DGTW Alpha of +0.2%. Given that specialized losers held represent 8% of a fund's AUM conditional on holding any specialized losers, we can estimate a performance drag of holding specialized losers of 8 basis points per year at the fund level. To put this in the context of existing research, Jin and Scherbina (2011) estimate a one-time savings of 7 basis points from fund managers selling momentum losers they have inherited from the managers they replaced.[6]

Our paper is not the first to document disposition behavior in teams. Cici (2012) reports that investor teams exhibit a higher disposition effect than solo investors, on average, using U.S. stock funds from 1980-2009; Rau (2016) reports a similar result for experiments involving student subjects. Our contribution relative to these two papers is to pinpoint vanity as the driver of team

[5] This methodology is similar to the one used by Odean (1998). The major difference is that we use DGTW Alphas, i.e., examine the risk-adjusted performance, whereas Odean examines returns in excess of the market portfolio (a CRSP value-weighted index).

[6] Our identification of responsibility requires excluding positions inherited from managers who are no longer part of fund management. In contrast, Jin and Scherbina (2011) focus on how new managers deal with such inherited positions.

disposition effects and to demonstrate that vanity is costly in a comprehensive sample of professional investors.

The Vanity hypothesis contributes to the literature examining the effect of peers on investment decision making biases. In perhaps the most closely related paper, Heimer (2016) shows that social interaction causes heightened disposition behavior among retail investors. Relative to Heimer (2016), whose results can be viewed as vanity being at work in a special sample of retail investors (those actively trading foreign currencies and interacting with peers online), our study suggests that professional fund teams managing trillions of dollars are subject to vanity as well, and that such vanity is costly.

The Vanity hypothesis also illustrates limits of the "blame shifting" hypothesis in the context of teams. The blame shifting hypothesis states that the reluctance to sell losers can be overcome by having scapegoats – for example, retail investors in an actively managed fund loser can blame the active fund manager (Chang, Solomon, and Westerfield, 2016) and a fund manager inheriting losers from a fired predecessor can blame that predecessor (Jin and Scherbina, 2011). In our case, there are natural scapegoats for specialized losers (the initiating managers) but not for collective losers, yet teams are more reluctant to sell specialized losers than collective losers. Vanity may trump blame shifting in fund teams because, although other team members may make natural scapegoats, a team member may not want to blame them so as not to jeopardize future collaboration and career prospects.[7]

[7] One could argue that managers are reluctant to sell losers they are responsible for to avoid being blamed by their peers. But this argument also presupposes vanity; responsible managers need to care about how they are perceived by their peers to let the prospect of peer blame affect their actions. The team dynamic we propose is fundamentally different from that in Jin and Scherbina (2011) and Chang, Solomon, and Westerfield (2016) as their scenarios involve blaming actors who neither anticipate nor respond to blame shifting.

Barahona, Cassella, and Jansen (2021) compare extrapolation bias across solo- and team funds. They report that teams attenuate extrapolation bias and suggest that teams benefit from individual team members having to communicate their thoughts to their peers which helps resolve cognitive mistakes. In contrast to their paper, which highlights a benefit of team decision making in terms of forming more rational beliefs, our study highlights vanity as a cost of team decision making due to exacerbating the effects of managers' preferences.

I. Data

I.1 Construction of Data Set

We start with all US-domiciled open-end funds in Morningstar Direct and identify actively managed U.S. equity funds. We use Morningstar Direct as our primary data source because it provides data on fund manager identity as well as the managers' starting and end dates with each fund that they work for during the sample period.[8] Our choice of Morningstar Direct is also guided by the findings of Patel and Sarkissian (2017) that Morningstar Direct offers a better quality of fund manager information than other databases.

In order to create a mapping between the fund information in Morningstar Direct and fund holdings in the Thomson S12 database, we go through a two-step merging process: (i) merging the Morningstar Direct data with the mutual fund database of the Center for Research in Security Prices (CRSP MFDB) and (ii) using the MFLINKS database to merge the data set obtained in step (i) with the Thomson S12 database. In executing step (i), we closely follow the clean-up and

[8] To identify team composition at any given point in time as well as historical purchase prices for different managers, we require individual manager start and end dates provided by Morningstar Direct. These dates are not available for anonymously team-managed funds (Massa, Reuter, and Zitzewitz, 2010). Although we have to exclude these funds from our sample, there is no reason to believe that anonymity vis-à-vis investors would change our results.

merging procedures for the Morningstar and CRSP mutual fund databases described in the Data Appendix of Pastor, Stambaugh, and Taylor (2015). Finally, we merge the resulting database with CRSP's U.S. stock database to add information on fund holdings such as stock prices, corporate actions/ adjustment factors, market capitalization, and book-to-market ratio. Regarding fund holdings, we apply the cleaning procedures proposed by Frazzini (2006). Following Hartzmark (2015), we require a fund to hold at least 20 CRSP merged stocks to be included in the sample.

To identify whether a fund is managed by a "bona-fide" team – that is, a team of managers who interact with one another as required by our hypotheses – we supplement our data set with information on fund-level advisory arrangements available via fund N-SAR filings (for 1998-2017) and N-CEN filings (for 2018). To merge the filings with the CRSP MFDB, we follow the procedure proposed by Moreno, Rodriguez, and Zambrana (2018). We identify bona-fide team funds as those with (i) multiple managers listed in Morningstar Direct and (ii) only one advisor reported in the corresponding filings. Requiring that funds with multiple listed managers have only one advisor allows us to avoid classifying a fund as a team fund when its managers work in different (sub-)advisor entities – potentially as solo managers – and do not interact with each other at all. This is a conservative procedure that results in clean sample of team funds. There are some instances of funds with multiple affiliated advisors where portfolio managers are jointly responsible for the fund even though they work at different corporate entities in different locales. These cases are costly to detect, however, as they usually require a manual prospectus review. Our methodology thus minimizes the error of classifiying a fund as managed by a bona-fide team when it is not, at the expense of excluding some bona-fide teams from the analysis. Similarly, we only classify a fund with one listed manager in Morningstar Direct as a solo-managed fund if the fund

only reports one advisor in the corresponding N-SAR or N-CEN filing (as having multiple (sub-) advisors implies some unobserved shared responsibility).

The Online Appendix contains more details regarding the various matching processes followed in constructing the main data set to facilitate replication.

I.2 Construction of Key Variables and Summary Statistics

Our analysis involves assigning responsibility for each fund holding to one or more managers. We deem a manager responsible for a fund holding if that manager was present in the period when the holding was initiated most recently, that is, the most recent period when the fund went from not holding the stock to holding the stock. We generally assume that a manager was present in a period when his start date with the fund predates the period and his end date with the fund occurs after the period. For the period containing the manager's start date, we assume that the manager was present provided that the start date is within the first 10 days of the beginning of the period. Similarly, for the period containing the manager's end date, we assume that the manager was present provided that the end date is within the last 10 days before the end of the period. The 10-day rule reflects the fact that manager arrivals and departures are tightly bunched around period-ends. We drop fund holdings for which we cannot assign responsibility to any of the current managers. This happens because the corresponding holdings were initiated before any of the current managers joined the fund. Jin and Scherbina (2011) focus on such "inherited" positions and report that managers tend to aggressively sell such positions, especially momentum losers. By excluding such positions, we also ensure that our results are not driven by previously documented selling patterns for inherited positions.[9] The resulting 8,052,083 fund × stock × periods,

[9] Inherited positions account for 17% of all fund × period × stock observations. In untabulated resuls, we verify that our results are robust to including inherited winners and losers as separate position categories.

representing 2090 actively-managed U.S. equity funds managed by 4968 unique managers over the period 1998-2018, form the core sample for our paper.

Based on the assignment of responsibility described above, we further differentiate team positions into positions involving "collective" responsibility (collective positions) and positions involving "specialized" responsibility (specialized positions). Collective positions are holdings that were initiated in the presence of all current team members, whereas specialized positions are holdings that were initiated in the presence of a strict subset of the current team.

Our analysis also involves classifying each fund holding as a winner or a loser. Here, we follow the prior literature (see, e.g., Cici, 2012) and define a position as a winner during a given period if its average daily price during that period is above its volume-weighted average purchase price (and as a loser otherwise). We define a purchase during a given period as an increase in the number of shares held at the end of a period relative to the beginning of the period; we adjust the number of shares held using CRSP's adjustment factor to avoid classifying corporate actions such as splits as purchases. Analogously, we define a sale during a given period as a decrease in the adjusted number of shares held at the end of a period relative to the beginning of the period. In calculating and updating the volume-weighted average purchase price, we follow the prior literature and assume that a purchase during a period happens at the average daily price during that period. Combining the winner/loser classification with the responsibility considerations above results in six position categories and their associated 0-1 indicator variables: *Specialized Winner*, *Specialized Loser*, *Collective Winner*, *Collective Loser*, *Solo Winner*, and *Solo Loser*.

We compute the risk-adjusted performance of each holding observation during the three months following the holdings report date as the difference between the total return of the holding

return and that of a value-weighted portfolio matching the holding's market cap, book-to-market ratio, and prior 12-month return (see Daniel, Grinblatt, Titman, and Wermers DGTW, 1997).[10]

Panel A of Table 1 reports means and standard deviations for our key dependent and independent variables. *Sale*, the dependent variable, is defined as a dummy variable that equals 1 if the position was sold during the fund × stock × period and 0 otherwise. The sample mean of *Sale* is 34%, indicating that funds frequently sell their positions either partially or completely. Specialized positions account for 11.1% of sample observations (=0.077 for *Specialized Winner* + 0.034 for *Specialized Loser*) and 18% of team observations (=(0.077+0.034)/ (0.077+0.034+0.314+0.189)). This relatively small fraction is due to specialized positions requiring the arrival of one or more managers after the position was initiated; collective positions are not subject to such a requirement. Solo fund holdings represent less than 40% of the sample observations, reflecting the shift towards fund teams. The mean 3-month ahead *DGTW Alpha* across holdings is -0.03%, indicating that fund holdings tend to underperform on a risk-adjusted, equally-weighted, basis.

Panel B of Table 1 reports the unconditional selling probability of different winner and loser categories. Unconditionally, mutual funds exhibit a reverse disposition effect across all positions (the probability of selling a loser is higher than the probability of selling a winner); especially in solo positions. In contrast, at the univariate level, there is a pronounced disposition effect in specialized positions.

[10] Following DGTW, we assign each stock to one of 125 portfolios of securities that share similar size, book to market, and momentum characteristics and then construct value-weighted, buy and hold portfolios. Specifically, each year, at the end of June, we sort stocks into quintiles based on their market value. Within each size quintile, we sort stocks in industry-adjusted book-to-market quintiles. Within each of the 25 buckets formed by size and book-to-market quintiles, we sort firms into quintiles based on the preceding 12 months returns of each firm. For each portfolio of the resulting 125 portfolios, we construct a value-weighted monthly benchmark return using the firms' June market capitalizations as weights.

II. Explaining Team Disposition Effects

In this section, we develop and test two hypotheses to explain team disposition effects: the Vanity hypothesis and the Groupthink hypothesis.

II.1 Hypothesis Development

Both hypotheses are based on the premise that selling winners provides psychological benefits whereas selling losers imposes psychological costs. The Vanity hypothesis goes back to the seminal paper by Shefrin and Statman (1985) who conjecture that selling decisions are partly driven by pride and regret. Barberis and Xiong (2012) formalize this idea in terms of realization utility theory and Ingersoll and Jin (2013) extend the theory. Frydman, Barberis, Camerer, Bossaerts, and Rangel (2014) report experimental evidence consistent with realization utility theory. The Vanity hypothesis is also related to the concepts of cognitive dissonance (see, e.g., Chang, Solomon, and Westerfield, 2016) and self-justification (see, e.g., Kaustia, 2010b). The Vanity hypothesis differs from these concepts (and their application in the existing literature) along two dimensions. First, the Vanity hypothesis focuses on the psychological costs attributable to the interaction of actors whereas cognitive dissonance does not require any such interaction. Second, the Vanity hypothesis allows for both psychological costs and benefits of such interaction whereas cognitive dissonance only relates to costs.

Realization utility theory might be viewed as an unlikely starting point for a study of professional investors. After all, a meta comparison of mutual fund managers and individual investors (e.g., Cici, 2012, versus Odean, 1998) suggests that fund managers are less prone to exhibit a disposition effect. This does not imply that fund managers are less prone to realization utility, however. It could well be that fund managers are subject to realization utility, but are also more aware of offsetting effects such as momentum in security returns, tax inefficiencies

associated with selling winners early and holding on to losers, or that they have incentives to sell losers to window-dress their portfolios (see, e.g., Agarwal, Gay, and Ling, 2014).

As realization utility theory is formulated for a single decision maker, it does not provide testable hypotheses "out of the box." Our twofold premise is that (i) the psychological benefits and costs associated with a selling decision for a given position vary with how much responsibility a team member has for that position relative to other team members and (ii) those who had a greater share of responsibility for purchasing a stock also have a greater say in whether that stock should be sold or held.

To illustrate the two hypotheses, it is instructive to contemplate two extreme allocations of responsibility: the case of specialized responsibility (the decision to buy the stock was made by one team member or a strict subset of team members) and the case of collective responsibility (the decision to buy the stock was a consensus decision with the active involvement of all team members).

The Vanity hypothesis predicts that the psychological costs and benefits associated with selling investments are greatest in the case of specialized responsibility. The roots of this hypothesis also go back to Shefrin and Statman (1985, p781) who conjecture that, for retail investors, "the regret at having erred may be exacerbated by having to admit the mistake to others (spouse, the IRS)." The Vanity hypothesis essentially extends this conjecture from retail investors to professional investors, with team members playing the role of others who amplify the effects of pride and regret in Shefrin and Statman's conjecture. This amplification effect due to vanity is not at work in the same way in a solo setting, at least not at the position level. Since a solo manager can be held responsible for all decisions, external evaluators (e.g., bosses, other fund managers in the family, or investors) will focus on overall performance rather than on position-by-position assessments.

Moreover, solo managers can effectively hide their losers from external evaluators by selling them. In contrast, the evaluation of a team member – by fellow team members and others – will naturally focus on decisions for which the member bears special responsibility. Even if a team position is sold and disappears from external view, team members will likely recall who was primarily responsible for that position.

The case of collective responsibility diminishes the role of vanity. This is not to say that team members do not feel pride or regret for their individual contributions when they share responsibility. Rather, it implies any additional pride or regret due to looking good or bad *relative to others* is diminished. Thus, if team disposition effects were driven by vanity, one would expect such effects in cases of specialized responsibility.

In contrast, according to the Groupthink hypothesis, the case of collective responsibility is a necessary condition for groupthink. According to Janis (1982), all team members have to be actively involved in the decision for groupthink to be at work. Active involvement by all team members can promote excessive conformity to the initial view that a position is a good investment. Janis outlines several mechanisms that could drive such excessive conformity relative to the case of a single decision maker. Doubts regarding the initial view may be suppressed by either doubters censoring their own contrary views so as not to offend their colleagues, some team members directly pressuring doubters (e.g., by reminding them that they voted for the initial purchase or threatening to blame them in case the loser was sold and subsequently outperformed), or team members seeking out information that justifies their initial view. Bénabou (2013) develops a groupthink model which delivers collective denial of bad news even though all actors are

individually rational. Thus, if team disposition effects were driven by groupthink, one would expect such effects in cases of collective responsibility.[11]

The theoretical team literature focuses on the benefits of information sharing across team members and the costs of coordination and free-riding (see, e.g., Marschak and Radner, 1972, and Holmstrom, 1982). Presumably, both such benefits and such costs are more pronounced in cases of collective responsibility than in cases of specialized responsibility. Cases of collective responsibility involve individual inputs from all teams members, which may result in better information, but also give rise to free-rider issues (because individual inputs may not be observable) and coordination costs (because individual inputs may require further processing before the team reaches a decision). The net effect is unclear. Moreover, it is unclear why any cost-benefit trade-off at the time of a sale should be systematically related to the status of an investment as a past winner or loser.

Prior research suggests that blame shifting can explain some variation in the disposition effect. Jin and Scherbina (2011) and Chang, Solomon, and Westerfield (2016) argue that the availability of a scapegoat, such as a predecessor (in the case of a new fund manager deciding whether to sell an inherited position) or an active fund manager (in the case of a retail investor deciding whether to sell a fund), reduces the reluctance to sell a loser. Blame shifting, however, appears to be an unlikely explanation for team disposition effects, in general, and disposition effects in specialized positions, in particular. By construction, fund teams offer a greater scope for blame shifting than solo funds, yet teams exhibit more pronounced disposition behavior. Moreover, because initiating

[11] In practice, the allocation of responsibility will lie in between the two extremes of specialized and collective responsibility. For example, if team members specialized in different industries, the team's industry expert would likely have more responsibility for positions in that industry even if all other team members were present when the positions were initiated and are thus classified as collective. This suggests that any observed differences in disposition behavior across collective and specialized positions may understate the effects due to vanity and groupthink.

managers make natural scapegoats for specialized positions, one might expect lower disposition effects for such positions.

II.2 Hypothesis Testing

II.2.1 Baseline Specification

To discipline our empirical analysis, we start with Hartzmark and Solomon's (2019) linear probability model for investor *i*'s decision to sell versus hold a stock *j* during period *t* and augment it by replacing the standard winner indicator with five distinct winner and loser indicators required to test our hypotheses (omitting the *Specialized Loser* category).[12] This gives rise to our baseline specification:

$$Sale_{i,j,t} = \beta_1 Specialized\ Winner_{i,j,t} + \tag{1}$$

$$+\beta_2 Collective\ Winner_{i,j,t} + \beta_3 Collective\ Loser_{i,j,t} +$$

$$+\beta_4 Solo\ Winner_{i,j,t} + \beta_5 Solo\ Loser_{i,j,t} +$$

$$+\boldsymbol{\gamma X_{i,j,t}} + \boldsymbol{\delta_i} + \boldsymbol{\mu_{i,t}} + \boldsymbol{\delta_i} \times \sqrt{Holding\ Period}_{i,j,t} + \epsilon_{i,j,t}$$

where $\mathbf{X_{i,j,t}}$ is a vector of time-varying fund- and stock-specific control variables, $\boldsymbol{\delta_i}$ are fund fixed effects and $\boldsymbol{\mu_{i,t}}$ are portfolio size fixed effects. Control variables include return from (volume-weighted average) purchase price interacted with winner and loser dummies (to control for the possibility that the propensity to sell changes as a function of the magnitude of return since purchase and that these changes differ across the positive and the negative domain), stock volatility estimated over the past 250 trading days interacted with winner and loser dummies (to control for

[12] In turn, this model is based on Ben-David and Hirshleifer (2012); see also Grinblatt and Keloharju (2001) and Birru (2015). Hartzmark (2015) estimates a logit model with the same controls. Like Chang, Solomon, and Westerfield (2016), Hartzmark and Solomon (2019), and An, Engelberg, Henriksson, Wang, and Williams (2019), we opt for a linear probability model to be able to estimate specifications with fixed effects.

the possibility that the propensity to sell changes as a function of past volatility and that these changes differ across the positive and the negative domain), and $\sqrt{Holding\ Period}$. Fund fixed effects control for heterogeneity in average selling propensities across funds (e.g., due to funds having different trading strategies), portfolio size fixed effects control for the possibility that the propensity to sell a given position depends on the number of positions in the portfolio, and interactions between fund fixed effects and $\sqrt{Holding\ Period}$ control for fund-specific variation in the sensitivity of the selling propensity to the holding period. As in Hartzmark and Solomon (2019), standard errors are clustered by fund and period.

The Vanity hypothesis predicts the disposition effect in specialized positions. In terms of regression coefficients, this translates into predicting $\beta_1 > 0$. In contrast, the Groupthink hypothesis makes the prediction $\beta_2 - \beta_3 > 0$.

II.2.2 Baseline Results

Column (1) of Table 2 reports the estimates for the baseline model in Equation (1). The results are broadly consistent with the Vanity hypothesis. There is a disposition effect in specialized positions which is both highly statistically significant and economically meaningful. Other things equal, teams are 2.9% more likely to sell a *Specialized Winner* than a *Specialized Loser* – a 9% increase in selling probability relative to the 34% unconditional probability of selling a specialized position during a given period. There also appears to be a disposition effect in collective positions (*Collective Winners* are sold more aggressively than *Collective Losers*, $\beta_2 - \beta_3 > 0$), which is consistent with the Groupthink Hypothesis. Based on a comparison of regression coefficients, however, disposition behavior in specialized positions is significantly greater than disposition

behavior in collective positions ($\beta_1 > \beta_2 - \beta_3$). Moreover, disposition behavior in specialized positions is significantly greater than disposition behavior in solo positions ($\beta_1 > \beta_4 - \beta_5$).

Column (2) of Table 2 reports the results of a specification that replaces $\sqrt{Holding\ Period}$ with holding period fixed effects, both by themselves and interacted with fund fixed effects as well as Winner/Loser returns since purchase. The motivation for this specification is that specialized positions tend to be longer held and have higher absolute returns since purchase than other positions, coupled with Ben-David and Hirshleifer's (2012) empirical observation that selling propensities (of retail investors) appear to be a complex function of holding periods and returns since purchase. To address the concern that the disposition effect in specialized positions could be the artifact of such a complex function for institutional investors, we use holding period fixed effects rather than imposing a functional form by using $\sqrt{Holding\ Period}$; there are 83 different holding periods in our sample. As a result, the number of control variables jumps from about 5,400 to more than 68,600. For example, instead of including the 3 control variables return × winner dummy, return × loser dummy, and $\sqrt{Holding\ Period}$ as in the baseline specification, we now have 248 controls: return × winner dummy, return × loser dummy, 82 holding period dummies, 82 return × winner dummy × holding period dummy interactions, and 82 return × loser dummy × holding period dummy interactions. The magnitude of the disposition effect in specialized positions does decline in this more saturated specification, but remains statistically significant and economically meaningful, as well as significantly greater than the disposition effect in collective positions ($\beta_1 = 1.7\% > 0.5\% = \beta_2 - \beta_3$) and solo positions ($\beta_1 = 1.7\% > -0.2\% = \beta_4 - \beta_5$). To put the economic magnitude in context of prior research, Hartzmark and Solomon (2019, Table II, p. 2163) find a similar magnitude when comparing the disposition effect

involving stocks that are winners regardless of dividends and the disposition effect involving stocks that are only winners because of dividends.

The main take-away from Table 2 is that team disposition effects primarily manifest in specialized positions which is consistent with vanity being a key driver of team disposition effects.

II.3 Further Tests of Vanity

II.3.1 Stronger Disposition Effect When Responsible Managers Have Relatively More Experience

So far, we have assumed that managers' vanity with respect to a decision is only driven by how much responsibility the managers have for that decision; otherwise, all managers are equally subject to vanity. Susceptibility to vanity, however, will vary across decision makers. In a seminal study on the measurement of narcissism, Raskin and Terry (1988) extract a vanity component from a narcissistic personality inventory and report that vanity is related to a sense of authority and superiority. Netemeyer, Burton, and Lichtenstein (1995) develop and validate vanity scales based on survey responses to attitudes such as "I want my achievements to be recognized by others." In the absence of vanity scores for individual fund managers, which would be ideal for our purposes, we conjecture that managers will be more susceptible to vanity when they are more experienced relative to their team colleagues. It is reasonable to think that experience confers a sense of authority and superiority which is empirically correlated with vanity (Raskin and Terry, 1988). Hence, one would expect greater disposition effects due to vanity when managers are relatively more experienced; managers who enjoy a sense of authority or superiority by virtue of their seniority might view admitting mistakes to their junior colleagues as endangering their authority and thus strive to avoid doing so. Experience is also correlated with other factors that may play a role in disposition behavior such as career concerns (see Chevalier and Ellison, 1999) or learning

over time (see Seru, Shumway, and Stoffman, 2010). Learning, however, would predict that disposition effects become weaker with experience, not stronger. The impact of career concerns on the disposition effect is ambiguous: on the one hand, inexperienced fund managers may be particularly averse to admitting mistakes because mistakes weigh more heavily given a short track record (which would predict that disposition effects become weaker with experience); on the other hand, inexperienced managers may have greater incentives to exert effort which could mitigate behavioral biases.

To proxy for the experience of a fund manager who is responsible for a specialized position in a given quarter, we use the difference between that quarter and the earliest date the manager appears as a fund manager in Morningstar Direct. If there are several managers responsible for a position, we average their experience. We define the experience gap for a specialized position in a given quarter as the difference between the (average) experience of the responsible managers and the (average) experience of their non-responsible team colleagues. Each quarter, we sort all specialized positions in terms of the experience gap and classify specialized positions as having a high experience gap if the experience gap is at or above the median for that quarter or a low experience gap otherwise. The experience gap averages 10 years in the high experience gap categories (*Specialized Winner/Loser High Experience Gap*) versus 2 years in the low experience gap categories (*Specialized Winner/Loser Low Experience Gap*).

Next, we estimate the following modification of the baseline specification, omitting the *Specialized Loser High Experience Gap* category:

$$Sale_{i,j,t} = \beta_1 Specialized\ Winner\ High\ Experience\ Gap_{i,j,t} + \qquad (2)$$

$$+\beta_2 Specialized\ Winner\ Low\ Experience\ Gap_{i,j,t}$$

$$+\beta_3 Specialized\ Loser\ Low\ Experience\ Gap_{i,j,t} +$$

$$+\beta_4 Collective\ Winner_{i,j,t} + \beta_5 Collective\ Loser_{i,j,t} +$$

$$+\beta_6 Solo\ Winner_{i,j,t} + \beta_7 Solo\ Loser_{i,j,t} +$$

$$+\boldsymbol{\gamma X_{i,j,t}} + \boldsymbol{\delta_i} + \boldsymbol{\mu_{i,t}} + \boldsymbol{\delta_i} \times \sqrt{Holding\ Period}_{i,j,t} + \epsilon_{i,j,t}$$

The refined Vanity hypothesis predicts that the disposition effect should be more pronounced in the case of a high experience gap than in the case of a low experience gap, that is, $\beta_1 > \beta_2 - \beta_3$. Column (1) of Table 3 presents model (2) estimates for the baseline specification à la Hartzmark and Solomon (2019) whereas Column (2) of Table 3 reports the results of estimating a specification with holding period fixed effects and their interactions with both fund fixed effects and winner/loser returns.

Consistent with the Vanity Hypothesis, the disposition effect in specialized positions is significantly greater when the experience gap between team members is large. For both specifications, the disposition effect in specialized positions with a high experience gap is roughly 30% greater than the baseline disposition effects in specialized positions reported in the corresponding columns of Table 2.

II.3.2 Stronger Disposition Effect in Unambiguous Winners and Losers

So far, we have assumed that (i) fund managers consider winners successes and losers mistakes and that (ii) all team members agree on the classification of a position as a success or a mistake. These assumptions are reasonable in many cases but need not always be satisfied. Regarding the first assumption, one can imagine a winner that is not a success because, despite being a winner,

the corresponding stock has underperformed some benchmark during its tenure (for example, the market portfolio or the fund's prospectus benchmark or stocks with similar characteristics). Alternatively, a winner may not be considered a success because, although the stock has performed well relative to a benchmark, the fund has been underweight the stock relative to that benchmark (thus hurting relative fund performance). Regarding the second assumption, it is possible for team members to disagree on a position being a success or a mistake as a result of joining the fund at different times (and thus being present for different purchases, resulting in different reference prices). This was demonstrated in the introductory example where manager A viewed a stock trading at $55 as a mistake (because the volume-weighted average price across all purchases for which A was present was $60) whereas manager B viewed that stock as a success (because the volume-weighted average price across all purchases for which B was present was $50).

In this subsection, we maintain the first assumption and examine violations of the second assumption. Identifying violations of the first assumption requires taking a stand on how fund managers benchmark the performance of individual positions (other than relative to their purchase price) and obtaining data for such a benchmark, neither of which are straightforward tasks. In contrast, the second assumption can be relaxed within the existing data set while maintaining the – empirically validated – assumption that investors care about the prices at which they have purchased assets.

The intuition is that, according to the Vanity hypothesis, it should be easier for managers to admit a mistake (i.e., sell a specialized loser) when their peers do not consider it to be a mistake (because they do not consider the position to be a loser). Disagreement about the winner or loser status of a position can only arise in specialized positions because, by construction, fund managers have the same purchase reference price in collective positions (because they were present for all

purchases together). We distinguish between clear specialized positions and ambiguous specialized positions. A *Clear Specialized Winner* is a *Specialized Winner* that is considered a winner by all team members, whereas an *Ambiguous Specialized Winner* is a *Specialized Winner* that is considered a winner by the initiating managers and a loser by at least one non-initiating manager. Similarly, a *Clear Specialized Loser* is a *Specialized Loser* that is considered a loser by all team members, whereas an *Ambiguous Specialized Loser* is a *Specialized Loser* that is considered a loser by the initiating managers and a winner by at least one non-initiating manager. Across all specialized positions, the classification as a winner or loser is clear in 80% of cases.

To examine this hypothesis, we consider a modified specification of model (1):

$$Sale_{i,j,t} = \beta_1 Clear\ Specialized\ Winner_{i,j,t} + \tag{3}$$

$$+\beta_2 Ambiguous\ Specialized\ Winner_{i,j,t} + \beta_3 Ambiguous\ Specialized\ Loser_{i,j,t} +$$

$$+\beta_4 Collective\ Winner_{i,j,t} + \beta_5 Collective\ Loser_{i,j,t} +$$

$$+\beta_6 Solo\ Winner_{i,j,t} + \beta_7 Solo\ Loser_{i,j,t} +$$

$$+\boldsymbol{\gamma X_{i,j,t}} + \boldsymbol{\delta_i} + \boldsymbol{\mu_{i,t}} + \boldsymbol{\delta_i} \times \sqrt{Holding\ Period}_{i,j,t} + \epsilon_{i,j,t}$$

The refined Vanity hypothesis predicts that disposition effects should be more pronounced in clear specialized positions than in ambiguous specialized positions, that is, $\beta_1 > \beta_2 - \beta_3$. Table 4 presents model (3) estimates in the standard Hartzmark/Solomon (2019) specification (Column 1) as well as the specification augmented with holding period fixed effects and their interactions with fund fixed effects and winner/loser returns (Column 2). Consistent with the Vanity Hypothesis, disposition effects in specialized positions that are clear winners and losers are significantly greater.

By itself, this result could also be consistent with the effect of salience which is examined, for example, by Frydman and Wang (2020). They report that a reduction in the salience of reference prices causes a reduction in the disposition effect among retail investors. Extrapolating to our institutional setting, one might thus expect a reduced disposition effect for ambiguous positions because, by definition, such positions do not have one clear reference price. Salience, however, cannot explain the other disposition patterns we observe.

II.3.3 Discussion

One might imagine that disposition behavior could depend on whether a single manager can be held responsible for decisions or whether several managers (still a strict subset of the current team) can be held responsible. The direction, however, is unclear. On the one hand, in the case of a single initiating manager, selling a loser amounts to a clear admission of a mistake on the part of that manager (which may increase the reluctance of that manager to do so). On the other hand, in the case of several initiating managers, selling a loser either requires one manager to take the blame (similar to the single case) or several managers sharing the blame (which one or more of them may be unwilling to do, thus increasing the reluctance of selling a loser). In untabulated results, we find that the results are similar for both cases.

Moreover, one might expect the disposition effect in specialized positions to be attenuated if non-initiating managers were present when additional shares were purchased as the additional purchase might "implicate" the non-initiating managers as well. In untabulated results, however, we do not find significant differences between the case of specialized positions with and without additional purchases; there is a significant disposition effect in both cases. One possibility is that initiating managers retain responsibility for positions they initiated, even if other managers join

the team. Another possibility is that additional purchases tend to reflect rules-based responses to flows rather than active choices by non-initiating team members.

II.4 Further Tests of Groupthink

II.4.1 Disposition Effect Unaffected by Group Cohesion

Collective responsibility is only a necessary, not a sufficient, condition for groupthink to affect team decisions. According to Janis (1982), the primary antecedent for groupthink is the cohesiveness of a group.[13] Thus, it is possible that our failure to detect strong disposition behavior in collective positions is due to our failure to discriminate between decisions made by cohesive teams and those made by non-cohesive teams.

To proxy for the cohesiveness of a team at a given point in time, we adopt the "board intra-connections" measure developed by Coles, Daniel, and Naveen (2020) to measure the cohesiveness of a corporate board. For a given team at a given point in time, we consider all possible pairs of team members. For a given pair, we compute the length of time the two members have served together on the team. Finally, we find the minimum length of time served together across all pairs on the team. This number is our proxy for the cohesiveness of that team at that time. Each period, we sort teams by this proxy and classify teams at or above the median as teams with high cohesion and teams below the median as teams with low cohesion. On average, members on low-cohesion teams have served a minimum of 3 years together whereas members on high-cohesion teams have served a minimum of 7 years together.

Based on the classification of a team as low- or high-cohesion at a point in time, we modify the baseline specification (1) as follows:

[13] Even then, however, Janis (p242) is careful to point out that cohesive groups are not "doom[ed] to become victims of groupthink every time they make a collective decision."

$$Sale_{i,j,t} = \beta_1 Specialized\ Winner_{i,j,t} + \quad (4)$$

$$+\beta_2 Collective\ Winner\ High\ Cohesion_{i,j,t} + \beta_3 Collective\ Loser\ High\ Cohesion_{i,j,t} +$$

$$+\beta_4 Collective\ Winner\ Low\ Cohesion_{i,j,t} + \beta_5 Collective\ Loser\ Low\ Cohesion_{i,j,t} +$$

$$+\beta_6 Solo\ Winner_{i,j,t} + \beta_7 Solo\ Loser_{i,j,t} +$$

$$+\boldsymbol{\gamma X_{i,j,t}} + \boldsymbol{\delta_i} + \boldsymbol{\mu_{i,t}} + \boldsymbol{\delta_i} \times \sqrt{Holding\ Period}_{i,j,t} + \epsilon_{i,j,t}$$

The more targeted version of the Groupthink hypothesis predicts that disposition effects should be greater in collective positions of high-cohesion teams than in those of low-cohesion teams, that is $\beta_2 - \beta_3 > \beta_4 - \beta_5$. Table 5 presents model (4) estimates for the standard Hartzmark/Solomon (2019) specification (Column 1) as well as the specification augmented with holding period fixed effects and their interactions with fund fixed effects and winner/loser returns (Column 2). The corresponding results fail to support the refined Groupthink hypothesis.

II.4.2 Disposition Effect Unaffected by Expert Arrival

Janis (1982, p. 266) conjectures that independent experts help remedy groupthink: "One or more outside experts or qualified colleagues within the organization who are not core members of the policy-making group should be invited to each meeting on a staggered basis and should be encouraged to challenge the views of the core members." One can view the joining of one or more fund managers as the arrival of (somewhat) independent experts, as they are not prejudiced by prior purchase decisions. The presence of such experts may thus prompt the team to revisit the decision of holding on to losers or sell winners, thereby promoting more rational decision making.

To examine this conjecture, we start by identifying all instances in which an already existing team (that has operated in a stable configuration for at least one year) adds at least one member

and keeps all incumbents as well as at least one of the new members for a year. We require all incumbent team members to remain on the team so as not to confound "expert arrival" with "team member replacement" because the latter may prompt more aggressive selling of losers as reported by Jin and Scherbina (2011). For each of these events, we differentiate between three periods: (i) a 3-quarter pre-period in which the team operated in a stable configuration up until the quarter just prior to expert arrival, (ii) the quarter just prior to expert arrival, and (iii) the year after expert arrival. We differentiate between (i) and (ii) to examine whether a team changes its disposition behavior in anticipation of expert arrival. Such a change could be motivated by vanity: if managers are aware that the fund is looking to add new managers and are aware of their vanity, they might want to disproportionately sell losers to window-dress (to avoid having to admit mistakes later on by selling losers in front of new team members). This gives rise to the following specification (omitting the indicator variable $Loser > 1\ Quarter\ Before\ Expert\ Arrival$)

$$Sale_{i,j,t} = +\beta_1 Winner\ Year\ After\ Expert\ Arrival_{i,j,t} + \quad (5)$$

$$+\beta_2 Loser\ Year\ After\ Expert\ Arrival_{i,j,t} +$$

$$+\beta_3 Winner\ Quarter\ Before\ Expert\ Arrival_{i,j,t} +$$

$$+\beta_4 Loser\ Quarter\ Before\ Expert\ Arrival_{i,j,t} +$$

$$+\beta_5 Winner > 1\ Quarter\ Before\ Expert\ Arrival_{i,j,t} +$$

$$+\boldsymbol{\gamma X_{i,j,t}} + \boldsymbol{\delta_i} + \boldsymbol{\mu_{i,t}} + \boldsymbol{\delta_i} \times \sqrt{Holding\ Period}_{i,j,t} + \epsilon_{i,j,t}$$

Based on the Groupthink hypothesis and the remedial effect of arriving experts, one would expect disposition behavior to weaken after expert arrival ($\beta_1 - \beta_2 < \beta_3 - \beta_4\ and\ \beta_1 - \beta_2 < \beta_5$). Table 6 reports the results of estimating Equation (5). The evidence is not consistent with

this conjecture. If anything, there is a significant reverse disposition effect during the quarter prior to the team addition which turns insignificant after the team addition. This could be viewed as further support for the Vanity Hypothesis; incumbent managers sell losers before the arrival of a manager to whom the mistakes would have had to be admitted to.

II.5 Alternative Explanations

The addition of one or more managers at some point in the fund's past is a necessary condition to observe specialized positions. This conditioning prompts the following alternative explanations: (i) our results may be an artifact of comparing funds with changing management structure to funds with a stable management structure, and (ii) our results may reflect omitted variables associated with team additions. The subsections below address these explanations in turn.

II.5.1 Differential Stability of Management Structure

One might be concerned that funds whose managerial structure changes are different from funds whose structure remains stable in ways that are not captured by the control variables, and that this difference drives our results. It is possible, for example, that fund teams whose managerial structure changes at some point during the sample are subject to greater behavioral biases throughout the sample period and thus exhibit greater disposition behavior when selling positions.

To address this possibility, we focus on the sample of funds that switch structure by adding one or more managers at some point during the sample period. Table 7 reports the results, which are very similar to the baseline results reported in Table 2. Thus, our results do not appear to be artifacts of comparing funds with stable management to funds with changing management.

II.5.2 Omitted Variables Due to Team Changes

It is still possible that disposition behavior in switcher funds somehow increases with the arrival of new managers across both collective and specialized positions and that we mistakenly attribute this increase to specialized positions and hence vanity. This could happen because specialized positions can only be observed after the arrival of new managers, whereas collective positions can be observed both before and after the arrival. To address this possibility, we add fund × period fixed effects and further restrict the sample to holdings of switcher funds after the arrival of one or more new managers. In addition, we require each fund × period to contain at least one specialized position and at least one collective position to be included in the sample. This effectively allows us to compare the decision of a team to sell a specialized position in a given period with the decision of the same team to sell a collective position during the same period:

$$Sale_{i,j,t} = \beta_1 Specialized\ Winner_{i,j,t} + \quad (6)$$

$$+\beta_2 Collective\ Winner_{i,j,t} + \beta_3 Collective\ Loser_{i,j,t} +$$

$$+\boldsymbol{\gamma X_{i,j,t}} + \boldsymbol{\mu_{i,t}} + \boldsymbol{\delta_i} \times \boldsymbol{\tau_t} + \boldsymbol{\delta_i} \times \sqrt{Holding\ Period}_{i,j,t} + \epsilon_{i,j,t}$$

Column (1) of Table 8 reports the results of estimating Equation (6). Consistent with the Vanity Hypothesis, we continue to find a statistically significant and economically meaningful disposition effect in specialized positions. Moreover, disposition behavior in specialized positions is significantly greater than disposition behavior in collective positions. When we replace the interactions between $\sqrt{Holding\ Period}$ and fund fixed effects with interactions between holding period fixed effects and fund fixed effects and add interactions between holding period fixed effects and winner/loser returns (see Column (2) of Table 8 for this saturated specification that offers only about 6 observations for each parameter to be estimated), the coefficients of all winner/loser indicator variables decrease, but the disposition effect in specialized positions

remains statistically significant and significantly greater than the disposition effect in collective positions.

III. Vanity Is Costly

Having established vanity as a candidate explanation for disposition behavior in specialized positions, we examine whether holding specialized losers and selling specialized winners imposes a cost on team funds. To do so, we follow Odean (1998) and compare the future excess returns of specialized losers held with the future excess returns of specialized winners sold. Odean (1998) defines future excess return over the next 84 (252 and 504) trading days by subtracting the CRSP value-weighted index return from the return of the stock for the corresponding period. We modify Odean's analysis in two ways. First, we choose the time between the current and the next holdings report date as the future return window (typically a quarter); doing so avoids overlapping future return windows which simplifies the statistical inference and, importantly, yields a more accurate estimate of the performance impact of specialized losers held.[14] Second, we subtract the value-weighted return of a portfolio of stocks that match the size, book-to-market, and past return characteristics of the holding (see Daniel, Grinblatt, Titman, and Wermers, 1997). Doing so delivers the DGTW Alpha which measures the holding's future risk-adjusted return; in particular, the DGTW Alpha should remove the effects of disposition behavior loading against the momentum factor. In order to reduce the influence of outliers and errors, we winsorize DGTW Alphas at the 1% and 99% level at the stock-month level.

Panel A of Table 9 shows that for team funds, specialized winners sold have a weakly positive DGTW Alpha of 0.05% during the three months after the sale whereas specialized losers held

[14] This is because losers held will only affect our performance calculations for as long as they are part of the fund portfolio (in contrast, picking a longer future period that goes beyond the next report date runs the risk of having losers "held" contaminate performance calculations if they are sold at some point during this longer future period).

deliver a significantly negative DGTW Alpha of -0.25% (corresponding to an annualized DGTW Alpha of -1%). The performance differential of 0.30% over the next three months is economically meaningful and statistically significant, assuming that all observations are independent.[15] Thus, the disposition effect in specialized positions appears to be costly for team funds, even before considering momentum effects and the likely adverse tax implications of selling winners and holding on to losers. Given that specialized losers held represent an average 8% of a fund's AUM (conditional on the fund holding any specialized losers), we can estimate a performance drag of holding on to specialized losers of 8 basis points per year at the fund level. To put this economic magnitude in context, Jin and Scherbina (2011) estimate a one-time savings of 7 basis points from fund managers selling losers they have inherited from the managers they replaced.

The assumption that all observations are independent is unlikely to be true, however. To examine the robustness of our inference to relaxing this assumption, we estimate a pooled regression of 3-month ahead DGTW Alpha of team fund holdings on two indicator variables *Specialized Winner Sold* and *Specialized Loser Held* (with the intercept reflecting the omitted specialized winner held and specialized loser sold categories), allowing model errors to be correlated within same stock × period and same fund clusters. We allow for error clustering by stock × period because, as DGTW Alpha is defined at the stock × period level, a high (low) DGTW Alpha in a given stock × period will tend to produce correlated positive (negative) errors for that stock × period. We allow for error clustering by fund because fund-level skill may produce correlated errors across same-fund observations. Panel B of Table 9 reports the results. The coefficient estimates straightforwardly map into the mean DGTW Alphas reported in Panel A. For

[15] Odean reports a difference in excess returns of approximately 1% over the next 84 trading days, but he examines retail investors, does not adjust for the effects of size, value, and momentum, and uses a horizon longer than a quarter.

example, subtracting the coefficient of *Specialized Loser Held* of -0.304 from the coefficient of *Specialized Winner Sold* of -0.004 results in the differential of 0.300% reported in Panel A. With cluster-robust standard errors, the differences between the DGTW Alphas of *Specialized Winner Sold* and *Specialized Loser Held* remain statistically significant.

To address concerns that returns of specialized winners sold and specialized losers held are correlated cross-sectionally, we conduct calendar-time portfolio analyses as follows. At the beginning of a given month, we aggregate specialized winners sold across all team funds by considering one of two different weighting schemes: (i) popularity weighting (if there are 10,000 specialized winners sold across all funds, and one stock is a specialized winner sold by 100 funds, then that stock receives a weight of 100/10,000 = 1% in the popularity-weighted portfolio) and (ii) value weighting (if the total value of all winners sold during the prior period was $100 billion and one stock represented $1 billion of that amount, that stock receives a weight of 1/100 = 1% in the value-weighted portfolio). Next, we estimate a stock's DGTW Alpha contribution to the calendar-time portfolio that month as the product of the stock's weight and the stock's DGTW Alpha during that month; the monthly DGTW Alpha of the specialized winners-sold portfolio is the sum of the DGTW Alpha contributions across all specialized winners sold. We proceed in an analogous fashion to obtain the monthly DGTW Alphas of the specialized losers-held portfolio.[16]

Panel C of Table 9 reports the average monthly DGTW Alphas of the specialized winners-sold and specialized losers-held portfolios under popularity weighting (Column 1) and value weighting (Column 2). Similar to the results reported above, the DGTW Alphas of the specialized winners-sold portfolio are not impressive; specifically, they are neither statistically significant nor

[16] The Appendix provides further details on the construction of the calendar-time portfolios.

economically meaningful regardless of the weighting scheme. The specialized losers-held calendar-time portfolio deliver negative DGTW Alphas that are similar in magnitude to Panels A and B: -0.11% to -0.12% per month depending on the weighting scheme, translating into approximately -1.3% per year. Statistical significance, however, weakens under the conservative assumption that observations are only independent over time, yielding 249 independent observations. Only the popularity-weighted portfolio of specialized losers held has a weakly significant DGTW Alpha and the differences between specialized losers held and specialized winners sold are no longer significant.

The key take-away from Table 9 is that the tendency to hold on to specialized losers appears to impose a cost on team funds even before taking any tax inefficiencies or momentum effects into account – suggesting that vanity is costly.

IV. Conclusion

The past decades have seen a dramatic shift towards teamwork in investment decisions, which highlights the importance of understanding the benefits and costs of team decision making.

This paper focuses on the costs of team decisions. We develop behavioral hypotheses about when team decisions can amplify costly behavioral biases and present evidence consistent with the Vanity Hypothesis. According to the Vanity Hypothesis, allocating primary responsibility for a task (picking a stock, for example) to a team member will amplify disposition behavior, that is, that team member being too quick to declare victory and too slow to concede defeat. The intuition behind the Vanity hypothesis is that, in case of a good task outcome, the responsible team member receives utility both from a job well done and from looking good in front of others; by the same token, however, a poor task outcome requires admission of defeat to self and others.

As a methodological contribution to the literature examining fund teams, we identify team-managed funds as those with multiple named managers in Morningstar Direct (which is standard) but only one advisor as per the corresponding N-SAR or N-CEN filings (which is novel). The one-advisor requirement is important for testing behavioral hypotheses that require interaction among the team members. We estimate that, by exclusively relying on Morningstar Direct to identify team funds, one third of the resulting sample will be treated as team-managed even though there may be limited or no interaction among the listed managers.

Beyond the realm of mutual funds, our results suggest that the psychological costs of having to admit mistakes to peers is an important dimension of costs associated with team decision making, in addition to the more widely-studied dimensions of moral hazard and coordination.

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Tables

Table 1: Summary Statistics

This table reports summary statistics for a comprehensive sample of actively managed domestic U.S. equity funds between 1998 and 2018. The unit of observation is a fund × stock × period. The sample excludes all "inherited positions," that is, positions of solo and team funds that were initiated before any of the fund's current managers joined the fund. *Sale* is a dummy variable that equals 1 if the position was sold during the fund × stock × period and 0 otherwise. *Winner* is a dummy variable that equals 1 if the stock is a winner at the fund level (the stock trades above its fund-level volume-weighted average purchase price), and 0 otherwise. *Loser* is a dummy variable that equals 1 if the stock is a loser at the fund level (trades at or below its fund-level volume-weighted average purchase price), and 0 otherwise. *Specialized Winner* is an indicator that is one if only some members of the current team were present when the stock position was initiated and if the stock is a winner, and zero otherwise. *Specialized Loser* is an indicator that is one if the stock position was initiated when only some members of the current team were present and that trades at or below its volume-weighted average purchase price. *Collective Winner* is an indicator that is one if all members of the current team were present when the stock position was initiated and if the stock is a winner, and zero otherwise. *Collective Loser* is an indicator that is one if all members of the current team were present when the stock position was initiated and if the stock is a loser. *Solo Winner* is an indicator that is one if the stock position was initiated in the presence of the fund's current solo manager and if the stock is a winner, and zero otherwise. *Solo Loser* is an indicator that is one if the stock position was initiated in the presence of the fund's current solo manager and if the stock is a loser, and zero otherwise. 3-month ahead *DGTW Alpha* is the quarterly stock return minus the return of a value-weighted portfolio that matches the firm's market cap, book-to-market ratio, and prior 12-month return (see Daniel, Grinblatt, Titman, and Wermers, 1997). Panel A reports summary statistics of key variables whereas Panel B reports key summary statistics of unconditional selling probability for different winner and loser categories.

Panel A: Key Summary Statistics

	Mean	SD
Sale	0.342	0.474
Winner	0.632	0.482
Loser	0.368	0.482
Specialized Winner	0.077	0.267
Specialized Loser	0.034	0.180
Collective Winner	0.314	0.464
Collective Loser	0.189	0.392
Solo Winner	0.242	0.428
Solo Loser	0.145	0.352
3-month ahead DGTW Alpha [%]	-0.029	18.062
Observations	8,052,083	

Panel B: Category-level Unconditional Selling Probabilities

	Mean	SD	Count
Winner	0.341	0.474	5,089,669
Loser	0.345	0.475	2,962,414
Specialized Winner	0.350	0.477	620,086
Specialized Loser	0.332	0.471	271,290
Collective Winner	0.362	0.481	2,524,570
Collective Loser	0.360	0.480	1,525,183
Solo Winner	0.311	0.463	1,945,013
Solo Loser	0.327	0.469	1,165,941

Table 2: Disposition Effect Is Greater When Only Some Team Members Are Responsible

This table presents estimates for linear probability models of a fund's decision to sell versus hold a position. Observations are at the fund × stock × period level. Unless otherwise mentioned, variables are defined as in Table 1; the *Specialized Loser* category is omitted. The following control variables are included in all regressions: return since purchase interacted with winner and loser indicators and the stock volatility over the past 250 trading days interacted with winner and loser indicators, as well as fund- and portfolio size-fixed effects. Column (1) also includes the $\sqrt{Holding\ Period}$ by itself and interacted with fund fixed effects. Column (2) includes a full set of interactions between Holding Period fixed effects and fund fixed effects as well as interactions between Holding-Period fixed effects and Winner/Loser Indicator × Return since purchase. Standard errors are clustered by fund and period; t-statistics are reported in parentheses. The p-values compare the magnitude of disposition effects in absolute terms and across categories. For example, p-value(1-(2-3)) is the probability with which the estimated coefficient of *Specialized Winner* minus the difference of the coefficient of *Collective Winner* minus the coefficient of *Collective Loser* would be observed under the null hypothesis that the disposition effect in specialized positions equals the disposition effect in collective positions. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)
Specialized Winner (1)	0.029***	0.017***
	(5.83)	(3.74)
Collective Winner (2)	0.040***	0.026***
	(6.89)	(4.70)
Collective Loser (3)	0.032***	0.021***
	(6.76)	(4.93)
Solo Winner (4)	0.005	-0.010
	(0.61)	(-1.08)
Solo Loser (5)	0.003	-0.008
	(0.35)	(-0.84)
Constant	0.299***	0.331***
	(42.77)	(52.40)
Controls	Y	Y
Fund FE	Y	Y
Portfolio Size FE	Y	Y
$\sqrt{Holding\ Period}$ × Fund FE	Y	N
Holding Period FE × Fund FE	N	Y
Holding Period FE × Winner/Loser × Return	N	Y
p-value (2-3)	0.053	0.236
p-value (1-(2-3))	0.000	0.001
p-value (1-(4-5))	0.000	0.000
p-value (2-3-(4-5))	0.188	0.111
Observations	8,052,083	8,046,456
Adj-R^2	0.134	0.155

Table 3: Stronger Disposition Effect When Responsible Managers Have Relatively More Experience

This table presents estimates for linear probability models of a fund's decision to sell versus hold a position. Observations are at the fund × stock × period level. *High (Low) Experience Gap* indicates that the difference between the average industry experience of responsible managers and the average industry experience of their non-responsible colleagues is at or above (below) the median difference between the average industry experience of responsible managers and the average industry experience of their non-responsible colleagues across all specialized positions that period. Thus, for example, *Specialized Winner High Experience Gap* is an indicator that is one if (i) only some members of the current team were present when the stock position was initiated and if (ii) the stock is a winner and if (iii) the difference between the average industry experience of managers responsible for the stock and the average experience of their colleagues is at or above the median of the average difference across all specialized positions that period, and 0 otherwise. *Collective Winner*, *Collective Loser*, *Solo Winner*, and *Solo Loser* category variables are included in all regressions, but estimates are not reported to conserve space. The omitted category in Columns 1 and 2 is *Specialized Loser High Experience Gap*. The control variables are the same as in Table 2. Standard errors are clustered by fund and period; t-statistics are reported in parentheses. The p-values compare the magnitude of disposition effects in absolute terms and across categories. For example, p-value(1-(2-3)) is the probability with which the difference between the coefficient estimate of *Specialized Winner High Experience Gap* and the coefficient estimates of *Specialized Winner Low Experience Gap and Specialized Loser Low Experience* would be observed under the null hypothesis that the disposition effect in specialized positions did not depend on the experience gap. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)
Specialized Winner High Experience Gap (1)	0.037***	0.023***
	(7.15)	(4.69)
Specialized Winner Low Experience Gap (2)	0.019**	0.011
	(2.31)	(1.31)
Specialized Loser Low Experience Gap (3)	-0.001	-0.000
	(-0.19)	(-0.01)
Constant	0.300***	0.331***
	(40.07)	(46.67)
Controls	Y	Y
Fund FE	Y	Y
Portfolio Size FE	Y	Y
$\sqrt{Holding\ Period}$ × Fund FE	Y	N
Holding Period FE × Fund FE	N	Y
Holding Period FE × Winner/Loser × Return	N	Y
p-value (2-3)	0.003	0.087
p-value (1-(2-3))	0.008	0.048
Observations	8,052,083	8,046,456
Adj-R^2	0.134	0.155

Table 4: Stronger Disposition Effect in Unambiguous Winners and Losers

This table presents estimates for linear probability models of a fund's decision to sell versus hold a position. Observations are at the fund × stock × period level. *Clear Specialized Winner* is an indicator that is one if (i) only some members of the current team were present when the stock position was initiated and if (ii) the stock is a winner (its average price during the period is above its volume-weighted average purchase price at the fund level) and if (iii) all current team members consider the stock a winner based on the volume-weighted average purchase price across purchases for which they were present, and zero otherwise. *Ambiguous Specialized Winner* is an indicator that is one if only some members of the current team were present when the stock position was initiated and if the stock is a winner and if at least one current team member considers the stock a loser (based on the volume -weighted average purchase price across purchases for which that member was present), and zero otherwise. *Ambiguous Specialized Loser* is an indicator that is one if only some members of the current team were present when the stock position was initiated and if the stock is a loser at the fund-level and if at least one current team member considers the stock a winner (based on the volume -weighted average purchase price across purchases for which that member was present), and zero otherwise. *Solo Winner* and *Solo Loser* category variables are included in all regressions, but estimates are not reported to conserve space. The *Clear Specialized Loser* category variable is omitted. The remaining variables are defined as in Table 1 and the control variables are the same as in Table 2. Standard errors are clustered by fund and period; t-statistics are reported in parentheses. The p-values compare the magnitude of disposition effects in absolute terms and across categories. For example, p-value(1-(2-3)) is the probability with which the estimated coefficient of *Clear Specialized Winner* minus the difference of the coefficient of *Ambiguous Specialized Winner* minus the coefficient of *Ambiguous Specialized Loser* would be observed under the null hypothesis that the disposition effect in clear specialized positions equals the disposition effect in ambiguous specialized positions. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)
Clear Specialized Winner (1)	0.033***	0.020***
	(5.41)	(3.57)
Ambiguous Specialized Winner (2)	0.005	-0.006
	(1.09)	(-1.27)
Ambiguous Specialized Loser (3)	-0.008	-0.011**
	(-1.39)	(-2.05)
Collective Winner (4)	0.039***	0.024***
	(6.13)	(4.00)
Collective Loser (5)	0.031***	0.019***
	(6.22)	(4.27)
Constant	0.301***	0.333***
	(40.54)	(49.29)
Controls	Y	Y
Fund FE	Y	Y
Portfolio Size FE	Y	Y
$\sqrt{Holding\ Period}$ × Fund FE	Y	N
Holding Period FE × Fund FE	N	Y
Holding Period FE × Winner/Loser × Return	N	Y
p-value (2-3)	0.039	0.406
p-value (1-(2-3))	0.023	0.062
Observations	8,052,083	8,046,456
Adj-R^2	0.134	0.155

Table 5: Disposition Effect in Collective Positions Is Unaffected by Group Cohesiveness

This table presents estimates for linear probability models of a fund's decision to sell versus hold a position. Observations are at the fund × stock × period level. *High (Low) Cohesion* for a given collective position indicates that the team responsible for that position is in the top (bottom) half in terms of the length of time team members have managed the fund together across all funds that period. For example, *Collective Winner High Cohesion* is an indicator that is one if (i) all members of the current team were present when the stock position was initiated and if (ii) the stock is a winner and if (iii) the team is classified as a high cohesion team, and zero otherwise. *Solo Winner* and *Solo Loser* category variables are included in all regressions, but estimates are not reported to conserve space. The *Specialized Loser* category variable is omitted. The remaining variables are defined as in Table 1 and the control variables are the same as in Table 2. Standard errors are clustered by fund and period; t-statistics are reported in parentheses. The p-values compare the magnitude of disposition effects in absolute terms and across categories. For example, p-value(2-3) is the probability with which the difference between the coefficient estimate of *Collective Winner High Cohesion* and the coefficient estimate of *Collective Loser High Cohesion* would be observed under the null hypothesis that there is no disposition effect in collective positions of high-cohesion teams. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)
Specialized Winner (1)	0.029***	0.017***
	(5.84)	(3.74)
Collective Winner High Cohesion (2)	0.045***	0.030***
	(6.45)	(4.48)
Collective Loser High Cohesion (3)	0.035***	0.025***
	(5.89)	(4.54)
Collective Winner Low Cohesion (4)	0.036***	0.023***
	(6.14)	(4.03)
Collective Loser Low Cohesion (5)	0.029***	0.017***
	(5.71)	(3.67)
Constant	0.300***	0.331***
	(43.01)	(52.79)
Controls	Y	Y
Fund FE	Y	Y
Portfolio Size FE	Y	Y
$\sqrt{Holding\ Period}$ × Fund FE	Y	N
Holding Period FE × Fund FE	N	Y
Holding Period FE × Winner/Loser × Return	N	Y
p-value (2-3)	0.066	0.386
p-value(4-5)	0.122	0.196
p-value (2-3-(4-5))	0.483	0.839
Observations	8,052,083	8,046,456
Adj-R^2	0.134	0.155

Table 6: Disposition Effect Is Unaffected by Expert Arrival

This table presents estimates for linear probability models of a fund's decision to sell versus hold a position during a two-year window centered on the period during which one or more new team members arrive. We also require that the incumbent manager(s) as well as at least one of the new manager(s) stay on for the entire year after the arrival-period. Observations are at the fund × stock × period level. *Year After Expert Arrival* indicates that the fund × stock × period is within the first year after such an event. *Quarter Before Expert Arrival* indicates that the fund × stock × period is in the quarter just prior to expert arrival. *>1 Quarter Before Expert Arrival* indicates that the fund × stock × period is more than one quarter prior to expert arrival. *Winner and Loser* are defined as in Table 1. For example, *Winner Year After Expert Arrival* is an indicator that is one if (i) the stock is a winner and (ii) the period is within the year after expert arrival, and 0 otherwise. We omit the *Loser >1 Quarter Before Expert Arrival* category. The control variables are the same as in Table 2. Standard errors are clustered by fund and period; t-statistics are reported in parentheses. The p-values compare the magnitude of disposition effects in absolute terms and across categories. For example, p-value(1-2) is the probability with which the difference between the coefficient estimate of *Winner Year After Expert Arrival* and the coefficient estimate of *Loser Year After Expert Arrival* would be observed under the null hypothesis that there is no disposition effect in year after expert arrival. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)
Winner Year After Expert Arrival (1)	0.026	0.029
	(1.31)	(1.41)
Loser Year After Expert Arrival (2)	0.032*	0.042**
	(1.76)	(2.16)
Winner Period Before Expert Arrival (3)	0.046	0.038
	(1.38)	(1.19)
Loser Period Before Expert Arrival (4)	0.129***	0.122**
	(2.79)	(2.57)
Winner>1 Period Before Expert Arrival (5)	0.009	0.005
	(0.67)	(0.36)
Constant	0.269***	0.298***
	(23.31)	(30.87)
Controls	Y	Y
Fund FE	Y	Y
Portfolio Size FE	Y	Y
$\sqrt{Holding\ Period}$ × Fund FE	Y	N
Holding Period FE × Fund FE	N	Y
Holding Period FE × Winner/Loser × Return	N	Y
p-value (1-2)	0.583	0.197
p-value (3-4)	0.022	0.016
p-value ((1-2)-(3-4))	0.026	0.034
p-value (1-2-5)	0.278	0.180
p-value (3-4-5)	0.012	0.014
Observations	167,184	167,034
Adj-R^2	0.183	0.210

Table 7: Results in Switcher Sample Are Similar to Baseline Results

This table presents estimates for a linear probability model of a fund's decision to sell versus hold a position. To be included, a fund has to switch from a solo management structure to a team structure or vice versa at least once during the sample period. Observations are at the fund × stock × period level. All variables are defined as in Table 2. The *Specialized Loser* category is omitted. Standard errors are clustered by fund and period; t-statistics are reported in parentheses. The p-values compare the magnitude of disposition effects in absolute terms and across categories. For example, p-value(1-(2-3)) is the probability with which the estimated coefficient of *Specialized Winner* minus the difference of the coefficient of *Collective Winner* minus the coefficient of *Collective Loser* would be observed under the null hypothesis that the disposition effect in specialized positions equals the disposition effect in collective positions. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)
Specialized Winner (1)	0.030***	0.018***
	(5.80)	(3.73)
Collective Winner (2)	0.042***	0.028***
	(7.00)	(4.85)
Collective Loser (3)	0.030***	0.020***
	(6.30)	(4.54)
Solo Winner (4)	0.007	-0.007
	(0.83)	(-0.87)
Solo Loser (5)	0.002	-0.009
	(0.22)	(-1.03)
Constant	0.290***	0.323***
	(41.29)	(51.02)
Controls	Y	Y
Fund FE	Y	Y
Portfolio Size FE	Y	Y
$\sqrt{Holding\ Period}$ × Fund FE	Y	N
Holding Period FE × Fund FE	N	Y
Holding Period FE × Winner/Loser × Return	N	Y
p-value (2-3)	0.018	0.106
p-value (1-(2-3))	0.000	0.017
p-value (1-(4-5))	0.000	0.004
p-value (2-3-(4-5))	0.206	0.178
Observations	5,402,681	5,399,007
Adj-R^2	0.147	0.165

Table 8: Results in Conditioned Team Fund Sample

This table presents estimates for linear probability models of a fund's decision to sell versus hold a position. For a fund × stock × period to be included as an observation, the fund has to be a team fund during the period and the fund × period needs to contain at least one specialized position (a position that was initiated when only some members of the current team were present) and at least one collective position (a position that was initiated when all members of the current team were present). All variables are defined as in Table 2 (note that *Solo Winner* and *Solo Loser* categories/observations are not applicable because of the team fund requirement). The *Specialized Loser* category is omitted. Standard errors are clustered by fund and period; t-statistics are reported in parentheses. The p-values compare the magnitude of disposition effects in absolute terms and across categories. For example, p-value(1-(2-3)) is the probability with which the estimated coefficient of *Specialized Winner* minus the difference of the coefficient of *Collective Winner* minus the coefficient of *Collective Loser* would be observed under the null hypothesis that the disposition effect in specialized positions equals the disposition effect in collective positions. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)
Specialized Winner (1)	0.024***	0.010***
	(6.77)	(3.21)
Collective Winner (2)	0.028***	0.018***
	(6.33)	(4.00)
Collective Loser (3)	0.024***	0.015***
	(5.87)	(3.94)
Constant	0.304***	0.330***
	(87.35)	(113.29)
Controls	Y	Y
Fund FE × Period FE	Y	Y
Portfolio Size FE	Y	Y
$\sqrt{Holding\ Period}$ × Fund FE	Y	N
Holding Period FE × Fund FE	N	Y
Holding Period FE × Winner/Loser × Return	N	Y
p-value (2-3)	0.338	0.486
p-value (1-(2-3))	0.000	0.054
Observations	2,143,880	2,143,880
Adj-R^2	0.325	0.327

Table 9: Team Disposition Effects In Specialized Positions Are Costly

Panel A presents summary statistics for 3-month ahead DGTW Alphas for *Specialized Winners Sold* and *Specialized Losers Held* by team funds during the 1998-2018 sample period. P-value (W-L) is the probability that the observed future performance difference between *Specialized Winners Sold* and *Specialized Losers Held* would be observed under the null of equal performance, assuming that all observations are independent. Panel B reports estimates of a pooled regression model of 3-month ahead DGTW Alpha on the category indicator variables *Specialized Winner Sold* and *Specialized Loser Held,* omitting the *Specialized Winner Held and Specialized Loser Sold* categories whose average equals the constant term, allowing model errors to be correlated by stock × period and by fund. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

Panel A:

	3-Month Ahead DGTW Alpha [%]
Specialized Winner Sold (W)	0.050*
p-value	0.093
Nobs	205,699
Specialized Loser Held (L)	-0.251***
p-value	0.000
Nobs	175,039
(W-L)	0.300***
p-value	0.000
Nobs	380,738

Panel B:

Dependent Variable	3-Month Ahead DGTW Alpha [%]
Specialized Winner Sold (1)	-0.004
	(-0.07)
Specialized Loser Held (2)	-0.304**
	(-2.35)
Constant	0.053
	(1.17)
p-value (1-2)	0.044
Observations	858,289
Adj-Rsq	0.000
SE Clustering	Stock x Period, Fund

Panel C:

	Monthly [%] DGTW Alpha of Calendar-Time Portfolios	
	(1)	(2)
	Stocks Weighted by Popularity	Stocks Weighted by Value
Specialized Winner Sold (W)	-0.008	0.020
p-value	0.883	0.776
Nobs	249	249
Specialized Loser Held (L)	-0.111*	-0.123
p-value	0.099	0.301
Nobs	249	249
W-L	0.102	0.144
p-value	0.264	0.316
Nobs	249	249

Online Appendix for Vanity in Teams

Data on Individual Managers of Active U.S. Stock Funds

We start with all US-domiciled open-end funds in the Morningstar Direct database spanning the period 1998 to 2018 (as of March 2020). To limit our analysis to U.S. equity funds, we only include funds with the Morningstar Category codes US Fund Large-Cap Value, US Fund Large-Cap Growth, US Fund Large-Cap Blend, US Fund Mid-Cap Value, US Fund Mid-Cap Growth, US Fund Mid-Cap Blend, US Fund Small-Cap Value, US Fund Small-Cap Growth, and US Fund Small-Cap Blend. We identify (and exclude) a fund as an index fund if Morningstar assigns an index flag of "Yes" or when the fund name contains the string "index"/ "indx". Morningstar assigns *fundid* as unique id to each fund and *secid* as a unique id to each fund-share class. These two identifiers are never reused. Morningstar also provides *ticker* and *cusip* at fund share class level. We keep *fundid, secid, ticker,* and *cusip* in our raw Morningstar dataset.

For each fund-share class (*secid*), Morningstar provides a variable called *managerhistory* which includes the names of all fund managers that have ever worked for the fund since the fund's inception and the respective timelines of the managers. For example, for *fundid*=FS00008O50, *managerhistory* = [2011-12-15 -- 2016-03-18] Walter B. Todd;[2011-12-15 -- 2013-07-29] Michael W. Nix. We verify that the manager information reported by Morningstar is consistent across share classes. The information in *managerhistory* allows us to obtain the exact composition of the management team for a given fund and period. In less than 1% of the cases, Morningstar just provides a "Team" label rather than manager-level information. We exclude these cases because the manager start and end dates are crucial for our analysis.

Adding Information on Fund Holdings

Adding information on the stocks a fund holds involves a five-step process. First, we merge our Morningstar data set at the share-class level on either *ticker* or *cusip* with CRSP's mutual fund database to get CRSP's *crsp_fundno*, a unique fund identifier. Out of 14,838 share-class observations, 88% can be matched on either ticker or cusip; we discard the remaining observations. This results in a data set of 3,991 actively managed domestic U.S. equity funds.

In a second step, we merge the data with MFLINKS on the *crsp_fundno* to add the *WFICN* identifier to our data set. A *WFICN* is not available for 1,434 funds. This results in a data set of 2,557 actively-managed U.S. equity funds.

In a third step, we merge our data on *WFICN* with the S12 Thomson Reuters database for the period 1998 to 2018 to get information on the fund's holdings. We limit our sample to fund level reports with a maximum gap between two consecutive reports of 6 months. This is to account for semi-annual reporting of holdings by mutual funds in years prior to 2004. We are able to find holdings information for 95% of the funds.

Fourth, we merge our data with CRSP's U.S. stock database to add information on fund holdings such as stock prices, corporate actions/adjustment factors, market capitalization, and book-to-market ratio. We clean and prepare the holdings data by applying the filters used by Frazzini (2006).

Fifth, we remove inherited positions. We identify an inherited position as a stock position that was initiated by one or more managers who are no longer part of the fund's management. Such positions account for 17% of all fund × stock × periods. The resulting sample comprises 10,992,134 fund × stock × periods.

Adding Information on Fund Advisory Structure

We gather fund advisory information from N-SAR filings for 1998-2017 and from N-CEN filings for 2018.[17] N-SAR filings are filed semi-annually, and N-CEN filings are filed annually by all mutual funds.

[17] SEC rescinded Form N-SAR on June 1, 2018 (https://www.sec.gov/investment/investment-company-reporting-modernization-faq) and required registered investment companies to file Form N-CEN from 2018 onwards on an annual basis, no later than 75 days from the close of the fiscal year. Form N-CEN provides fund portfolio details as well as advisory and sub-advisory information.

We download these filings from the SEC's Electronic Data Gathering, Analysis, and Retrieval EDGAR system. Filings provide advisor names, sub-advisor names, and whether the advising entity is an advisor or a sub-advisor. The filings do not provide any unique identifier that can directly be matched with CRSP MFDB or Morningstar or Thomson Reuters. We thus follow Moreno, Rodriguez, and Zambrana (2018) in parsing and cleaning N-SAR information as well as linking it to CRSP's MFDB. We merge N-CEN filings with CRSP's MFDB using either ticker or fund name. Using MFLINKS, we merge the CRSP-NSAR-NCEN data with fund holdings. We are able to assign information on advisory structure to all the fund holdings.

The table below breaks down the fund × stock × period sample in terms of number of fund managers listed in Morningstar Direct and in terms of number of advisors reported in the corresponding EDGAR filing.

	Funds with	
Advisory structure	≥ 2 managers listed in MS	1 manager listed in MS
1 advisor	4,941,129	3,110,954
1 advisor and 1 sub-advisor	1,195,062	297,141
Multiple advisors or 1 advisor and multiple sub-advisors	1,304,106	106,363
Only subadvisor(s) reported	26,516	10,863
	7,466,813	3,525,321

Our main sample of 8,052,083 fund x stock x periods across 2,090 unique funds consists of 4,941,129 "bona-fide" team observations and 3,110,954 "bona-fide" solo observations.

Constructing DGTW Alphas of Calendar-Time Portfolios of Losers Held and Winners Sold

Losers-Held Portfolio

Team funds report their holdings at different frequencies (mostly quarterly, but sometimes at lower or higher frequencies) and also have different fiscal years. Given this heterogeneity in reporting dates, we form monthly calendar-time portfolios of losers held as follows. For a given month, we identify all team funds that report their holdings at the end of the prior month and classify stocks that trade below their volume-weighted average purchase price as losers held. For a given loser held by a given fund at that time,

we assume the stock to be a loser held until the fund's next reporting date. This procedure yields a list of losers held across all funds at the end of a given month; a stock will show up as many times on this list as many funds hold it as a loser. We then use one of two weighting schemes to aggregate losers for a given month: (1) popularity weighting and (2) value weighting.

The popularity weight of a given stock in the losers-held portfolio at the beginning of a given month is the number of funds for whom the stock is a loser held at the end of the prior month, divided by the total number of losers held across all funds at the end of the prior month.

The value weight of a given stock in the losers-held portfolio at the beginning of a given month is the dollar value invested in the stock across all the funds for whom it is classified as a loser held at the end of the prior month (using, for a given fund, the reported shares held at the most recent reporting date multiplied by the stock's price at the end of the prior month), divided by the dollar value invested in all stocks classified as losers held across all funds at the end of the prior month.

For each stock in the losers-held portfolio, we estimate its contribution to the DGTW Alpha of the portfolio as the product of its weight in the portfolio at the beginning of the month and its DGTW Alpha during that month; as before, we winsorize DGTW Alphas at the 1% and 99% level. The DGTW Alpha of the losers-held portfolio is then sum of the DGTW Alpha contributions across all portfolio constituents.

Winners-Sold Portfolio

For a given month, we identify all team funds that report their holdings at the end of the prior month. For a given fund and month, a stock is classified as a winner sold if the fund sold the stock (as inferred from a reduction in the number of shares held, adjusted for splits and spinoffs) and if the stock (would have) traded at or above its volume-weighted average purchase price. A winner sold thus identified is assumed to remain part of the aggregate winners-sold portfolio for three months.[18] This procedure yields a list of

[18] Unlike losers held, for which the next reporting date provides a natural update of whether a stock continues to be held as a loser, there is no natural horizon over which to measure the performance of a winner sold. We choose a quarterly horizon as this is the frequency at which most funds report their holdings. Choosing longer horizons may result in the misclassification of a winner sold (e.g., if a fund later repurchases the stock).

winners sold across all funds at the end of a given month; a stock will show up as many times on this list as many funds sell it as a winner.

Similar to the losers-held portfolios, we aggregate winners sold into portfolios using popularity weighting and value weighting. The popularity weight of a given stock in the winners-sold portfolio at the beginning of a given month is the number of funds which sold that stock as a winner over their last reporting period, divided by the total number of winners sold across all funds at the end of the prior month. The value weight of a given stock in the winners-sold portfolio at the beginning of a given month is the dollar value sold in the stock across all the funds for whom it is classified as a winner sold (for a given fund and reporting period, this is the number of shares sold during the last reporting period times the stock's price at the end of the last reporting period), divided by the dollar value sold in all stocks classified as winners sold over the last reporting period. DGTW Alphas are calculated analogously to the losers-held portfolios.