Do Teams Alleviate or Exacerbate Behavioral Biases? Evidence from Extrapolation Bias in Mutual Funds*

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Abstract

Whether teams attenuate or exacerbate the behavioral biases which are pervasive at the individual level is an open question. To address this question, we use the mutual fund industry as a laboratory. Our focus is on how return extrapolation, a bias in investor behavior that has received considerable attention in recent work, is transmitted from individual fund managers to the team-managed funds they join. We show that teams heavily attenuate the influence of extrapolation bias on funds' trading behavior. Additional analysis reveals that this attenuation is not due to differences in investment experience, compensation contracts, workload, and investment styles between solo-managed and team-managed funds. Rather, our evidence suggests that the elicitation of team members' inner cognitive reflection can be responsible for teams' reduction in behavioral biases. Our results highlight the attenuation of the extrapolation bias as a potential benefit of team-based asset management.

Keywords: Cognitive biases, Extrapolation, Heuristics, Mutual funds, Teams
JEL classifications: G23, G41, D91

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1. Introduction

Organizations around the world entrust teams with a large variety of tasks. These tasks range from writing and enforcing the laws of a country, to setting its monetary and fiscal policies, or designing and implementing mechanisms of corporate governance. Despite the central role of teams in many areas of decision-making, we still know little about the way in which teams influence the quality of decisions. Most of the studies on teams are in experimental settings (e.g. Shaw 1932; Hill 1982; Kocher and Sutter 2005; Cooper and Kagel 2005; Kagel and McGee 2016). In these settings, teams' performance relative to individuals is highly dependent on the specific nature of the task at hand,[1] thus leaving open the question of how teams fare when dealing with complex real-world problems.

Studies have pointed out team members' ability to identify each other's judgment mistakes as an important determinant of the teams' effectiveness and performance. However, many studies in cognitive psychology show that in key areas of decision-making individuals share common intuitive rules of thumb, called heuristics, which lead to systematic and widespread mistakes (e.g. Tversky and Kahneman 1974; Kahneman 2003). The shared use of common heuristics can generate similar mistakes among team members and hence hinder teams' ability to identify such mistakes. In fact, to the extent that team interactions induce groupthink (Janis 1972; Bénabou 2013), teams can even exacerbate rather than reduce the impact of common judgment biases on decision-making. Overall, whether teams attenuate or exacerbate the behavioral biases which are pervasive at the individual level is an open question.

In this study, we investigate this question by using field data, as we analyze teams in the mutual fund industry. The choice of risky investments that mutual fund managers and teams confront is a classical example of judgment under uncertainty, an area of decision-making

[1]For instance, teams make better decisions than their individual members in learning and concept-attainment tasks, while statistical teams (i.e., counterfactual teams whose decisions are simply averages of individual team participants' views) outperform actual teams in tasks involving brainstorming and the lack of a clear answer. See for example Hill (1982). See also Kerr et al. (1996) and Charness and Sutter (2012) for more recent surveys.

in which heuristics and judgment biases are known to be pervasive at the individual level.[2] Therefore, investment decisions in mutual funds represent a natural venue to research how judgment biases affect teams' decisions. Moreover, while mutual fund teams have become the prevalent organizational form in the asset management industry over the last few decades, there is also a substantial number of mutual funds that feature a single portfolio manager, and many instances in which a researcher can compare how the same mutual fund managers make investment decisions individually as well as members of a team. By performing a comparison of behavioral biases in team-managed funds with the biases that team members display in their solo-managed funds, we can measure the role of teams as an organizational arrangement, while controlling for differences between the managers involved in solo-managed funds and the managers who operate in team-managed funds.

To study whether teams of mutual fund managers attenuate or exacerbate their members' own tendencies to make investment mistakes, we concentrate on the extrapolation bias. This bias, which studies have linked to the representativeness heuristic (e.g., Barberis 2018), leads investors to predict higher future asset returns following high past returns despite the weak empirical evidence of autocorrelation in returns. Our focus on extrapolation stems from the considerable attention that it has received in recent theoretical and empirical work.[3]

We measure extrapolation in mutual funds with panel-level regressions of active quarterly changes in fund holdings on past-year returns.[4] We then show that extrapolation leads to investment outcomes that are systematically suboptimal from an asset manager's standpoint. Specifically, compared to contrarian managers, that is, managers who do not buy stocks following good returns but rather sell these stocks, extrapolative fund managers (i) achieve

[2]See Benartzi and Thaler (2001), Malmendier and Nagel (2011), Bordalo et al. (2018a).

[3]The seminal work on heuristics and biases is Tversky and Kahneman (1974). Recent surveys of that work are Kahneman (2002) and Kahneman (2011). Work on extrapolation includes Greenwood and Shleifer (2014), Barberis et al. (2015), Barberis et al. (2018), Cassella and Gulen (2018), Jin and Sui (2019), Da et al. (2020).

[4]Active changes in portfolio weights correct for the mechanical effect of past returns on weights. Regressions include other observable stock characteristics that can drive funds' portfolio choice. To distinguish irrational extrapolative behavior from standard momentum trading (Jegadeesh and Titman 1993), we also repeat our main analysis by excluding momentum stocks. Our main result remains unchanged.

systematically worse future risk-adjusted and style-adjusted returns, (ii) are less likely to become top performers within their style category, and (iii) receive less inflows and hence their funds grow less. Given the evidence in the literature that fund managers' compensation and career are strongly influenced by performance, ranking, size, and flows (e.g., Ma et al. 2019); these results indicate that fund managers who extrapolate past returns are systematically worse off than fund managers who do not extrapolate. This set of findings indeed supports the interpretation of extrapolation among fund managers as a bias.

In the second and central step of the analysis we compare the extent to which extrapolation affects money management in solo funds as opposed to team-managed funds. We argue that a simple comparison of extrapolation bias in solo versus team-managed does not clearly identify the role of teams in decision biases. The reason is that there can be key compositional differences between the set of fund managers who work in teams as opposed to those who work independently. For instance, theory work by Huang et al. (2019) suggests that fund managers who operate in teams can be fundamentally different from the managers of solo-managed funds along dimensions such as skill. To address this issue, we take advantage of one key feature of our empirical setting; namely, that there are team-managed funds whose members have all at some point in their career managed one or more funds by themselves. Using this sample of managers and funds, we can test whether teams alleviate or exacerbate biases compared to individual asset managers in a setting where the compositional differences between team-managed and solo-managed funds are absent.

Our main contribution is to show that teams largely attenuate the extrapolation bias in investment decisions. In particular, in regressions of team-managed' degree of extrapolation on team members' average individual-level extrapolation, teams attenuate the extrapolative behavior that their members exhibit at the individual level by about 80%. This result is robust to various ways of estimating managers' and teams' extrapolative behavior and to the inclusion of a large set of fixed effects and controls that help to account for differences in styles among team-managed funds.

Our evidence that biases exhibited at the individual level are reduced in teams could in principle be due to measurement error in the estimation of fund managers' extrapolative behavior. However, we show that measurement error does not drive our results. First, our results are robust to the use of an IV estimator in the spirit of Jegadeesh et al. (2019). After showing via simulations that this IV procedure generates unbiased coefficient estimates in our regressions, we use the IV to investigate whether our results are due to measurement error. We find little evidence that this is the case. Second, if measurement error was behind our result, one would expect to find a similar attenuation of individual-level behavior both in teams composed of extrapolators and in teams composed of contrarians, because both these characterizations of fund managers' trading behavior rely on an identical estimation procedure. In contrast, we find that only fund managers' individual extrapolative behavior is attenuated in teams, while there is little evidence that contrarian behavior is attenuated at all. Overall, the reduction of extrapolation bias that we observe when comparing individual-level decision-making with the investment decisions of teams is an empirical finding that is not due to measurement error.

Whereas our empirical results are consistent with a positive role of teams for decision biases, other explanations are also possible that do not hinge on teams per se. We identify four such alternative explanations. First, an experiential learning story could be considered (De Groot and de Groot 1978, Levitin 2002), whereby managers accrue increased expertise during their solo-management years, and such expertise leads to the reduction in extrapolation bias that is observed when these managers operate in the team. We investigate this alternative explanation using asset management tenure as a proxy for managerial experience. We find no evidence of a conditioning role of managers' accrued experience for team-level biases. As a matter of fact, the documented attenuation of extrapolation bias occurs both in teams of "learners", i.e., teams whose members reduce their extrapolative behavior in the later years of their solo-managed decisions, and in teams of "non-learners", i.e., teams whose members maintain a similar extrapolative behavior in the earlier and in the later years of their solo-

managed decisions. We therefore conclude that experiential learning is unlikely to generate our result.

Second, mutual fund managers' compensation structures could be systematically different in solo-funds as opposed to team-managed funds. In particular, if compensation was more sensitive to performance in team-managed as opposed to solo-managed funds, fund managers' investment mistakes when operating in teams becomes more costly. More high-powered incentives in team-based assignments could lead managers to shift from heuristic judgment to more effortful deliberation, and such as shift has been linked to a reduction of judgment biases (e.g., Kahneman 2003). Such a bias reduction would be consistent with our evidence, but it would not be due to teams as an organizational arrangement. To investigate this explanation formally, we follow Ma et al. (2019) and hand-collect data on managerial compensation from funds' prospectus filings that are submitted yearly to the SEC. We find that the compensation structures do not differ when managers operate individually or in a team. The only difference concerns the beneficial ownership of fund's shares. We find such ownership to be higher when managers operate alone compared to when they operate in a team. This result implies that, if anything, managerial compensation should lead to weaker, rather than stronger, biases in solo-managed decisions than in teams. Therefore, managerial compensation is not a plausible explanation for our main finding.

Third and related, managers may operate under substantial lower workload when they engage in a team-managed as opposed to a solo-managed fund. A lower workload could in turn reduce managers' reliance on heuristic rules (Stanovich and West 2008). However, such a reduction would not be due to team-decision-making and could have been achieved just as well by a reduction in solo managers' workload. To investigate this explanation, we use the number of stocks that a manager oversees across all the portfolios he manages or co-manages as a proxy for workload. We indeed find that managerial workload declines when they manage as part of a team. However, we find no evidence of a conditioning role of changes in workload for the reduction of biases in team-based asset management. Thus,

although plausible, workload reduction does not help reconcile our finding.

Finally, we consider a "style-migration" alternative explanation for our finding, whereby the attenuation of biases that is observed in teams could be the result of systematic changes in investment styles when mutual fund managers transition from solo management to team management. However, such style migration is in practice not very common, and we find little evidence that it explains our main result. All in all, none of the four alternative explanations we have considered helps to explain our result. We therefore conclude that it is reasonable to interpret our main result as stemming from the impact of teams on decision-making.

In the last part of the paper, we ask what mechanism can generate the documented reduction in judgment biases when managers operate in teams. Like the literature, we argue that cognitive mistakes such as extrapolation can stem from the influence that heuristics and intuitive judgment, as opposed to reasoning and effortful deliberation, exert on managers' investment decisions. We use insights from earlier work by Kahneman (2000), Frederick (2005), and Stanovich and West (2008) to propose that teams may be able to reduce the influence of extrapolation on decision-making by creating cues that elicit effortful deliberation in their members and that thus abate decision biases. We argue that teams can provide two types of cues. First, team members may engage in deeper cognitive reflection and realize their own cognitive mistakes simply by virtue of having to communicate and share their views with other team members. We call this cue the internal reflection hypothesis. Second, team members can become aware of their biases through the scrutiny offered by the other members of the team. We refer to this cue as the external screening hypothesis.

We argue that these two distinct mechanisms can be told apart in the data. In particular, heterogeneity in the extrapolative behavior within a team is likely to facilitate team members' ability to screen the judgment mistakes of their fellow team members. On the contrary, the attenuation of extrapolation bias that is due to internal reflection depends less on team members' heterogeneity, because it stems from fund managers engaging in greater introspection when they act within a team as opposed to when they act alone. Using the

different mediating role that heterogeneity in the extrapolative behavior has for internal reflection as opposed to external screening, we test which of these two mechanisms is better supported by the data. Our tests show evidence that is more in line with the internal reflection hypothesis and less supportive of the external screening hypothesis.

Our paper adds to three separate strands of literature. First, we add to the literature in economics that studies the impact of teams and groups on decision-making empirically. Most of the empirical evidence on the impact of teams on decisions is obtained in an experimental setting. In this setting, the focus is often on whether teams or individuals' participation to groups help improve rational self-interested choice in strategic interactions (Bornstein and Yaniv 1998; Kocher and Sutter 2005; Cooper and Kagel 2005; Charness et al. 2007b; Sutter 2009). Some experimental work concerning teams and judgment biases in non-strategic games exists, but the evidence of whether teams help reduce such biases is rather mixed (Kerr et al. 1996).[5] A few recent working papers have begun to investigate the role of teams outside of the laboratory. Like our work, these papers too use the mutual fund industry as an empirical setting. However, these papers address questions that are different from the one we investigate, namely, whether teams afford superior investment performance by pooling team members' heterogeneous skills, ideas, or information (Bär et al. 2011; Prather and Middleton 2002; Fedyk et al. 2017; Patel and Sarkissian 2017; Harvey et al. 2020), and what is the interplay between managerial structures and incentives (Patel and Sarkissian 2021). Our paper contributes to the literature by providing field evidence that teams can achieve superior decisions in non-strategic settings not only due to the pooling of resources of information, but also because teams help reduce the effect of heuristics and biases on decision-making.

Second, our study provides considerable new evidence concerning the incidence of heuristics and biases in the asset management industry. The literature offers a mixed view of fund managers. Some studies regard asset managers as "smart-money", that is, sophisticated

[5]For more recent work in this area see for instance Charness et al. (2007a), Charness et al. (2010).

market participants who are less prone to heuristics and behavioral biases than less sophisticated investors (e.g. Frazzini and Lamont 2008). In contrast, other studies argue that fund managers can also make systematic investment mistakes that could arise due to bounded rationality and heuristic rules. Our contribution to this line of work is twofold. First, we quantify the extent to which fund managers' trading behavior conforms to the extrapolation bias (e.g. Greenwood and Shleifer 2014; Barberis et al. 2015; Barberis 2018). We show that there is large heterogeneity in return extrapolation across funds. Second, we address the deeper question of whether the adoption of teams in the asset management industry reduces or exacerbates the effect of extrapolation bias on financial decisions. Our findings indicate that teams can play an important role in reducing the incidence of behavioral biases on trading behavior and investment performance.

Third, our study relates to the growing empirical literature that examines the role of diversity for team decision-making and performance (e.g. Adams and Ferreira 2009; Gompers and Wang 2017; Bernile et al. 2018). Most of the literature uses team members' heterogeneity with respect to observable characteristics such as gender, ethnicity, experience, or expertise as an indirect gauge into the underlying heterogeneity in team members' information, preferences, or beliefs. Such an approach requires to take a stand on the characteristics that define diversity. Our approach to measuring heterogeneity in beliefs within a team is different. We abstract from the issue of having to select what characteristics would correlate the most with heterogeneity in extrapolation bias among team members. Rather, we gain a direct insight into team members' diversity in extrapolative beliefs by analyzing these members' trading behavior when they manage a fund individually. We then ask a narrow but focused question on how heterogeneity in extrapolation observed at the individual level affects extrapolative behavior in a team. Our results indicate that such a heterogeneity may not be a necessary condition for teams to successfully reduce the impact of extrapolation bias on decision-making.

The remainder of the study is organized as follows. The descriptions of the data and

the empirical methodology are in Section 2. In Section 3, we present evidence of the negative relationship between funds' extrapolative behavior and investment performance that is consistent with the interpretation of extrapolation as a cognitive bias that leads to worse investment outcomes. In Section 4, we investigate the extent to which teams attenuate or exacerbate return extrapolation in mutual funds. We then perform several robustness checks in Section 5. Section 6 empirically explores the mechanism through which teams can help reduce cognitive biases. Section 7 concludes.

2. Data and Empirical Methodology

In this section, we present the data and our methodology.

2.1. Construction of the main dataset

Our analysis focuses on US active domestic equity mutual funds. We use five data sources for our analysis: CRSP's monthly stock file (stock prices and returns), COMPUSTAT's annual file (accounting-based stock characteristics), Morningstar Direct (fund manager information and fund styles), CRSP's mutual fund data (fund holdings and fund characteristics), and Thomson Reuters (fund holdings).

In our first step of the analysis, we merge the CRSP and Morningstar Direct mutual fund databases. The merger is based on recent work by Berk and van Binsbergen (2015), Pástor et al. (2015), and Kim (2020). While a detailed description of the merger is in Appendix A, we provide a brief summary here. We first clean CRSP and Morningstar separately by following Pástor et al. (2015) and Kim (2020). The CRSP mutual fund description file is our master file. In order to match Morningstar to CRSP, we use two different matching approaches. In the first approach, we use the CUSIP or ticker as in Pástor et al. (2015) and Kim (2020) to match the two files. We then follow Berk and van Binsbergen (2015) and correct for potential errors in the merger that are due to the reuse of tickers and CUSIP codes in these databases. The second approach complements the first in that we perform a second

merger between CRSP and Morningstar based on year, month, monthly fund return, and monthly total net asset value (Berk and van Binsbergen 2015). The final dataset represents 82% of our initial universe of CRSP US active domestic equity mutual funds.

In the second step of the analysis, we match these funds to their respective holdings data in Thomson Reuters (s12 holdings file for mutual funds) and CRSP (s12 mutual fund holdings database). From 1980 to 2008, we use Thomson Reuters and after that period we use CRSP. The CRSP mutual fund holdings data only start in 2003 and its coverage is slightly smaller than Thomson Reuters until 2008. However, after 2008, CRSP has better coverage than Thomson Reuters, so we switch to them (Shive and Yun 2013). We link Thomson Reuters and CRSP using the MFLINKS dataset from the Wharton Research Data Services.

In the third step, using stock CUSIP numbers from CRSP, we link mutual funds' holdings to the stock-level information (prices, returns, book-to-market, profitability, and investments) contained in the merged CRSP-COMPUSTAT database. We consider the universe of stocks with codes 10 and 11 that trade on the NYSE, NASDAQ, and the AMEX, and we exclude stocks trading below \$5. Finally, we link each mutual fund to their respective managers. This linkage renders a dataset that contains data on manager-fund-stock-quarter holdings.

Table 1 shows the summary statistics for our sample of US active domestic equity funds. It comprises 6,926 unique managers and 2,531 unique funds. The average total net assets (TNA) equal \$1.6 billion. Of all the mutual funds, 66% are managed by teams rather than individual managers. Figure 1 shows the exponential increase in the fraction of both funds and TNA that are managed by teams versus individual managers over the past two decades. The median number of managers per fund equals two. Manager tenure, which we define as quarter t minus the quarter when the manager appears first in the sample, has a median equal to 28 quarters, or 7 years. Each fund holds a median of 58 stocks.

2.2. Measuring extrapolation

To directly measure the extrapolative belief formation in mutual fund managers, we need to have extensive data on mutual fund managers' expectations of stock-level returns. These data, to the best of our knowledge, are not readily available for research. To circumvent this limitation, we rely on a key insight from the theory and empirical work on return extrapolation (e.g. Greenwood and Shleifer 2014; Barberis et al. 2015). This work suggests that extrapolators' beliefs affect their trading decisions in that extrapolators buy (sell) stocks when these stocks have done well (poorly) in the recent past. Therefore, we capture fund managers' beliefs by measuring the extent to which portfolio holdings move according to past stock returns.

For ease of notation, we refer to funds in the remainder of this subsection, but we apply the same method to measure extrapolation for a team of managers or for individual managers. For each unique fund $j = 1, \ldots, J$, we regress the changes in the portfolio weight of stock s between the end of quarter t and the end of quarter $t+1$, $\Delta w_{s,j,t+1}$, on that stock's past yearly return, $r_{s,t-4\to t}$, plus a host of stock characteristics $C_{s,t}$ and time fixed effects λ_t:

$$\Delta w_{s,j,t+1} = \alpha_j + \beta_j^X r_{s,t-4\to t} + \gamma_j C_{s,t} + \lambda_t + e_{s,j,t+1} \quad \text{for} \quad j = 1, \ldots, J. \tag{1}$$

We define the fund's investment universe as the set of stocks a fund has held at any point in time from the previous and over the next 11 quarters. This approach adapts the definition of fund managers' universe recently proposed in Koijen and Yogo (2019) that captures first-time purchases while also considering when a stock may have already entered the manager's universe, but the manager has yet to buy it. Regarding the controls, we use time fixed effects, λ_t, and stock level controls, $C_{s,t}$. Because we are measuring the extrapolation beta of managers in different time periods, we add time fixed effects to control for trading behavior that is transitory and could be spurred by temporary market conditions. We also add the following standard stock characteristics as controls that are known to influence trading and

predict returns: size, book-to-market, asset growth, profitability, and past 12-month return volatility.[6]

The main coefficient of interest in Equation (1) is β_j^X that we refer to as the *extrapolation beta* of fund j. We refer to funds with an extrapolation beta below or equal to zero ($\beta_j^X \leq 0$) as *contrarians*, while $\beta_j^X > 0$ identifies *extrapolators*.

As far as the dependent variable is concerned, using a simple quarter-on-quarter change in portfolio weights is not appropriate for our analysis. The reason is that portfolio weights can change even in the absence of active managerial decisions because of how a stock performs relative to the other stocks the fund owns. Instead, to capture the component of funds' trading that is due to active choices by fund managers, we define weight changes as:

$$\Delta w_{s,j,t+1} = w_{s,j,t+1} - \frac{(1 + r_{s,t\to t+1})}{(1 + r^P_{j,t\to t+1})} w_{s,j,t}, \tag{2}$$

where $r^P_{j,t\to t+1}$ is the total portfolio return for fund j in quarter $(t, t+1]$, and $r_{s,t\to t+1}$ is the stock-return over the same quarter. This correction ensures that we are only capturing weight changes that reflect portfolio managers' active buying or selling. A further correction is needed to account for the fact that funds are likely to be heterogeneous in the number of stocks they hold. Empirically, the volatility of cross-sectional weight changes is smaller for funds that hold many stocks. This heterogeneity in volatility leads to systematic differences in the magnitudes of β_j^X across funds. Thus, to make the magnitudes of β_j^X comparable across funds, we standardize the weight changes to a mean of zero and a standard deviation of one in each quarter.

Our interpretation of a positive β_j^X is that it indicates a fund's behavioral tendency to extrapolate past returns into the future. This tendency leads to incorrect investment decisions if past stock returns do not predict future stock returns in the cross-section. In this respect, one could argue that our measure of extrapolative behavior simply captures the funds' propensity to adopt a momentum strategy, that is, a strategy that buys (sells) stocks

[6] We explain these controls in greater detail in Appendix B.1.

that have done well (poorly) in the past (Jegadeesh and Titman 1993). Since past extreme winners are known to outperform past extreme losers in the subsequent period, a positive β_j^X could actually reflect behavior that is not based on cognitive biases or heuristics and is instead rather rational.

It bears emphasizing that a positive β_j^X is unlikely to reflect the funds' momentum trading. There are four reasons why. First, the momentum strategy provides only prescriptions for trading in the tails of the distribution of past stock returns, while our regressions capture trading behavior over the entire cross-section of funds' holdings. Second, the momentum strategy indicates that past stock returns measured as of the end of quarter t should affect portfolio choice at that point in time rather than changes in portfolio holdings a quarter later. In fact, the empirical evidence on the momentum strategy indicates that it achieves the best performance with a 1-month holding period (Hou et al. 2020). So, funds that are optimally trading stocks based on the momentum signal as of quarter t might have already rebalanced away from those stocks one quarter later. As a result, momentum trading by a fund does not evidently lead to a larger value of the fund's extrapolation metric. Third, momentum trading executed at time t might mechanically affect weight changes at time $t+1$, since past winners do predictably better than past losers and possibly the rest of the fund managers' holdings, and hence these past winners would experience a positive weight change. However, as we explained earlier, our weight changes are calculated so as to reduce the mechanical effect of increases in stock prices on portfolio weights, whether due to momentum or not. Fourth, as we show in the next section, extrapolative funds offer inferior investment outcomes compared to contrarian funds. This result shows that the extrapolation metric is likely to capture the incidence of a cognitive bias that impairs performance as opposed to momentum, that improves performance.

To further strengthen the interpretation of extrapolation betas as a gauge for irrational behavior rather than momentum trading, we redo our analyses in Section 5 using an alternative measure. For this measure, we re-estimate the extrapolation betas by excluding momentum

stocks, that is, the stocks that are in the bottom and top deciles of momentum sorts.

Table 1 provides the summary statistics of our sample. We observe that both the average and median funds have an extrapolation beta below zero, which means that funds are on average contrarians. However, there is a substantial heterogeneity across funds in which 33% of the sample being characterized by an extrapolation beta larger than zero, and the remaining 67% showing contrarian behavior. We reach similar conclusions when looking at the no-momentum variant of our extrapolation measure. We also find a large cross-sectional pairwise correlation between the two extrapolation metrics in Panel B. This correlation indicates that the results of the paper are not sensitive to the use of one particular specification of the extrapolation metric. Therefore, in the remainder of the paper we use the first specification as our baseline extrapolation beta, β_j^X, and clarify when the alternative no-momentum metric is used for robustness.

[Place Table 1 about here]

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2.3. Fund performance and fund flows

One important part of the analysis concerns the relationship between a fund manager's extrapolative behavior and the fund's investment performance. To measure a mutual fund's performance, we consider four distinct types of metrics: (i) fund returns, (ii) fund flows, (iii) volatility of the fund's risk adjusted returns, and (iv) the volatility of fund flows.

To measure the fund returns, we use two distinct approaches. We first use Fama-French Carhart (FFC) alphas (Fama and French 1993; Carhart 1997) as a measure of the risk adjusted returns. Studying four-factor alphas is particularly meaningful for our analysis, since these alphas measure the component of a fund's performance that is orthogonal to the performance of known strategies, such as value, size, or momentum. Orthogonalizing funds' performance with respect to the momentum returns facilitates the study of extrapolation

and its effect on returns after accounting for momentum-based trading. To estimate the risk adjusted returns at time t, we use a rolling window of the previous five years of monthly returns to estimate the fund's factor exposures. We then use these exposures to estimate the fund's risk adjusted returns over the following quarter by subtracting the portion of fund returns that are the result of factor exposures from the fund's returns. Using these factor exposures, we also calulate monthly FFC alphas to estimate manager risk as the standard deviation of FFC alphas over the following 12-month period.

As an alternative measure of performance, we also use an indicator variable for whether a fund is considered a star fund in a given quarter. A star fund is one that ranks in the top 10% of quarterly returns in its respective category of its Morningstar style box. This measure is particularly relevant for our analysis, because managers' compensation can be linked directly to the achievement of this star status (Ma et al. 2019). Moreover, funds with a top Morningstar star rating attract the majority of mutual fund flows (e.g. Guercio and Tkac 2008) that also generates indirect benefits from that status.

In addition to investment performance, we analyze how extrapolation is related to the mutual fund flows. New money flowing into the fund is of particular interest, because assets under management are an important part of a fund manager's variable compensation. We use the total fund flow in a given quarter relative to its size in the previous quarter:

$$Flow_{j,t} = \frac{TNA_{j,t} - TNA_{j,t-1} \times (1 + r^{P}_{j,t-1 \to t})}{TNA_{j,t-1}}, \tag{3}$$

where $TNA_{j,t}$ is the TNA of fund j at time t. Furthermore, we also calculate the flow measure at a monthly frequency to estimate its volatility as the standard deviation in monthly fund flows over the following 12-month period.

3. Extrapolation in Mutual Funds: Implications for Investment Outcomes

In this section, we ask to what extent the extrapolation of stock returns by mutual fund managers can be considered a bias. If data on fund managers' expectations of future stock returns were available, we would be able to directly test whether their extrapolative beliefs result in systematic expectational errors. Absent these data, the alternative approach we follow is to ask whether fund managers who extrapolate past returns are worse off compared to similar managers who are not extrapolators.

To this end, we rely on insights from Ma et al. (2019). They use the regulation introduced by the SEC in 2005 that requires funds to disclose details on its management and compensation structure. In their sample, more than 98% of US mutual fund managers have performance-based incentives. About 70% of the funds that disclose the breakdown between fixed and performance-based pay indicate that the performance-based pay represents a larger fraction of fund managers' compensation than the fixed pay. Some funds also disclose that their managers' compensation can grow substantially when a fund places in the top of the performance distribution. Furthermore, for a fraction of fund managers who report a non-fixed salary component, the fund ties that component to its TNA. Finally, Berk et al. (2017) show that fund managers' performance is not only important for their current compensation, but also for their career prospects.

Based on these insights, we argue that, insofar as return extrapolation among fund managers is a bias, it should lead to lower expected returns via (i) lower investment performance, (ii) lower probability that a fund achieves a top performance status, and (iii) lower fund flows which lead to lower TNA. To investigate this conjecture, we analyze how the extrapolation metric measured at the fund level is related to several investment performance metrics as well as to fund flows. We regress predictive regressions of fund outcomes on extrapolation betas estimated recursively over a 2-year rolling window to ensure that there is no look-ahead bias. We then define an extrapolation dummy that is positive for fund j at time t if the

fund's recursively estimated extrapolation metric is positive, and zero otherwise.

We subsequently perform panel regressions where the extrapolation dummy is used to test whether, in the cross-section of funds, return extrapolation leads to inferior investment outcomes. We perform the analysis by adding style × quarter fixed effects. This addition is important, since it facilitates the absorption of systematic differences in performance that can arise due to differences in investment style. Since almost 80% of fund managers' compensation contracts are tied to the performance of the fund relative to a relevant benchmark (Ma et al. 2019), style × quarter fixed effects also better capture the relationship between fund-level extrapolation and the component of fund returns that affects managers' compensation. We perform the analysis both univariately, and in the presence of controls that the literature has linked to fund performance. Details on these controls are in Appendix B.2.

The results of the analysis are in Table 2. In Panel A, we examine the relationship between lagged fund extrapolation and future performance across three metrics that are likely to be tied to a manager's variable compensation. These are (i) a fund's risk-adjusted return, (ii) a dummy variable that is equal to one if a fund is in the top 10% of funds' performance distribution within its style category, and (iii) fund flows; all measured over the following quarter. We focus on the multivariate results for brevity. Across all specifications, the relationship between extrapolation and performance is negative and statistically significant, that is, extrapolative funds underperform contrarian funds in the cross-section. These results are robust to the addition of several controls, such as the fund's past expense ratio, size, number of stocks managed, and past flows.

The results are also significant in terms of economic magnitude. In columns 1 and 2 of Panel A in Table 2, we find that funds that extrapolate underperform their peers by 12 basis points per quarter (column 2) or a total of 2.4% lower investment returns in the subsequent five years. Using a linear probability model, columns 3 and 4 of Panel A show that extrapolative funds have a 1.7 percentage point lower likelihood of achieving a top 10% ranking. Given that only 10% of the funds can achieve this status in any given quarter,

this lower likelihood represents a 17% decrease in the probability of achieving top status compared to funds that do not extrapolate. This result is particularly important because the reward for being a top performer is high, both directly in terms of actual compensation (Ma et al. 2019) and indirectly due to fund flows that chase performance (Sirri and Tufano 1998; Guercio and Tkac 2008).

The final columns of Panel A show a large and statistically significant, negative relationship between extrapolation and fund flows: funds that extrapolate receive fund flows that are 1 to 1.2 percentage points lower than their peers on a quarterly basis that is an economically large effect given that the average fund grows by 1.8 percentage points per quarter. In other words, market participants punish extrapolative fund managers by delegating a smaller amount of capital to them compared to contrarian funds. These results support those of the previous columns where we find that extrapolative fund managers deliver lower performance. These results also help strengthen the interpretation that our extrapolation metric captures an investor bias instead of momentum trading, because we find a negative link between extrapolation and future returns after correcting for the FFC factors that orthogonalize fund returns with respect to the performance of momentum.[7]

While the results so far indicate that extrapolation lowers fund performance and a fund manager's expected compensation, extrapolative funds might not only achieve lower performance, but also enjoy substantially lower performance and flow volatilities that thus lower managers' compensation volatility. In case of lower compensation volatility, extrapolation might make a risk-averse fund manager better off rather than worse off. To investigate formally whether this is the case, in Panel B of Table 2 we study the role of extrapolation on outcomes that proxy for the risk that fund managers face: the volatility of risk-adjusted

[7]In addition to return extrapolation, in unreported tests we have also explored how cash flow extrapolation affects mutual fund outcomes (Alti and Tetlock 2014; Hirshleifer et al. 2015; Bordalo et al. 2018a; Choi and Mertens 2019). To that end, we have used the coefficient on operational profits from our estimation of Equation (1) as a proxy, with a larger positive value of the coefficient indicating a stronger cash-flow extrapolation. However, the evidence that cash-flow extrapolation leads to biases in our empirical setting is mixed. We thus decide to focus primarily on return extrapolation, and leave to future research how teams respond to cash-flow extrapolation.

returns and the volatility of fund flows. The results show no evidence of a different volatility in risk-adjusted returns nor flows between extrapolative and contrarian funds. Overall, funds whose trading conforms to the extrapolation of past returns underperform their peers and experience smaller growth in TNA. Underperformance and lower fund size lead to compensation losses for a fund manager which are not mitigated by lower compensation volatility. Therefore, these results support the idea that return extrapolation is a bias that leads to systematic investment mistakes.

[Place Table 2 about here]

4. Extrapolation Bias and Asset Management Teams

Having shown that extrapolation is a bias, we now ask to what extent teams attenuate or exacerbate the extrapolative behavior that fund managers show individually. Thus far, the literature has studied the effect of teams on performance or trading behavior by comparing all teams with all non-teams (e.g. Chen et al. 2004; Bär et al. 2011). The potential limitation of this approach is that the managers that comprise a team could be fundamentally different from the managers that manage a fund on their own. For instance, teams may consist of asset managers who are more or less educated, skilled, or experienced than the managers who manage a fund on their own. As a result, differences in the behavior of team-managed funds and solo-managed funds could be misinterpreted as the impact of teams as an institutional arrangement, while they are instead driven by compositional differences between managers operating in these two sets of funds.

To reduce the concern above, the trading behavior of a team-managed fund needs to be compared with the behavior that the members of that same team show when they manage a fund alone. This is precisely the approach that we follow. To do this comparison, we identify a restricted sample of mutual fund teams whose managers have all at some point in their career managed a fund by themselves.

4.1. Restricted sample

To construct the restricted sample of US equity mutual funds, we identify the subset of management teams whose members have managed a fund alone at some point in time during their careers. To ensure that we are capturing actual teams instead of incorrectly reported turnover events in solo-managed funds, we require the management teams to operate for at least four consecutive quarters to be included in the restricted sample. In total we have 549 unique managers, 939 unique funds, and 350 unique teams that satisfy these conditions. That is, the restricted sample makes up 37% of our original sample of mutual funds.

Table 3 shows the summary statistics for the restricted sample, and Panel A covers the solo managers and Panel B the teams. A comparison between the restricted sample and the full domestic US equity sample in Table 1 shows many similarities. Funds in both share a similar distribution of managerial experience, number of stocks, and fund fees. A few differences also exist. For instance, compared to the full sample, the restricted sample features a lower number of managers per fund. This is not surprising, since observing large team-based mutual funds whose managers have all managed a fund alone at some point in time is less likely.

In the restricted sample, the statistics concerning extrapolation for solo managers and teams are similar. For both groups, the average extrapolation metric is around -0.06 that indicates both the average team and the average solo manager in our sample are contrarian. This result is comparable to what we found for the full sample. Similarly, the fraction of extrapolators in the restricted sample is also close to what we found for the full sample. In particular, 36% (31%) of individually (team) managed funds are classified as extrapolative versus 33% in the full sample. This overall comparison means that our restricted sample is representative of the full sample of US equity mutual funds.

[Place Table 3 about here]

4.2. Empirical Approach: Conceptual Framework

Our main empirical question is whether fund managers' extrapolation at the individual level gets inherited by the team these managers join. Within the restricted sample, we can investigate this transmission in two ways. First, we can compare the average extent of extrapolation in the teams versus the solo funds of the restricted sample. Having fixed the population of fund managers in both types of funds, this comparison already draws some possible conclusions on the role of extrapolation in teams.

The second more ambitious approach estimates quantitatively the degree to which team members transmit the extrapolation to a team. To this end, for each team in the restricted sample, we construct a *statistical team counterfactual*. The use of a statistical counterfactual in tests of teams' decision-making is common in the experimental literature on teams. The idea behind it is to observe how each team member deals with a task alone, and then compare team members' average individual performance (i.e., the statistical team counterfactual) with the performance achieved by an actual team composed of the same individuals. The comparison between teams and statistical counterfactuals is informative about the value of teams to decisions in that the human capital deployed in both the actual and the counterfactual team is the same, but the synergistic benefits of team members' interactions are absent in the counterfactual. We adopt this approach in our setting.

To this end, we define $\hat{\beta}_j^{CF}$ as the average extrapolative behavior shown by the individual members of a team when they manage alone. We then formally test how teams inherit the trading behavior of their members, by means of the following regression:

$$\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^E + \delta_2 D_j^E + \delta_3 C_j + \epsilon_j, \tag{4}$$

where $\hat{\beta}_j^{TM}$ is the extrapolation metric of the team, D_j^E is a dummy variable that is equal to one if the counterfactual team is extrapolative (i.e., $\hat{\beta}_j^{CF} > 0$), and C_j indicates a set of team controls.

The regression framework above facilitates the answering of two main questions. First, an estimation of the regression indicates whether team members' extrapolation survives the scrutiny and the aggregation of ideas that occur in a team. In particular, the sum $\delta_0 + \delta_1$ represents the extent to which extrapolation bias at the individual level is transmitted to a team. Under the null hypothesis of no effect of teams on decision-making, that is, $\delta_0+\delta_1 = 1$, there is a full transmission of extrapolation from team members to the team. The alternative hypothesis is that teams either exacerbate ($\delta_0+\delta_1 > 1$) or attenuate ($\delta_0+\delta_1 < 1$) extrapolative behavior. In the remainder of the section, we refer to the "team effect" as the evidence that team members' extrapolative behavior is not inherited perfectly by the team. So, a team effect would arise if $\delta_0 + \delta_1 \neq 1$ in the data. Furthermore, we refer to a *positive* team effect as evidence that the bias that exists at the individual level is attenuated by teams, that is, $\delta_0 + \delta_1 < 1$; and a *negative* team effect as evidence that biases are exacerbated by teams, that is, $\delta_0 + \delta_1 > 1$.

Second, the regression framework outlined above can contrast the transmission of extrapolation biases from individuals to teams with the way in which teams inherit contrarian behavior. This comparison is meaningful, since the results of the performance tests in Section 3 indicate that contrarian trading generates superior outcomes for fund managers compared to extrapolation. In this respect, the coefficient δ_0 shows the extent to which contrarian behavior at the individual level is inherited by the team. In contrast, the coefficient δ_1 sheds light on whether teams can discriminate between behavior that decreases performance, such as the extrapolation bias, and trading rules that enhance performance in the cross-section of funds, such as contrarian trading.

4.3. Empirical Approach: IV methodology

We first estimate Equation (4) with a standard OLS. However, the OLS coefficient estimates are likely to be biased. The reason is that our right-hand variable, $\hat{\beta}_j^{CF}$, is a generated regressor and as such is likely to have noise in the measurement. In the presence of

measurement error (uncorrelated with the error term) in one of the regressors, the coefficient estimates for that regressor are downward-biased (Champernowne 1972). As a result, measurement error can lead to an over-rejection of the null of no team effects in favor of the alternative hypothesis of a positive team effect.

To address the issue of measurement error in our regressions, we rely on an instrumental-variable (IV) approach that is in the spirit of Jegadeesh et al. (2019). Their IV approach relies on the richness of the data to address the measurement error in tests of asset pricing models. Specifically, in the first stage they estimate stocks' factor exposures in two disjoint subsamples of their overall data. They then use the two sets of exposure estimates as the independent and instrumental variables in the second-stage regression. They show that this procedure is valid in that the two variables are highly correlated, but their measurement errors are likely to be uncorrelated because both variables are estimated with disjointed samples. As a result, both the relevance and exclusion restriction criteria for this IV approach are satisfied.

Our setting shares similarities with Jegadeesh et al. (2019) in that our main regressor, $\hat{\beta}_j^{CF}$, is estimated from a rich dataset of fund holdings that spans many stocks over many quarters. As a result, we propose a similar approach by estimating extrapolation betas on disjointed samples. Specifically, to randomly split a manager's holdings data into two equally sized and disjointed subsamples, we randomly partition a fund's stock holdings in every quarter into two subgroups. We then separately estimate Equation (1) for both of the obtained random samples. Thus, we get two separate estimates of $\hat{\beta}_j^{CF}$ for each team, $\hat{\beta}_j^{CF,1}$ and $\hat{\beta}_j^{CF,2}$. We can then use $\hat{\beta}_j^{CF,2}$ as an instrument for $\hat{\beta}_j^{CF,1}$ (and vice versa) in the

following 2SLS regression:

$$\text{1st stage: } \begin{cases} \hat{\beta}_j^{CF,1} & = c_1 + \lambda_{1,0}\hat{\beta}_j^{CF,2} + \lambda_{1,1}\hat{\beta}_j^{CF,2} \times D_j^{E,2} + \lambda_{1,2}D_j^{E,2} + \lambda_{1,3}C_j + u_{1,j}, \\ \hat{\beta}_j^{CF,1} \times D_j^{E,1} & = c_2 + \lambda_{2,0}\hat{\beta}_j^{CF,2} + \lambda_{2,1}\hat{\beta}_j^{CF,2} \times D_j^{E,2} + \lambda_{2,2}D_j^{E,2} + \lambda_{2,3}C_j + u_{2,j}, \\ D_j^{E,1} & = c_3 + \lambda_{3,0}\hat{\beta}_j^{CF,2} + \lambda_{3,1}\hat{\beta}_j^{CF,2} \times D_j^{E,2} + \lambda_{3,2}D_j^{E,2} + \lambda_{3,3}C_j + u_{3,j}. \end{cases} \tag{5}$$

$$\text{2nd stage: } \hat{\beta}_j^{TM} = \alpha + \delta_0\hat{\beta}_j^{CF,1,pred} + \delta_1\hat{\beta}_j^{CF,1,pred} \times D_j^{E,1,pred} + \delta_2 D_j^{E,1,pred} + \delta_3 C_j + \epsilon_j,$$

where *pred* indicates the predicted values from the first-stage regressions.

Following Jegadeesh et al. (2019), we provide the results of simulations that are aimed at testing whether the approach outlined in Equation (5) generates unbiased estimates of the coefficients for the second-stage regressions in Appendix C. Our results provide strong support for the use of the IV in our setting. Therefore, we provide both OLS estimates and IV estimates in our main tables as a way to probe our results for biases in estimation and draw more robust conclusions.

4.4. Results

We provide summary statistics for the team extrapolation betas and the betas of counterfactuals teams in Table 4. Furthermore, we report the result of a difference-in-means test. When comparing the extrapolation metrics of all teams and all counterfactuals in the restricted sample, we find that the team betas and the counterfactual betas do not statistically differ from each other. More importantly, we also separate teams based on the behavior that their members show when they manage a fund alone. This separation renders two subsamples, one in which the counterfactual team extrapolates (i.e., $\beta^{CF} > 0$), and a second one in which the counterfactual team behaves as a contrarian. Here, a different picture emerges. While the behavior of teams and their counterfactuals does not statistically differ for teams whose members are contrarians on average, the teams that comprise managers that mainly extrapolate do have a substantially lower extrapolation beta compared to its corresponding counterfactual beta. Specifically, while the extrapolation beta of the counterfactual team

equals 0.06 in teams whose members extrapolate on average, extrapolation in the actual team is close to absent. This difference is significant at the 1% level. This finding offers introductory evidence that when managers join a team, their biased behavior becomes attenuated. At the same time, contrarian behavior remains similar when the managers join a team. This finding suggests that teams inherit their team members' reliance on past returns differently, and it depends on whether this reliance improves or destroys performance.

[Place Table 4 about here]

While the introductory analysis already shows that teams can have a positive effect by reducing biases, this analysis does not yet speak to the issue of how biases at the individual level are transmitted to a team. To this end, we perform a regression analysis along the lines described earlier in this section. Table 5 summarizes the results.

Columns 1, 2, 5, and 6 show the results of a simpler nested version of the full model in Equation (4):

$$\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 C_j + \epsilon_j.$$

While the nested regression does not directly explain whether biases are transmitted to the team, it provides evidence for the role of measurement error in our regressions as well as the usefulness of the IV methodology that we have implemented. Columns 1 and 2 show that the OLS estimates indicate that the transmission of heuristic rules is imperfect. The coefficient δ_0 for $\hat{\beta}_j^{CF}$ is always lower than one, and the null hypothesis of perfect transmission (δ_0=1) is always rejected at the 1% level. Without further analysis, the researcher could not rule out that this evidence is the result of measurement error in the independent variable. Therefore, to assess the robustness of this conclusion to measurement error, we adapt the IV approach described above to this simpler specification and estimate δ_0 again. As expected, the IV estimator of the coefficient of interest δ_0 is larger in magnitude than the OLS estimate.[8]

[8]The critical value for the weak instrument test based on correlations proposed by Nelson and Startz (1990) is 0.054 and is based on the number of teams. We find a correlation of 0.38.

This increase in magnitude corroborates the reasoning that the OLS coefficient estimates may have shrunk toward zero due to measurement error and that an IV procedure can correct for this issue. However, while the IV estimator grows closer to one, the IV does not change the conclusion because the null hypothesis of a full transmission is rejected at the 5% level in all specifications.[9]

Columns 3, 4, 7, and 8 show the results for our main regression of interest. For brevity, we concentrate on the IV results, but the conclusions we draw are similar to the OLS results. The coefficient δ_0 captures the transmission of contrarian behavior to the team. The estimate of δ_0 is close to one, and the null hypothesis that contrarian behavior is fully transmitted to the team (i.e., $\delta_0 = 1$) cannot be rejected. In stark contrast to the way in which teams absorb contrarian behavior, the regression estimates of the team effect for extrapolative behavior (i.e., the sum $\delta_0 + \delta_1$) is around 0.2. This positive team effect is also statistically significant.

In particular, we always reject the null that $\delta_0 + \delta_1 = 1$. Moreover, we are never able to reject the null that $\delta_0 + \delta_1 = 0$ at any conventional level of significance. These results are robust to including controls such as the team's average TNA, average tenure, and its investment styles. The results are also economically large: on average extrapolative behavior at the individual level is attenuated in teams by close to 80%. Overall, these findings show evidence of the attenuation of extrapolation bias in teams.

[Place Table 5 about here]

5. Robustness

This section aims to provide robustness for our main results. First, we show that our results are robust to the exclusion of momentum stocks from the analysis. Second, we show

[9] In Appendix C.5, we show that the results for the IV methodology are not sensitive to the choice of a random sample that is used in the IV procedure. We repeat the analysis for 2,500 randomly disjointed, drawn samples. We find that, as long as the IV passes the standard weak-instrument diagnostic, for example Nelson and Startz (1990), the results are similar.

that (i) experience, (ii) style migrations, (iii) workload, and (iv) compensation structures in solo-managed compared to team-managed funds do not explain the attenuation of biases in teams.

5.1. Exclusion of momentum stocks

One concern with our extrapolation metric is that it captures the fund manager's propensity to use a momentum trading strategy. To deal with this concern, we reestimate fund, manager, and team extrapolation betas with an alternative specification in which we identify stocks that would be used in a momentum trading strategy and exclude those stocks from fund holdings data when estimating the extrapolation beta.

To reestimate the extrapolation betas, we estimate cumulative stock returns from our CRSP universe from month $t-12$ to $t-1$ every quarter and use these returns to divide stocks into 10 momentum decile portfolios.[10] We then proceed by removing stocks that are in the top and bottom ranked portfolios from our data on mutual fund holdings and reestimate the extrapolation betas at the fund, single manager, and team level that we use throughout the paper.

We first redo the earlier analysis done in Table 2 by using this alternative extrapolation metric and examine whether extrapolative funds achieve worse outcomes than their peers. Like in previously reported findings, in Panel A of Table 6, we find that our conclusions remain unaltered. Extrapolating funds underperform the remaining funds in terms of risk adjusted returns, the likelihood of being a star fund in the following quarter, and attracting less flows, all with similar magnitudes as reported for our main measure in Table 2. Additionally, in column 4 of Panel A in Table 6, we also find evidence that extrapolative funds are associated with higher risk as measured by the future risk adjusted return volatility. This finding means the managers or teams that extrapolate do not benefit from lower risk in their

[10]Consistent with the literature on momentum (Jegadeesh and Titman 1993; Asness 1995; Farna and French 1996), we use the past year returns that exclude the one month before the formation period as our definition of momentum.

potential compensation. Thus, while studies have shown that momentum is a performance enhancing strategy, our results indicate that extrapolative funds are unlikely to follow such a trading strategy given the poorer outcomes that these funds achieve.

Second, we also reexamine the transmission of extrapolation bias from individuals to their respective teams using our no momentum stock extrapolation beta. In Panel B of Table 6, we confirm that our results are robust to this alternative metric. In columns 1 and 3, we reject the null hypothesis of a full transmission that means teams attenuate extrapolative behavior. In columns 2 and 4, we find that the attenuating effect is mainly present in extrapolative teams, while contrarian behavior gets fully transmitted to the team in the IV specification. These results are both qualitatively and quantitatively similar to those found in Table 5, and they confirm that our results are not driven by momentum seeking managers.

[Place Table 6 about here]

Finally, we also rule out that momentum crashes explain the poor performance of extrapolating funds. We re-estimate the regressions from Table 2, excluding the year 2001 and 2002 as well as the second and third quarters of 2009, which correspond to the periods of momentum crashes documented by Daniel and Moskowitz (2016) that span our sample period. In unreported regressions, we find that omitting these periods does not change the conclusion of our analysis: extrapolating funds still earn lower risk adjusted returns and receive less inflows.

5.2. Learning and Experience

Our previous findings indicate that teams help alleviate investment biases in decision-making. An alternative interpretation of our findings could be that such a bias attenuation is simply driven by the differential experience that fund managers have when they manage alone as opposed to when they manage in teams. Suppose that extrapolative managers are less experienced when they operate a fund on their own. Over time, these managers learn that

the extrapolation bias hurts their performance. Thus, they progressively extrapolate less. If learning has occurred by the time these managers join a team, then the counterfactual extrapolation betas, which are measured using the entire history of team members' solo-managed funds, may overstate team members' individual tendencies to extrapolate and as a result inflate our counterfactual extrapolation metric. Inflated counterfactual betas in our main test could generate results in line with ours even when there is a full transmission of biases from individuals to teams. Thus, the bias reduction might not stem from the workings of the team but from a form of mis-measurement of extrapolation at the individual level.

To address this concern empirically, we repeat our main analysis for a selected sample of *non-learners*, i.e., managers whose extrapolative behavior does not decline considerably with experience. To identify teams of non-learners, we first estimate fund managers' extrapolative behavior separately for the first half (the early sample) and the second half (the late sample) of the sample period in which these managers have managed a mutual fund alone. We then select the non-learners as the managers who extrapolate in both periods. To validate our definition of non-learners, we compare the extrapolation betas for the early and late sample and find for this group an average value of 0.07 for the early sample and an average value of 0.08 for the late sample. A difference in means test reveals that these values are statistically indistinguishable from each other (t-stat $= -1.48$).

Figure 2 presents scatter plots that relate team-level extrapolative behavior to team members' average extrapolative behavior for all teams versus extrapolative teams only, and does so for the full sample (Panel A) as well as the sample that consists of the non-learners only (Panel B). Panel A presents in graphical form the evidence on the transmission of extrapolative behavior that we offer in Table 5. More importantly, a comparison between Panel A and Panel B reveals very little differences between the extent to which extrapolative behavior is transmitted to teams of learners and non-learners. In particular, like for the full sample, teams attenuate the extrapolative behavior of non-learners just as much as they attenuate the extrapolative behavior of other managers. In stark contrast, the transmission

of contrarian behavior is the same among non-learners and the rest of the sample. Overall, this result confirms the robustness of our finding to a learning story. As a matter of fact, the finding that teams can alleviate biases even among managers that display little tendency to learn when operating alone, suggests that teams may serve as a stronger device to curb biases than individual experience.

As a second test, we use manager tenure as a proxy for experience and learning. We first note that, for learning outside of the team to be an explanation of our findings, we expect a large difference in tenure between managers when they are in a team and when we observe them acting alone. However, data in Table 3 indicate there is only a difference of three quarters between the average tenure managers have accrued when we observe them acting in teams as opposed to when we observe them acting individually.

We also perform additional analysis in Table 7 by formally testing whether our results of bias reduction in teams are due to experience effects. Formally, we run the following augmented regression with double interactions:

$$\begin{aligned}\hat{\beta}_j^{TM} &= \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^E \times \Delta Tenure_j + \delta_2\hat{\beta}_j^{CF} \times D_j^E + \delta_3\hat{\beta}_j^{CF} \times \Delta Tenure_j \\ &+ \delta_4 D_j^E \times \Delta Tenure_j + \delta_5 D_j^E + \delta_6 \Delta Tenure_j + \delta_7 C_j + \epsilon_j, \end{aligned} \tag{6}$$

where $\Delta Tenure_j$ indicates the team members' average difference in tenure when managing as part of team j versus when they manage alone. If the differential experience of managers drives our results, we expect coefficient δ_1 to be statistically significant and negative. At the same time, the coefficient δ_2, which measures bias attenuation when there are no differences in tenure between solo-managed versus team-managed funds, should become smaller and possibly not statistically significant. Panel B of Table 7 shows that δ_1 is statistically indistinguishable from zero. Moreover, the interaction term on the the counterfactual beta and the extrapolative team indicator δ_2 remains negative and in the same order of magnitude compared to the findings in the previous section. We take this as evidence that managers'

earned experience outside of the team does not drive our results.

[Place Figure 2 about here]

5.3. Style Migrations

Another alternative interpretation of our findings is that the attenuation of extrapolation bias that we find is driven by team members systematically migrating from one style when managing individually to another style when managing in a team. If differences in extrapolative behavior exist among different styles, then a migration between two styles could generate evidence consistent with our findings. We address this alternative interpretation in three ways. First, we add style fixed effects of the teams to our regressions. Second, we add stock characteristics such as market-to-book and size to our estimation of managers' extrapolative behavior. This is useful in that it should reduce the extent to which differences in extrapolation across managers are due to funds' size and value-based style classifications. Third, we perform a similar double interaction regression as in Equation (6), where we formally test whether our results of bias reduction in teams are due to style migrations:

$$\begin{aligned}\hat{\beta}_j^{TM} &= \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^E \times D_j^{SM} + \delta_2\hat{\beta}_j^{CF} \times D_j^E + \delta_3\hat{\beta}_j^{CF} \times D_j^{SM} \\ &+ \delta_4 D_j^E \times D_j^{SM} + \delta_5 D_j^E + \delta_6 D_j^{SM} + \delta_7 C_j + \epsilon_j, \end{aligned} \tag{7}$$

where D_j^{SM} indicates a style migration for team j. In order to construct the indicator, we identify the prevalent style of the team as well as the prevalent style among team members when they manage alone. We consider all nine style classifications in Morningstar. Out of 350 teams, 84 experienced style migrations, or 24% of the teams (Panel A, Table 7). If the style migrations drive our results, we expect the coefficient δ_1 to be statistically significant and negative, and δ_2 should become smaller and less statistically significant. Panel C of Table 7 shows that δ_1 is statistically indistinguishable from zero. Moreover, the interaction

term on the the counterfactual beta and the extrapolative team indicator δ_2 remains negative and even becomes larger in magnitude compared to our findings in the previous section. We take this as evidence that style migrations do not drive our results.

[Place Table 7 about here]

5.4. Manager workload and bounded rationality

Prior work has established that individuals make decisions with limited cognitive resources, and that the use of heuristics and the appearance of biases (like extrapolation) are more likely when these resources are depleted. Thus, it is possible that our evidence that biases are reduced when managers operate in teams stem from a loosening of such constraints in team-managed compared to solo-managed funds.

A loosening of bounded rationality constraints could in part be due to the team itself, in that the efficient division of labor that takes place within the team minimizes the burden imposed on each of the managers in the team. If this was the case, it would be appropriate to attribute the reduction of cognitive biases to the team as an organizational arrangement. However, the loosening of the bounded rationality constraints could also take place for reasons that are not intrinsically rooted in teams and teamwork, and rather stem from differences in workload between solo and team-based management. For instance, if the managers in our sample oversee larger and more complex portfolios at the time in which we measure their individual-level extrapolative behavior, but their overall workload (inside and outside of the team) is systematically lower at the time in which they operate as part of a team, the constraints of bounded rationality can be systematically more binding in solo-managed funds as opposed to team-managed funds. Systematically looser bounded rationality constraints and the ensuing reduction of biases observed in teams would not be due to the team per se, in that such a reduction could also be achieved in solo-managed funds if individual managers were asked to manage less complex and smaller portfolios.

To investigate whether systematic differences in workload between solo-managed funds and team-managed funds are responsible for our result, we approximate the workload of manager i with the number of stocks that the manager oversees in any of the portfolios he manages alone or in a team. We construct a time series of the total number of stocks the manager has in his investment universe at any given quarter t.[11] To construct the time series, we compute the total number of stocks managers have in their investment universe in all the solo-managed funds they manage at that point in time, plus the proportional fraction of the investment universe of the team-managed funds these managers co-manage. When allocating stocks of the team-managed holdings to a manager, we assume that a manager holding a given stock in his solo-managed funds will also be overseeing investments in that stocks when operating as part of a team. Finally, we carefully avoid double counting of overlapping stocks across multiple funds a manager can be involved with at the same time. For example, if a manager is simultaneously managing a solo-managed fund and a team-managed fund, and if both funds hold an Apple stock, than the Apple stock would only count once towards the manager's workload. Formally, we therefore define the workload as:

$$Workload_{i,t} = \sum_{j \in S_{i,t}} U_{j,t} + \sum_{j \in C_{i,t}} \max \left[\frac{U_{j,t}}{NumManagers_{j,t}} - Overlap_{i,j,t}, 0 \right], \tag{8}$$

where $U_{j,t}$ is the number of stocks in the investment universe of fund j at time t, $S_{i,t}$ is the set of solo-managed funds that manager i is in charge of at time t, $C_{i,t}$ is the set of funds that manager i co-manages at time t, $NumManagers_{j,t}$ is the total number of managers for fund j at time t, and $Overlap_{i,j,t}$ is the number of stocks from the team-managed fund j that overlap with the investment universe of all the solo-managed funds of manager i at time t. We include a max operator to ensure that any additional assignment that a manager has does not decrease their workload.

We obtain the workload of the members of team j when operating in the team and when

[11] Here we use the same definition of the investment universe as the one we use when we estimate our extrapolation metric, see Section 2.

operating in the counterfactual team composed of solo-managed funds, by taking time-series averages of the workload metric for each manager during that team's existence and take the average across managers. Finally, we compute the difference in workload when the managers operate in a given team j versus when they manage alone, which we define as $\Delta Workload_j$. The summary statistics in Panel A of Table 7 highlight the importance of this robustness check. We observe that, on average, managers have lighter workloads when managing in teams compared to when they manage solo-managed funds. On average, managers in team-managed funds oversee 39 fewer stocks compared to when they manage funds on their own.

We perform a similar regression as in Equation (6), where we replace $\Delta Tenure_j$ with $\Delta Workload_j$. If the differential workload of managers drives our results, we expect coefficient δ_1 to be statistically significant and negative. At the same time, the coefficient δ_2, which measures bias attenuation when there are no differences in workload between solo-managed versus team-managed funds, should become smaller and possibly not statistically significant. Panel D of Table 7 shows that δ_1 is statistically indistinguishable from zero. Moreover, the interaction term on the the counterfactual beta and the extrapolative team indicator δ_2 remains negative and of the same order of magnitude of the one observed in our main tests. We take this as evidence that managers' reduction in workload once they join the team does not drive our results.

5.5. Portfolio managers' compensation

A final alternative interpretation of our findings is that the attenuation of extrapolation bias is not driven by teams per se, but rather it is driven by systematic differences in portfolio managers' compensation between team and solo-managed funds. For instance, if managers have a compensation structure that is more strongly linked to their performance when they manage in teams as opposed to when they manage alone, then these stronger compensation incentives induce managers to use more effortful deliberation in their investment decisions.

More effortful decisions could reduce the reliance on heuristics observed in teams (Kahneman and Frederick 2002), consistent with our main finding.

To address this alternative interpretation, we hand-collect data from each fund's SAI through the SEC's EDGAR as of 2006. We choose 2006, because as of that year mutual funds are required to disclose information on how they compensate portfolio managers. For each fund that is in our restricted sample as of the first quarter of 2006, we collect data on the compensation structure, based on the last available SEC report before the fund leaves our sample.[12] We closely follow Ma et al. (2019) to extract the compensation structures. Specifically, we generate four dummy variables that are equal to one if managers have (i) a fixed compensation, (ii) compensation based on the performance of the fund, (iii) compensation based on the AUM of the fund, and (iv) share ownership in their own funds, and zero otherwise. In total, we went through 438 SAI filings. We convert the data at the fund level to the team or manager level by averaging the compensation structures across all the funds they manage. We then compare the compensation structure of the team with the compensation structure of the counterfactual, that is, the average compensation structure of the managers that comprise the team when they manage alone.

We have a total of 219 teams that appear in our restricted sample as of 2006q1, of which 72 teams are defined as extrapolative teams. Of these teams, we were able to collect data on 133 teams, out of which 43 are extrapolative teams. Table 8 compares the compensation structures for all teams and for the contrarians and extrapolators separately. First, taking all teams together, we find that the compensation structures do not differ between the actual and counterfactual team, except for fund ownership. The counterfactual team is more likely to own shares of their own funds, which implies that, if anything, managers in solo-managed funds have stronger compensation incentives compared to managers who operate in a team. This pattern is similar if we take contrarian and extrapolative teams separately, both in terms of statistical significance and magnitude. Because teams face compensation structures

[12] Ma et al. (2019) show that the compensation structure of a fund is stable over time, so we assume that the last available report is representative for the history of the fund's compensation structure.

that are similar or less tied to performance compared to their solo-managed funds, we take this finding as evidence that differences in compensation incentives between team-managed and solo-managed funds do not drive our results.

[Place Table 8 about here]

6. Channel

The finding that the extrapolation bias is reduced when operating in teams has important implications for investors at large, because it provides evidence of a positive role of teams in the asset management industry. However, the follow-up question of what mechanism delivers the documented bias attenuation is equally important. Answering this question can provide insights into optimal team design and guarantee teams can effectively deliver on the promise of attenuating individual-level biases.

6.1. Conceptual Framework

A large body of work in psychology and behavioral economics argues that the mechanisms that determine human cognition can be described through a dual-system model (Epstein 1994; Sloman 1996; Stanovich and West 2000; Kahneman and Frederick 2002; Kahneman 2003, 2011; Evans and Over 2013). The main idea behind the dual-system model is that cognition involves two distinct processes, namely, intuition (System I) and deliberation (System II). System I is fast, effortless, affective, stimulation-bound, and able to juggle multiple cognitive tasks at the same time. In contrast, System II is slow, effortful, and able to address one cognitive task at a time. The resources devoted to effortful processing of information are generally scarce and as a result, cognition has organized its activity by off-loading some tasks to System I.

Although, by relying on System I the burden on System II can be reduced, there are also potential costs. The seminal work of Tversky and Kahneman (1974) (henceforth KT)

highlights that System I relies often on heuristic rules. Heuristics, first documented by KT in the domain of decision-making under uncertainty, cause individuals to form beliefs about an object of interest, such as the probability an event will occur in the future, that depart systematically from rational expectations. Such a departure from rational expectations can be very costly. For instance, heuristics can lead to reacting too strongly or too little to incoming information compared to a rational counterpart (Bordalo et al. 2018b, 2020), or misperceive the risks certain actions entail (Gennaioli et al. 2012; Gennaioli and Shleifer 2020) with consequent excessive risk-taking and potentially suboptimal investment decisions (Benartzi and Thaler 2007).[13]

Like the literature, we argue that the extrapolation bias among asset managers can stem from the influence of System I on these managers' investment decisions.[14] We then ask whether mechanisms that help reduce biases within the dual-system framework can shed light on the bias reduction observed in teams.

Frederick (2005) labels *cognitive reflection* as the ability to successfully engage System II to override the incorrect judgments of System I. Kahneman (2000) points out that cognitive reflection is more or less likely to take place that depends on whether (i) the individual has sufficient knowledge (information and process) to reach a correct answer through deliberation and (ii) whether there are cues that evoke the necessity of intervention.

In the context of team-based asset management, the assumption that asset managers possess, already on their own, the necessary knowledge to allow System II to identify return extrapolation as a cognitive mistake is reasonable. Therefore, what teams may be able to provide to achieve successful cognitive reflection is a set of relevant cues that help engage

[13]It bears emphasizing that, although intuitive judgment can lead in some circumstances to suboptimal outcomes, this need not be the case in all settings. Gigerenzer and Goldstein (1996) and Katsikopoulos et al. (2020) offer examples of how simple rules of judgment can do as well or better than complex rule-based approaches to decision-making.

[14]The idea that extrapolation stems from heuristics is prevalent in other work. For instance, in Barberis et al. (1998), Greenwood and Shleifer (2014), and Barberis (2018), extrapolation is considered an outcome of the representativeness heuristic. Extrapolation can also stem from the law of small numbers (Rabin 2002). Finally, extrapolation can arise more naturally when bounded rationality constraints are binding (Hong and Stein 1999; Fuster et al. 2012; Glaeser and Nathanson 2017).

System II.

We argue that two types of cues can elicit cognitive reflection by the members of a team. The first is internal, and the second is external. Working in teams can provide an internal cue because team members need to communicate and motivate choices or beliefs to other team members. We conjecture that such a need can naturally shift the division of labor between System I and System II, so that the members of a team are able to rely on deliberation as opposed to intuition when making trading decisions. If this mechanism is at play, a manager who is prone to return extrapolation in his or her solo-managed fund may reassess this tendency when operating as part of the team. This reassessment in turn helps the manager override his or her intuitive judgment (and extrapolation in particular). We label this conjecture the *internal reflection* hypothesis.

Aside from internal reflection, we argue that teams also provide a second set of cues because team members can learn about their mistakes not through deeper introspection, but rather due to the critical assessment of each others' ideas. In this respect, other studies have shown that teams can achieve superior performance due to the scrutiny that team members offer when assessing each others' proposals or views.[15] For this reason, we are compelled to empirically test whether actual team interactions, as opposed to the mere organization of work in teams, provide additional cues for the attenuation of cognitive biases. We refer to this conjecture as the *external screening* hypothesis.

6.2. Empirical Strategy

We argue that the internal reflection mechanism and the external screening mechanism can be distinguished empirically in our setting. The reason is that these mechanisms have different predictions as to which teams should experience a larger or a smaller transmission of individual-level behavior.

[15] See for instance Marschak and Radner (1972) team-model as well as its application to the study of settings where team members possess heterogeneous skills, ideas, or information structures (Calvó-Armengol et al. 2015; Dessein and Santos 2006).

To see this difference, consider the workings of the internal reflection mechanism in a team composed only of extrapolative managers, as opposed to a mixed team that for example, is composed of half contrarian managers and half extrapolators. In the all-extrapolator team, internal reflection predicts the smallest pass-through of managers' individual tendencies. The reason is that all managers override their individual extrapolative behavior when working in a team. In contrast, only extrapolators override their individual behavior in a mixed team, while contrarian managers retain their individual behavior when operating in the team. As a consequence, the internal reflection mechanism predicts that in a regression of the team's extrapolative behavior ($\hat{\beta}_j^{TM}$) on its members' individual behavior ($\hat{\beta}_j^{CF}$), the regression coefficient should be closer to one in mixed teams as opposed to all-extrapolator teams. Thus, this coefficient would indicate a larger transmission of individual behavior to mixed teams as opposed to homogeneous teams of extrapolators.

The picture is different under the external screening mechanism. This mechanism is at play when one manager is able to identify the extrapolative tendencies of his or her peers. In this respect, it is reasonable to assume that contrarian managers, rather than extrapolators, can more naturally identify and challenge the extrapolative views held by some of their peers. As a result, in a regression of a team's extrapolative behavior ($\hat{\beta}_j^{TM}$) on its members' individual behavior ($\hat{\beta}_j^{CF}$), the external screening mechanism predicts that the regression coefficient should be closer to one in all-extrapolator teams as opposed to mixed teams. Thus, this coefficient indicates a larger transmission of individual behavior to the homogeneous teams of extrapolators as opposed to mixed teams.

Given the above, we test whether the attenuation of biases in extrapolative teams is the result of external screening versus internal reflection by virtue of the following regression:

$$\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^{AE} + \delta_2 \hat{\beta}_j^{CF} \times D_j^{M} + \delta_3 D_j^{AE} + \delta_4 D_j^{M} + \delta_5 C_{j,t} + \epsilon_j, \quad (9)$$

where D_j^{AE} is a dummy that equals one if all team members are extrapolators, while D_j^{M} is

a dummy that equals one if the team consists of both extrapolators and contrarians.

The regression effectively partitions the sample of teams into three sets: (i) a baseline set of all-contrarian teams in which the transmission of individual-level behavior to the team is measured by the coefficient δ_0, (ii) a set composed of all-extrapolator teams in which the transmission coefficient is $\delta_0 + \delta_1$, and (iii) a set composed of mixed teams in which the transmission coefficient is $\delta_0 + \delta_2$.

If internal reflection is mainly responsible for the reduction of biases, then we expect to find $\delta_0+\delta_1 < \delta_0+\delta_2$ or, more simply, $\delta_1 < \delta_2$; and there will be less transmission of individual-level behavior to the homogeneous extrapolative teams compared to mixed teams. On the contrary, external screening indicates that $\delta_0 + \delta_2 < \delta_0 + \delta_1$, or $\delta_2 < \delta_1$; and there will be less transmission of individual-level behavior to mixed teams compared to all-extrapolator teams.

Table 9 has the estimation of the model in Equation (9). We find in all specifications that $\delta_1 < \delta_2$, and we reject the null that the two coefficients are equal in all three specifications. This rejection indicates a larger attenuation of individual behavior in teams that are composed of all extrapolators, and a larger inheritance of individual-level behavior in mixed teams. Following our arguments above, this result supports the internal reflection mechanism but offers less support for the external screening mechanism. Overall, this result can shed new light on the means through which teams can help reduce cognitive biases.

[Place Table 9 about here]

7. Conclusion

Cognitive biases are pervasive, and are known to negatively impact individuals' investment behavior and financial outcomes. However, it is an open question whether the impact of cognitive biases would be attenuated or amplified when decisions are made by a team. To address this question, we use the mutual fund industry as a laboratory. We focus on how

return extrapolation, a cognitive bias that has received considerable attention in recent literature, influences the trading behavior of teams vis-a-vis the individual members of the team when they manage a fund alone. We document that return extrapolation generates suboptimal investment outcomes for an asset manager's standpoint, consistent with the interpretation of extrapolation being due to incorrect heuristics. We show that teams heavily attenuate the adverse impact of return extrapolation. Our results shed new light on the role of teams for bias correction, and highlight a potential benefit of team-based asset management.

We consider this paper as a step toward a deeper understanding of how formal organizational structures contribute to the attenuation or exacerbation of cognitive biases. The paper leaves many questions to future research. For instance, how do teams deal with other cognitive biases and with other sources of suboptimal investments that are evident at the individual level, such as overconfidence? What role do agency considerations play in the attenuation of bias that we have found? And what situational factors (e.g., the characteristics of a potential investment or task) or team members' personality traits are most useful for obtaining a reduction in cognitive biases in team decision-making? These are only some of the questions that we leave to future work.

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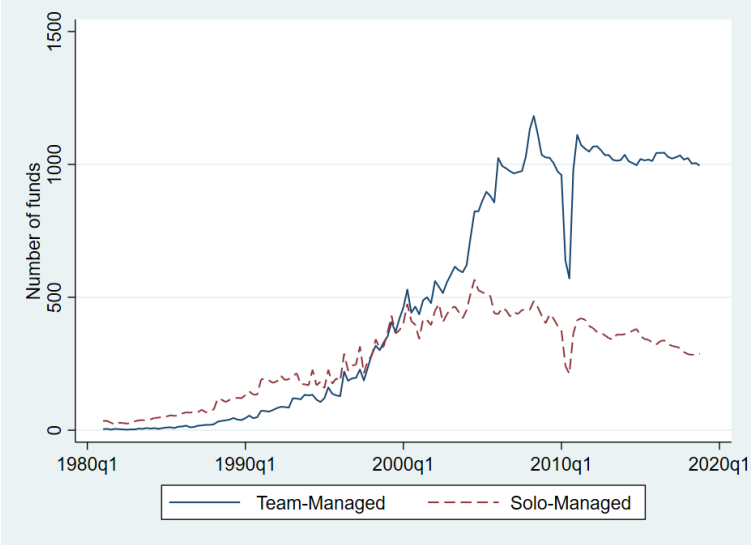
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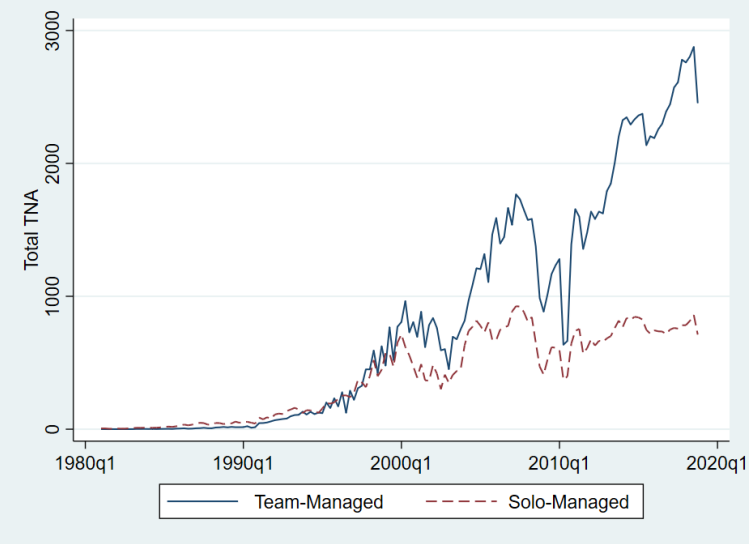
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Figure 1. **The Growth of Team-Based Asset Management**: This figure shows the time-series of the total number for funds (Panel A) and TNA (Panel B), in billion dollars, managed by individual mutual fund managers (red, dashed) and by teams of asset managers (blue, solid). The sample includes actively managed domestic US equity funds.

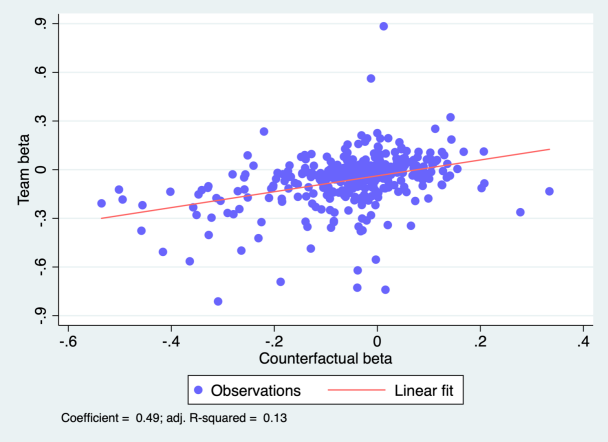


(a) Number of funds by type

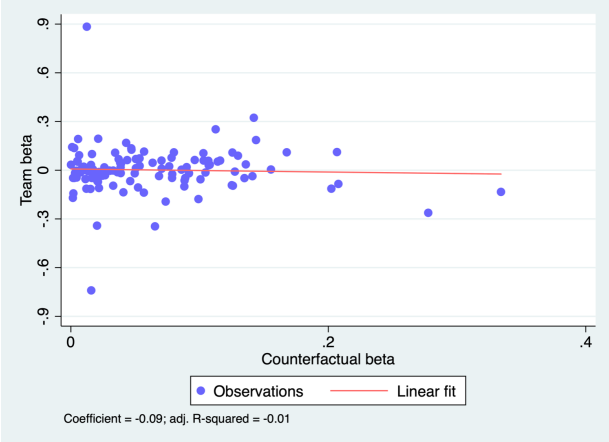


(b) TNA in $billion by fund type

Figure 2. **Counterfactual versus team betas**: This figure plots the team beta ($\hat{\beta}_j^{TM}$) against the counterfactual beta ($\hat{\beta}_j^{CF}$) for all teams (Panel A) and the teams that consist of non-learners (Panel B). The non-learners sample is defined as the set of managers who extrapolate in both the early and late part of the sample.

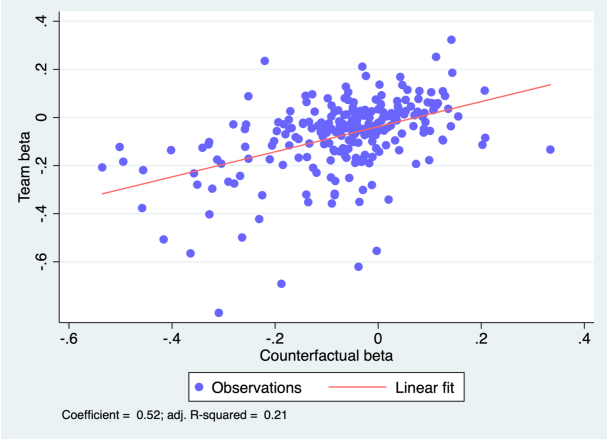


(a) All teams

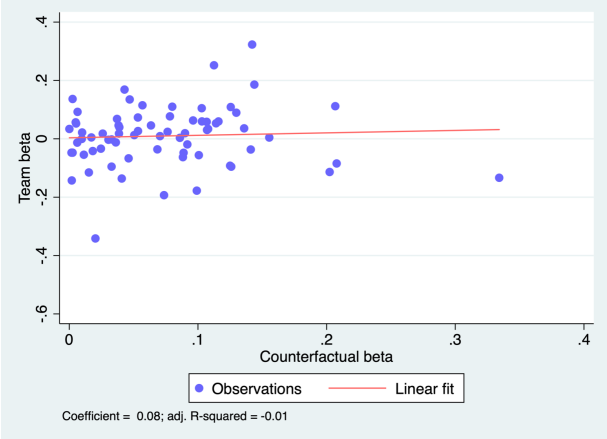


(b) Extrapolators

Panel A: Full sample



(c) All teams



(d) Extrapolators

Panel B: Non-learners sample

Table 1. **Summary statistics all active domestic US equity**: Panel A of this table reports the summary statistics for the domestic US equity sample for the period 1980Q1-2018Q4. Panel B shows the correlations of the variables that we use for our analyses. *TNA* is the fund's total net asset value, in million dollars; *team-managed* is a dummy variable that takes the value 1 if a fund is managed by a team; *manager tenure* is the average tenure of the managers in a given fund in quarters; the fund *extrapolation beta* is the extrapolation metric defined in Section 3; the *No Mom extrapolation beta* is the same metric estimated when excluding momentum stocks; the *Op Profit extrapolation beta* is the coeficient estimate on the operational profit control variable from Equation (1); *extrapolation dummy* is equal to 1 whenever the fund's extrapolation beta is positive; *expense ratio* is the fund's total expense ratio; *fund turnover* is the most recent portfolio turnover of a fund; *fund return* is the fund's quarterly raw return; *FFC Alpha* is the cumulative Fama-French-Carhart (FFC) quarterly alpha in percent; *flow* is the quarterly fund inflow in percent, constructed as in Equation (3); *FFC Alpha volatility* is the volatility of monthly FFC Alpha measured over 12 months in percent; *flow volatility* is the volatility of monthly fund inflows measured over 12 months in percent.

Panel A: Summary statistics

	Mean	St. Dev.	5th Pct.	Median	95th Pct.
Mutual fund characteristics					
Fund TNA	1773	6514	17	326	6996
Team-managed	0.66	0	0	1	1
Number of managers	2.69	3	1	2	7
Manager Tenure	34	25	4	28	82
Number of stocks	95	178	11	58	260
Extrapolation Beta	-0.094	0.226	-0.514	-0.050	0.181
Extrapolation Beta (No Mom)	-0.158	0.295	-0.702	-0.110	0.224
Extrapolation Beta (Op Profit)	-0.003	0.118	-0.098	0.000	0.083
Extrapolation Dummy	0.33	0.47	0	0	1
Expense Ratio	0.012	0.004	0.006	0.011	0.019
Fund Turnover	0.753	0.704	0.110	0.570	1.980
Mutual fund performance					
Fund Return	2.198	10.848	-15.545	2.996	15.403
FFC Alpha	-0.281	6.408	-4.486	-0.330	3.886
Flow	0.452	36.475	-12.588	-1.619	15.954
FFC Alpha volatility	1.460	3.653	0.528	1.188	2.994
Flow volatility	12.012	712.408	0.357	1.657	11.267

Panel B: Correlation table

Fund TNA	1															
Team-managed	0.01	1														
Number of managers	0.15	0.47	1													
Manager Tenure	0.09	-0.10	-0.05	1												
Number of stocks	0.08	0.06	0.20	-0.04	1											
Extrapolation Beta	-0.06	-0.02	-0.02	-0.06	0.03	1										
Extrapolation Beta (No Mom)	-0.06	-0.02	-0.03	-0.08	0.02	0.88	1									
Extrapolation Beta (Op Profit)	0.01	0.00	0.00	0.01	0.01	0.04	0.04	1								
Extrapolation Dummy	-0.04	0.01	-0.03	-0.02	-0.06	0.60	0.55	0.00	1							
Expense Ratio	-0.23	-0.07	-0.11	-0.06	-0.19	0.07	0.08	-0.01	0.01	1						
Fund Turnover	-0.12	-0.05	-0.03	-0.14	-0.01	0.20	0.23	0.02	0.07	0.17	1					
Fund Return	0.01	0.00	0.00	0.01	0.00	-0.01	0.00	0.01	-0.01	0.01	-0.01	1				
FFC Alpha	0.01	-0.01	-0.01	0.00	0.00	-0.01	-0.01	0.01	-0.02	-0.01	-0.01	0.45	1			
Flow	0.00	-0.01	0.00	-0.01	0.00	-0.03	-0.02	0.00	-0.02	0.00	-0.01	-0.02	-0.07	1		
FFC Alpha volatility	-0.02	-0.03	-0.03	0.00	-0.04	-0.01	0.00	-0.04	-0.01	0.08	0.05	0.11	0.18	-0.04	1	
Flow volatility	0.00	0.00	0.03	0.00	0.00	0.00	0.00	0.00	-0.01	0.00	0.00	0.00	0.00	0.01	0.00	1

Table 2. **Extrapolation as a bias – evidence from investment performance**: This table reports regressions of future quarterly fund performance (Panel A) and risk (Panel B) on the lagged fund's extrapolative behavior. Such behavior is identified by means of a dummy variable that is equal to 1 if the recursively estimated fund's extrapolation beta is positive, and 0 otherwise. In Panel A, we analyze fund performance using the following metrics: (i) the future fund's alpha, measured with respect to a Fama-French-Carhart (FFC) 4-factor model (×100); (ii) the probability that a fund's style-adjusted return is in the top 10% of the cross-sectional distribution of fund returns; (iii) fund flows (×100). In Panel B, we analyze the implications of extrapolative behavior for risk using two metrics. These are: (i) the volatility of a fund's monthly Alpha (×100); (ii) the volatility of monthly flows (×100). We perform the analysis with and without controls. Controls included in the multivariate regressions are: the fund's expense ratio; the number of stocks in the fund's portfolio (divided by 100); the fund's turnover; the average manager tenure in quarters (divided by 10); the log of the fund's total net assets (TNA); lagged quarterly fund inflows. All regressions also control for style-quarter fixed effects, using Morningstar fund's style classifications. Standard errors are double clustered by quarter and at the fund level, and reported in brackets. More details on the data is in Appendix B.2. Significance: ***99%, **95%, *90%.

Panel A: Managerial Performance

	FFC Alpha		**Top 10% Fund**		**Flow**	
	(1)	(2)	(3)	(4)	(5)	(6)
Extrapolation Dummy (t-1)	-0.134***	-0.120***	-0.014***	-0.016***	-1.003***	-1.152***
	[0.043]	[0.042]	[0.004]	[0.004]	[0.261]	[0.269]
Expense Ratio (t-1)		-17.112		2.123***		21.909
		[11.818]		[0.672]		[50.733]
N Stocks (t-1)		0.0010		-0.009***		0.151***
		[0.007]		[0.001]		[0.047]
Fund Turnover (t-1)		-0.118**		0.002		-0.408
		[0.052]		[0.004]		[0.501]
Avg. Manager Tenure (t-1)		0.0220		0.0010		-0.0800
		[0.020]		[0.001]		[0.065]
Log Fund TNA (t-1)		-0.088		-0.018***		-2.998***
		[0.065]		[0.005]		[1.027]
Log Fund TNA^2 (t-1)		0.007		0.001***		0.201***
		[0.005]		[0.000]		[0.074]
Flow Volatility (t-1)		0.103**		0.027***		0.2040
		[0.051]		[0.008]		[0.169]
Flow (t-1)		0.001*		0		
		[0.001]		[0.000]		
FFC Alpha (t-1)						0.097
						[0.066]
Time × Style FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	78911	71555	89937	77889	89937	75713
Adj. R-squared	0.0737	0.0727	-0.0031	0.0054	0.0113	0.0038

Panel B: Managerial Risk

	FFC Alpha Volatility		**Flow Volatility**	
	(1)	(2)	(3)	(4)
Extrapolation Dummy (t-1)	0.013	0.010	-11.019	-14.419
	[0.033]	[0.034]	[7.554]	[10.492]
Expense Ratio (t-1)		38.167***		233.199
		[6.886]		[651.257]
N Stocks (t-1)		-0.065***		-0.298
		[0.012]		[0.211]
Fund Turnover (t-1)		0.097***		-1.209
		[0.034]		[1.223]
Avg. Manager Tenure (t-1)		0.023*		-1.384
		[0.012]		[1.132]
Log Fund TNA (t-1)		-0.143**		14.672
		[0.064]		[13.561]
Log Fund TNA^2 (t-1)		0.012**		-1.352
		[0.005]		[1.140]
FFC Alpha (t-1)		0.012**		0.121
		[0.005]		[0.155]
Flow (t-1)		0		-0.027
		[0.000]		[0.027]
Flow Volatility (t-1)		0.0000		
		[0.000]		
Time × Style FE	Yes	Yes	Yes	Yes
Observations	64744	58055	71544	58285
Adj. R-squared	0.0354	0.0435	-0.0089	-0.0086

Table 3. **Summary statistics restricted sample**: This table reports the summary statistics for the restricted sample we use to conduct our main analysis. *TNA* is the fund's total net asset value, in million dollars; *manager tenure* is the average tenure of the solo managers or teams in quarters; the fund *extrapolation beta* is the extrapolation metric defined in Section 2.2; *extrapolation dummy* is equal to 1 if the fund's extrapolation metric is positive; *expense ratio* is the fund's total expense ratio; *fund return* is the fund's quarterly raw return. Panel A reports statistics for the solo-managed funds and their managers, and Panel B reports similar statistics for team-based funds. The sample period is 1980Q1-2018Q4.

Panel A: Solo managers

	Mean	St. Dev	5th Pct.	Median	95th Pct.
Change in holdings and stock returns					
Change portfolio weight (p.p.)	-0.003	0.903	-0.489	0	0.439
Last year return	0.199	0.721	-0.521	0.111	1.118
Mutual fund characteristics					
Fund TNA	1332	5333	4	166	4877
Number of managers	1	0	1	1	1
Manager Tenure	39	26	5	34	89
Number of stocks	92	220	2	56	228
Extrapolation Beta	-0.053	0.169	-0.336	-0.032	0.196
Extrapolation Dummy	0.36	0.48	0	0	1
Expense Ratio	0.012	0.005	0.006	0.012	0.020
Fund Return	0.030	0.095	-0.140	0.023	0.176

Panel B: Teams

	Mean	St. Dev	5th Pct.	Median	95th Pct.
Change in holdings and stock returns					
Change portfolio weight $\times 100$	-0.003	0.814	-0.539	0	0.487
Last year return	0.156	0.676	-0.570	0.085	1.033
Mutual fund characteristics					
Fund TNA	865	2330	7	230	3231
Number of managers	2.16	0.40	2	2	3
Manager Tenure	36	26	4	31	88
Number of stocks	88	187	4	56	222
Extrapolation Beta	-0.063	0.164	-0.351	-0.043	0.135
Extrapolation Dummy	0.31	0.46	0	0	1
Expense Ratio	0.012	0.004	0.006	0.012	0.020
Fund Return	0.027	0.089	-0.136	0.025	0.161

Table 4. **Extrapolation bias in solo versus team-managed funds**: This table compares the extrapolation beta of the team, $\hat{\beta}_j^{TM}$, with its statistical counterfactual $\hat{\beta}_j^{CF}$, i.e., the average beta observed among team members when they manage a fund alone. We report the results for all teams combined (Panel A), for contrarian teams (Panel B), and for extrapolative teams (Panel C). A team consists mainly of contrarians if $\hat{\beta}_j^{CF} \leq 0$ and of extrapolators if $\hat{\beta}_j^{CF} > 0$.

Panel A: All teams

	Mean	s.e.	t-stat	Obs.
$\hat{\beta}_j^{CF}$	-0.051	0.007	-7.730	350
$\hat{\beta}_j^{TM}$	-0.063	0.009	-7.171	350
Difference $\hat{\beta}_j^{CF}$ - $\hat{\beta}_j^{TM}$	0.012	0.009	1.369	350

Panel B: Contrarian teams

	Mean	s.e.	t-stat	Obs.
$\hat{\beta}_j^{CF}$	-0.105	0.007	-15.025	236
$\hat{\beta}_j^{TM}$	-0.094	0.011	-8.914	236
Difference $\hat{\beta}_j^{CF}$ - $\hat{\beta}_j^{TM}$	-0.011	0.011	-1.066	236

Panel C: Extrapolative teams

	Mean	s.e.	t-stat	Obs.
$\hat{\beta}_j^{CF}$	0.062	0.006	11.082	114
$\hat{\beta}_j^{TM}$	0.002	0.014	0.122	114
Difference $\hat{\beta}_j^{CF}$ - $\hat{\beta}_j^{TM}$	0.060	0.015	3.958	114

Table 5. **Transmission of extrapolation bias from individuals to teams**: In this table we estimate the transmission of extrapolation bias from solo managers to their respective teams. To this end we estimate the following regression: $\hat{\beta}_j^{TM} = \alpha + \hat{\beta}_j^{CF}(\delta_0 + \delta_1 D_j^E) + \delta_2 D_j^E + \delta_3 C_j + \epsilon_j$. In the regression, $\hat{\beta}_j^{TM}$ measures the team's actual extrapolative behavior, while $\hat{\beta}_j^{CF}$ is the team's counterfactual extrapolative behavior, based on team members' trading behavior when managing alone. D_j^E is an indicator variable that is equal to 1 when the members of the team exhibit extrapolative behavior on average when managing a fund alone. Team controls (C_j) are the time-series average log TNA of the team-managed portfolio, the time-series average log tenure of the team members, and style fixed effects, and are included as reported. Columns (1) to (4) perform the analysis using OLS. Columns (1) and (2) estimate a simpler model without any interaction term. Thus, in these columns, the coefficient δ_0 captures the transmission of all individual-level return-based trading behavior (be it extrapolative or contrarian) to the team. Column (3) and (4) estimate the full model. In these columns, we analyze separately the transmission of individual contrarian behavior (δ_0), the transmission of extrapolative behavior ($\delta_0+\delta_1$), and the difference between the two (δ_1). In Columns (5) to (8) the analysis is performed based on an IV methodology described in Section 4.3. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	OLS				**IV**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\beta}_j^{CF}$	0.4892***	0.4293***	0.5096***	0.4480***	0.696***	0.619***	0.857***	0.828**
	[0.0665]	[0.0682]	[0.0920]	[0.0947]	[0.1578]	[0.1660]	[0.3225]	[0.3413]
$\hat{\beta}_j^{CF} \times D_j^E$			-0.6030**	-0.5965**			-0.697**	-0.629**
			[0.2563]	[0.2605]			[0.3198]	[0.3144]
D_j^E			0.0480*	0.0500**			0.018	-0.003
			[0.0248]	[0.0249]			[0.0965]	[0.0996]
Style fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	350	350	350	350	350	350	350	350
Adj. R-squared	0.132	0.1625	0.1434	0.1739	0.0173	0.0687	0.0223	0.0694
Hypothesis testing:								
p-value $H_0 : \delta_0 = 1$	0.000	0.000	0.000	0.000	0.054	0.022	0.657	0.614
p-value $H_0 : \delta_0 + \delta_1 = 0$			0.696	0.537			0.705	0.614
p-value $H_0 : \delta_0 + \delta_1 = 1$			0.000	0.000			0.045	0.043

Table 6. **Robustness to the exclusion of momentum stocks**: We re-estimate managers' extrapolative behavior after excluding stocks that are part of the momentum strategy (Jegadeesh and Titman 1993, 2001). In each month t, a stock is classified as part of the momentum strategy in month t, and hence removed from the estimation of managers' extrapolative betas, if its cumulative 11-month return between the end of month $t-12$ and the end of $t-1$ is in the top or the bottom 10% of the cross-sectional distribution of stocks. Using this alternative estimation of managers' extrapolative behavior, in Panel A we repeat our analysis from Table 2 on the linkage between extrapolation and fund performance. In Panel B we use our alternative extrapolation metric to repeat the analysis in Table 5 on how extrapolative behavior is transmitted from individual team members to teams. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

Panel A: Fund performance and risk

Dependent Variable:	**FFC Alpha** (1)	**Top 10% Fund** (2)	**Flow** (3)	**FFC Alpha Vol.** (4)	**Flow Vol.** (5)
Extrapolation Dummy (t-1)	-0.065*	-0.012***	-1.191***	0.067*	-14.073
	[0.033]	[0.004]	[0.264]	[0.040]	[17.862]
Expense Ratio (t-1)	-16.8590	2.153***	23.844	38.163***	256.576
	[11.864]	[0.673]	[50.549]	[6.888]	[664.192]
N Stocks (t-1)	0.0010	-0.009***	0.149***	-0.064***	-0.314
	[0.007]	[0.001]	[0.046]	[0.012]	[0.227]
Fund Turnover (t-1)	-0.120**	0.0020	-0.404	0.095***	-1.217
	[0.052]	[0.004]	[0.501]	[0.034]	[1.079]
Avg. Manager Tenure (t-1)	0.023	0.001*	-0.076	0.023*	-1.3410
	[0.020]	[0.001]	[0.065]	[0.012]	[1.152]
Log Fund TNA (t-1)	-0.092	-0.018***	-3.026***	-0.144**	14.348
	[0.065]	[0.005]	[1.028]	[0.064]	[13.443]
Log Fund TNA^2 (t-1)	0.007	0.001***	0.203***	0.012**	-1.325
	[0.005]	[0.000]	[0.074]	[0.005]	[1.134]
Flow Volatility (t-1)	0.001**	0.000***	0.002	0.0000	
	[0.000]	[0.000]	[0.002]	[0.000]	
Flow (t-1)	0.001*	0		0	-0.026
	[0.001]	[0.000]		[0.000]	[0.028]
FFC Alpha (t-1)			0.097	0.012**	0.136
			[0.066]	[0.005]	[0.163]
Time × Style FE	Yes	Yes	Yes	Yes	Yes
Observations	71555	77889	75713	58055	58285
Adj. R-squared	0.0726	0.0051	0.0038	0.0435	-0.0086

Panel B: Transmission of extrapolation from solo to team-managed funds

	OLS		**IV**	
	(1)	(2)	(3)	(4)
$\hat{\beta}_j^{CF}$	0.3686***	0.4078***	0.434***	0.905***
	[0.0716]	[0.0942]	[0.1447]	[0.2649]
$\hat{\beta}_j^{CF} \times D_j^E$		-0.6156*		-0.968***
		[0.3731]		[0.3500]
D_j^E		0.042		-0.109
		[0.0412]		[0.1122]
Team TNA	-0.002	-0.003	0.000	-0.005
	[0.0073]	[0.0074]	[0.0073]	[0.0077]
Team tenure	0.000	0.000	0.000	0.000
	[0.0008]	[0.0008]	[0.0008]	[0.0009]
Style fixed effects	Yes	Yes	Yes	Yes
Observations	350	350	350	350
Adj. R-squared	0.1508	0.1526	0.1330	0.1248
p-value $H_0 : \delta_0 = 1$	0.000	0.000	0.000	0.719
p-value $H_0 : \delta_0 + \delta_1 = 0$		0.561		0.822
p-value $H_0 : \delta_0 + \delta_1 = 1$		0.001		0.000

Table 7. **Robustness to differences in experience, style, and workload**: In Panel A, we show summary statistics for the experience, style, and workload measures as specified in Sections 5.2-. In Panel B, we estimate the following regression: $\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^E \times \Delta Tenure_j + \delta_2 C_j + \epsilon_j$. Using managerial tenure as a proxy for trading experience, the main coefficient of interest in the regression is δ_1, which measures whether the attenuation of extrapolation bias is stronger for teams whose members have accrued more experience by the time they join the team. For more details on data construction, see Section 5.2. In Panel C, we estimate the following regression: $\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^E \times D_j^{SM} + \delta_2 C_j + \epsilon_j$. The main coefficient of interest is δ_1, which measures whether the attenuation of extrapolation bias is more pronounced for teams whose members experience style migration when moving from individually managed funds to a team-managed one. For more details on data construction, see Section 5.3. In Panel D, we estimate the following regression: $\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^E \times \Delta Workload_j + \delta_2 C_j + \epsilon_j$. The main coefficient of interest is δ_1, which measures whether the attenuation of extrapolation bias is more pronounced for teams whose members experience a decrease in workload when moving from individually managed funds to a team-managed one. For more details on data construction, see Section . In addition to the reported additional regressors, team-level controls include the time-series average log TNA of the team-managed portfolio, the time-series average log tenure of the team members, and style fixed effects. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

Panel A: Summary statistics

	Obs.	Mean	St. Dev.	Min	Max
All teams					
$\Delta Tenure_j$	350	1.73	18.16	-49.6	87.64
D_j^{SM}	350	0.24	0.43	0	1
$\Delta Workload_j$	350	-39	113	-408	481
Contrarian teams					
$\Delta Tenure_j$	236	3.56	18.18	-45.41	87.64
D_j^{SM}	236	0.25	0.44	0	1
$\Delta Workload_j$	236	-36	118	-408	481
Extrapolative teams					
$\Delta Tenure_j$	114	-2.07	17.58	-49.60	42.75
D_j^{SM}	114	0.21	0.41	0	1
$\Delta Workload_j$	114	-46	101	-406	299

Panel B: Differential Experience

	(1)	(2)
$\hat{\beta}_j^{CF}$	0.4905***	0.4297***
	[0.0988]	[0.1012]
$\hat{\beta}_j^{CF} \times D_j^E \times \Delta Tenure_j$	-0.0049	-0.0014
	[0.0190]	[0.0187]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.5829**	-0.5846**
	[0.2603]	[0.2643]
$\hat{\beta}_j^{CF} \times \Delta Tenure_j$	0.0033	0.0029
	[0.0058]	[0.0058]
$D_j^E \times \Delta Tenure_j$	-0.0004	-0.0002
	[0.0016]	[0.0016]
D_j^E	0.0498**	0.0517**
	[0.0253]	[0.0253]
$\Delta Tenure_j$	0.0005	0.0002
	[0.0009]	[0.0009]
Style FE	No	Yes
Controls	No	Yes
Observations	350	350
Adj. R-squared	0.1343	0.1647

Panel C: Style migrations

	(3)	(4)
$\hat{\beta}_j^{CF}$	0.5666***	0.5049***
	[0.1028]	[0.1045]
$\hat{\beta}_j^{CF} \times D_j^E \times D_j^{SM}$	0.8028	0.719
	[0.7815]	[0.7771]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.7219***	-0.6885**
	[0.2717]	[0.2762]
$\hat{\beta}_j^{CF} \times D_j^{SM}$	-0.3667	-0.3695
	[0.2243]	[0.2252]
$D_j^E \times D_j^{SM}$	-0.0941	-0.0696
	[0.0636]	[0.0642]
D_j^E	0.0698**	0.0662**
	[0.0276]	[0.0277]
D_j^{SM}	0.0235	0.0152
	[0.0309]	[0.0315]
Style FE	No	Yes
Controls	No	Yes
Observations	350	350
Adj. R-squared	0.1611	0.1853

Panel D: Differential Workload

	(5)	(6)
$\hat{\beta}_j^{CF}$	0.5347***	0.4606***
	[0.1018]	[0.1050]
$\hat{\beta}_j^{CF} \times D_j^E \times \Delta Workload_j$	-0.0026	-0.0019
	[0.0036]	[0.0036]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.7093**	-0.6862**
	[0.3242]	[0.3309]
$\hat{\beta}_j^{CF} \times \Delta Workload_j$	0.0007	0.0002
	[0.0014]	[0.0014]
$D_j^E \times \Delta Workload_j$	0.0002	0.0002
	[0.0002]	[0.0002]
D_j^E	0.0564**	0.0591**
	[0.0269]	[0.0271]
$\Delta Workload_j$	0.0002	0.0001
	[0.0001]	[0.0001]
Style FE	No	Yes
Controls	No	Yes
Observations	350	350
Adj. R-squared	0.1465	0.1752

Table 8. **Robustness to differences in compensation structures**: This table compares the compensation structures of the team with its statistical counterfactual. The contract incentives we consider are four dummy variables that are equal to one if managers have (i) a fixed compensation, (ii) compensation based on the performance of the fund, (iii) compensation based on the AUM of the fund, and (iv) share ownership in their own funds, and zero otherwise. For more details on data construction, see Section 5.5. We report the average extrapolation beta and compensation metrices for the counterfactual, the team, and for the difference between the counterfactual and the team (CF - team). Furthermore, we report the results for all teams combined (Panel A), for contrarian teams (Panel B), and for extrapolative teams (Panel C). A team consists mainly of contrarians if $\hat{\beta}_j^{CF} \leq 0$ and of extrapolators if $\hat{\beta}_j^{CF} > 0$.

Panel A: All teams

	Extrapolation beta		**Fixed pay**		**Performance pay**		**AUM pay**		**Ownership**	
	CF	team	CF	team	CF	team	CF	team	CF	team
Mean	-0.057	-0.070	0.031	0.039	0.837	0.858	0.197	0.176	0.694	0.579
s.e.	0.012	0.014	0.014	0.015	0.028	0.029	0.031	0.032	0.031	0.037
CF - team		0.013		-0.009		-0.021		0.021		0.115
t-stat		1.054		-1.014		-1.211		0.966		3.420
Obs.		133		131		130		130		126

Panel B: Contrarian teams

	Extrapolation beta		**Fixed pay**		**Performance pay**		**AUM pay**		**Ownership**	
	CF	team	CF	team	CF	team	CF	team	CF	team
Mean	-0.115	-0.110	0.034	0.037	0.820	0.852	0.204	0.179	0.708	0.615
s.e.	0.011	0.018	0.016	0.018	0.034	0.034	0.037	0.037	0.037	0.043
CF - team		-0.006		-0.004		-0.032		0.025		0.093
t-stat		-0.349		-0.488		-1.394		0.910		2.234
Obs.		90		89		89		89		86

Panel C: Extrapolative teams

	Extrapolation beta		**Fixed pay**		**Performance pay**		**AUM pay**		**Ownership**	
	CF	team	CF	team	CF	team	CF	team	CF	team
Mean	0.066	0.013	0.024	0.044	0.872	0.870	0.181	0.171	0.662	0.500
s.e.	0.012	0.014	0.017	0.027	0.044	0.046	0.051	0.057	0.046	0.065
CF - team		0.053		-0.020		0.002		0.010		0.162
t-stat		3.084		-0.896		0.103		0.334		2.865
Obs.		43		42		41		41		40

Table 9. **Mechanism: Internal reflection or external screening?**: The internal reflection hypothesis and the external screening hypothesis make different predictions concerning how team composition affects the transmission of individual-level behavior to the team. To investigate the role of team composition, we estimate the following regression: $\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^{AE} + \delta_2 \hat{\beta}_j^{CF} \times D_j^{M} + \delta_3 C_j + \epsilon_j$. In the regression, D_j^{AE} is an indicator variable that is equal to 1 for a team whose members all extrapolate in their solo-managed funds. D_j^{M} is instead an indicator variable that is equal to 1 for a team in which some members exhibit extrapolative behavior in their solo funds, and some members display contrarian behavior. Team-level controls include the time-series average log TNA of the team-managed portfolio, the time-series average log tenure of the team members, and team's style dummies. We also include the p-value for the test $\delta_1 = \delta_2$. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	(1)	(2)	(3)
$\hat{\beta}_j^{CF}$	0.5841***	0.5865***	0.5089***
	[0.1100]	[0.1108]	[0.1157]
$\hat{\beta}_j^{CF} \times D_j^{AE}$	-0.9805***	-1.0019***	-0.9193**
	[0.3588]	[0.3626]	[0.3636]
$\hat{\beta}_j^{CF} \times D_j^{M}$	-0.2055	-0.2135	-0.1655
	[0.1902]	[0.1907]	[0.1905]
D_j^{AE}	0.0614	0.0604	0.0643
	[0.0405]	[0.0406]	[0.0406]
D_j^{M}	-0.0026	-0.0037	0.001
	[0.0233]	[0.0234]	[0.0241]
Team controls	No	Yes	Yes
Style FE	No	No	Yes
Observations	350	350	350
Adj. R-squared	0.1428	0.1402	0.1706
Hypothesis testing:			
p-value $H_0 : \delta_1 = \delta_2$	0.0395	0.0378	0.0464

Appendix A Preparing the mutual fund dataset

In this appendix, we explain how we merged the CRSP, Morningstar and Thomson Reuters databases, as well as the construction of the control variables used in the paper. We start with the cleaning of the raw CRSP database and Morningstar database separately, followed by a detailed explanation of the merge between the two. We then explain how we match this to mutual fund holdings data from s12 filings, obtained from CRSP and Thomson Reuters.

A.1 Cleaning raw CRSP database

We select the CRSP fund share classes that are defined as domestic equity, but exclude the index funds. We download the monthly returns (mtna), size per share class (mtna), tickers (ticker), and cusip numbers (ncusip). We first delete observations for which total net asset values or returns are missing within a given month. The number of observations then equals 2,049,075 and there are 17,059 unique fund share classes (*CRSP fundno*'s).

We forward- and backward-fill the tickers within each fund share class. We then perform the following four checks:

1. *We check if a CRSP fundno has multiple tickers in a given month.* There are no such cases.

2. *We check if a CRSP fundno has multiple tickers over the entire sample period.* There are 960 *CRSP fundno*'s with time-varying tickers. We use the latest ticker for each *CRSP fundno* available, following Pástor et al. (2015).

3. *We check if a ticker has multiple CRSP fundno's in a given month.* There are 5,779 combinations of ticker and month that correspond to more than one *CRSP fundno*. As in Pástor et al. (2015), we replace these cases with a missing value.

4. *We check if a ticker has multiple CRSP fundno's over the sample.* There are 599 tickers with multiple *CRSP fundno*'s. These cases are automatically taken care of in

the merge.

We follow the exact same procedure for cusip numbers. We list here the number of cases to which 1-4 apply in case of cusip numbers:

1. There are no such cases.

2. There are 3,736 *CRSP fundno*'s with time-varying cusips.

3. There are 3,459 combinations of cusip and month that correspond to more than one *CRSP fundno*.

4. There are 49 cusips with multiple *CRSP fundno*'s.

A.2 Cleaning raw Morningstar database

We select the domestic equity Morningstar funds, excluding index funds, and download the monthly returns, size per share class, tickers, and cusip numbers. We first delete observations for which total net asset values or returns are missing within a given month. The number of observations then equals 1,838,776 and there are 15,947 unique fund share classes (*SecId*).

We again apply the four checks as we did for the CRSP database for both tickers and cusips. We summarize the number of cases to which 1-4 apply here:

1. There are no such cases for both ticker and cusip.

2. There is no *SecId* that has time-varying tickers or cusips. A *SecId* either never has a ticker (cusip) over the entire sample period or a *SecId* has the same ticker (cusip) over the entire sample period.

3. There are 547 combinations of ticker and month that correspond to more than one *SecId* for ticker and 650 for cusip and month.

4. There are 23 tickers with multiple *SecId*s for ticker and 3 for cusip.

A.3 Matching CRSP and Morningstar databases

The CRSP database is our master file and we merge this database to Morningstar using first a match based on ticker and then a match based on cusip (the results of the merge are exactly the same if we first merge based on cusip and then on ticker). In order to make sure that missing values are never matched, we replace the ticker (cusip) with the *CRSP fundno* number in CRSP and with the *SecId* in Morningstar (Berk and van Binsbergen 2015).

After we merge based on cusip and ticker, we also merge CRSP and Morningstar based on an exact match between year, month, monthly return, and monthly total net asset value. After that, we perform two near merges:

1. Exact match based on year, month, and total net asset value and a difference in monthly returns in the CRSP versus the Morningstar database that is at most two basis points.

2. Exact match based on year, month, and monthly return and a difference in total net asset value that is at most 20,000 USD.

We then correct for potential errors in the merge as some tickers and cusip numbers may be reused. We first check whether the same unique share class identifier from CRSP, *CRSP fundno*, consistently matches the Morningstar unique identifier for the history of that share class. Following Berk and van Binsbergen (2015), we drop all funds where the same share classes are matched less than 60% of the time. On the other hand, if a given share class is matched more than 60% of the time, we assume that this match is the correct match and change the observations that don't match accordingly.

We then use the Morningstar *FundId* to group funds that have multiple share classes and check whether we are able to match all of the share classes of a given fund. Following Pástor et al. (2015), if we are not able to find a full match, we drop those observations and end up with a final dataset of 580,06 observations. The merge matches 82% of our initial CRSP active US domestic equity universe.

At the end of this process we have a key that allows us to match any given unique CRSP share class number and portfolio number, *CRSP fundno* and *CRSP portno*, to a Morningstar portfolio level number, *FundId*. This key is then used to match funds to fund information from CRSP such as fund TNA, expense ratios, and returns, fund holdings from Thomson Reuters and CRSP, and fund managers from Morningstar.

A.4 Merging mutual fund holdings data

The next step in constructing our dataset is to match funds to their respective fund holdings. For this part we use two sources, the Thomson Reuters s12 Holdings file for mutual funds and the CRSP s12 Mutual Fund Holdings database. For the first part of the sample, from 1980 to 2008, we use Thomson Reuters s12 Holdings and we use CRSP after that.

Merging is then straightforward. We are able to match the CRSP Mutual Fund Holdings to our master data file using their unique fund identifiers which are present in both files, and we use the MFLINKS dataset from Wharton Research Data Services to match Thomson Reuters data to our master file that links Thomson Reuters fund identifiers to CRSP identifiers. We then collapse stock holdings every quarter at the portfolio level by adding all shares of a given stock for the fund's Morningstar FundId.

After matching the holdings, using stock CUSIP numbers from CRSP, we link mutual funds' holdings to the stock-level information (prices, returns, book-to-market, profitability, investments) contained in the merged CRSP-COMPUSTAT database. We consider the universe of stocks with codes 10 and 11 that trade on the NYSE, NASDAQ and AMEX exchanges, and we exclude stocks trading below $5. Finally, we link each mutual fund to their respective managers. This renders a dataset that contains manager-fund-stock-quarter holdings data.

Appendix B Control variable description

B.1 Control variables for measuring extrapolation

To measure extrapolation both at the manager and at the fund level, we control for several stock characteristics that have been associated to either pricing anomalies or risk premia. To the extent that rational managers want to gain exposure to these characteristics and that these correlate with past 12-month returns, including such characteristics as controls allows us to more accurately identify extrapolation.

We first include size and book to market ratios as controls in Equation (1). Small and value firms, which are respectively measured by a small firm size and high book to market ratio, have been widely documented as having historically high abnormal returns (Fama and French 1993). We follow up by also including asset growth and operating profitability as measured in Cooper et al. (2008) and Fama and French (2015), respectively. Regarding these characteristics, Cooper et al. (2008) document a negative relationship between past asset growth and future stock returns while Novy-Marx (2013) documents a positive relationship between firm profitability and future stock returns. Finally, we also include stock volatility over the past 12-months as highly volatile stocks have been associated with low expected returns (Ang et al. 2006).

Finally, another reason to include these characteristics is the documented preferences of institutional investors and their demand for certain characteristics. Gompers and Metrick (2001) document how several characteristics are determinants of stock institutional ownership, finding that institutions have a particular demand for larger firms. More recently, Koijen and Yogo (2019) document how different types of institutions differ in their demand elasticities for stock characteristics such as size, book to market, profitability, and investment (asset growth).

B.2 Mutual fund level control variables

When studying how mutual fund manager performance depends on managerial biases, it is important to control for other variables that are related to both factors to ensure our results are not driven by omitted variables. We start by controlling for fund expense ratios. This is directly tied to fund performance because we evaluate performance net of fees. In addition to this, expense ratios can be related to manager skill as motivated in Berk and Green (2004), because self-interested skilled managers can raise fees to capture the benefits of their skill. However, empirically, Gil-Bazo and Ruiz-Verdú (2009) find that expense ratios are negatively related to performance as a result of managers strategically setting fees when investors are not sensitive to performance.

Importantly, we control for manager characteristics such as their tenure and trading behavior which can be related to their tendency to extrapolate returns and have been extensively documented as having a relationship with fund performance. Golec (1996) and Chevalier and Ellison (1999) document a positive relationship between manager tenure and fund performance. To the extent that experienced managers are less likely to suffer from behavioral biases, manager tenure controls for this effect. We also control for trading behavior by including mutual fund portfolio turnover and number of stocks held in the mutual fund portfolio. This is motivated from evidence that high trading activity is positively related to performance as documented by Wermers (2000) and Cremers and Petajisto (2009).

Finally, we also control for other common fund characteristics such as fund size, fund flows and fund volatility. The positive relationship between fund size and skill is well documented in Berk and van Binsbergen (2015) and recent empirical research has also established that funds have decreasing returns to scale (Pástor et al. 2015; McLemore 2018). Furthermore, we also control for past fund flows and fund flow volatility, because funds with high unexpected outflows can experience high trading costs due to fund liquidity constraints (Coval and Stafford 2007; Rakowski 2010) and high flow volatility can negatively impact performance due to increased trading costs (Rakowski 2010).

Appendix C The transmission of extrapolation bias to teams: Simulation Results

In this section we simulate data to demonstrate how our instrumental variables (IV) approach is able to produce unbiased estimates of the transmission of individual behavior to the team. We find that our IV estimator successfully deals with the errors-in-variables problem, whereas a standard OLS estimator produces estimates that are biased towards zero.

In the final section of this appendix, we also use our manager stock holdings dataset to show that our main results are not driven by one particular lucky draw. Given that our IV approach depends on a random split of manager holdings, we show a distribution of IV estimates as a robustness check to confirm our evidence that teams dampen bias transmission of individual behavior. After producing 2,500 random draws and re-estimating our main results, we find that our conclusions remain unchanged.

C.1 Data generating process

We simulate data to demonstrate that measurement error has a downward bias on the OLS estimates and that our instrumental variable (IV) approach delivers unbiased estimates of the transmission coefficient. We start by simulating the returns of 100 stocks over 40 quarters, which matches the average number of holdings and duration of the mutual fund portfolios that we observe in our sample. Returns for each quarter are simulated with a single factor structure:

$$r_{s,t} = \gamma_s r_{m,t} + \varepsilon_{s,t}, \tag{10}$$

where γ_s is the factor exposure of stock s to the single factor $r_{m,t}$, and $\varepsilon_{s,t}$ is the idiosyncratic return of stock s. For each stock, we randomly draw γ_s from a Normal distribution $\mathcal{N}(1, 0.5)$, factor returns $r_{m,t}$ from a Normal distribution $\mathcal{N}(0.0175, 0.1)$, and the idiosyncratic return from a Normal distribution $\mathcal{N}(0, 0.15)$. These parameter choices translate to an average yearly return of $\mu = 7\%$ with annualized volatility $\sigma = 20\%$. To draw a parallel with our empirical setting, we then calculate the rolling 1 year returns of each stock to generate

manager trades based on a stock's past return over the last year: $r_{s,t-4\to t}$.

We simulate the trades of 600 managers according to our main empirical specification, where managers change their holdings by using past yearly stock returns:

$$\Delta w_{s,i,t+1} = \beta_i^{SM} r_{s,t-4\to t} + e_{i,s,t+1}, \tag{11}$$

where β_i^{SM} equals the true extrapolation beta of manager $i = 1, \ldots, N$, $r_{s,t-4\to t}$ the annual past return on stock s, and $e_{i,s,t+1}$ the noise term.

To simulate the manager trades, we draw the true extrapolation betas from the empirical distribution of the extrapolation betas in our sample. Formally, we assume that the individual manager betas are drawn from a normal distribution with mean $\mu_\beta = -0.05$ and standard deviation $\sigma_\beta = 0.16$. This means that we have both managers that extrapolate from past returns as well as managers that take contrarian positions relative to past stock returns. Because we standardize the changes in holdings in our empirical setting, we then simulate the noise term as a standard normal distribution with mean $\mu_e = 0$ and standard deviation $\sigma_e = 1$.

We then move on to generate teams of managers and simulate the team trades. We assume that managers are teamed up in non-overlapping pairs such that managers 1 and 2 form a team, 3 and 4, and subsequently managers 599 and 600. Each team trades in the same way as the average of the individual managers, except that their reliance on past returns is mitigated with a fraction κ_0. Formally, we define the changes in the holdings of team $j = 1, \ldots, J$ and its corresponding team beta as:

$$\Delta w_{s,j,t+1} = \beta_j^{TM} r_{s,t} + e_{j,s,t+1}, \tag{12}$$

where

$$\beta_j^{TM} = \kappa_0 \bar{\beta}_j^{SM} + \upsilon_j, \tag{13}$$

$$\bar{\beta}_j^{SM} \equiv \frac{\sum_{i=1|j}^{2} \beta_{i|j}^{SM}}{2}, \tag{14}$$

where $\beta_{i|j}^{SM}$ is the extrapolation beta of manager i that is in team j and υ_j is a noise term with a $\mathcal{N}(0, 0.1)$ distribution.

Equation (13) implies that the team beta is the average beta of the managers composing the team scaled by the transmission coefficient κ_0. When $\kappa_0 = 1$, there is full transmission of heuristic rules as the team manages the portfolio as if each individual manager were trading independently managed portfolios. However, when $\kappa_0 < 1$, team-managed portfolios have a lower tendency to use past stock returns as information for their trades, which can be in an extrapolative or contrarian manner. The opposite is true if $\kappa_0 > 1$, this means that the team exacerbates the usage of past returns as information for future trades.

We also model the transmission of heuristic rules that is conditional on the team composition, namely depending on whether the average team extrapolates or performs contrarian trades:

$$\beta_j^{TM} = \alpha + \kappa_0 \bar{\beta}_j^{SM} + \kappa_1 D_j \times \bar{\beta}_j^{SM} + \kappa_2 D_j + \upsilon_j, \tag{15}$$

such that D_j is an indicator variable equal to one when $\bar{\beta}_j^{SM} > 0$. This means that the transmission coefficient will be different for teams that are on average contrarian (κ_0) and those that are on average extrapolative ($\kappa_0 + \kappa_1$).

C.2 OLS estimation

Using the simulated solo manager and team trades, we can estimate the respective betas from the simulated data, $\bar{\hat{\beta}}_j^{SM}$ and $\hat{\beta}_j^{TM}$. For the OLS regressions, we regress the estimated

team betas on the average estimated extrapolation betas:

$$\hat{\beta}_j^{TM} = c + \delta_0 \bar{\hat{\beta}}_j^{SM} + \epsilon_j. \tag{16}$$

To test for the transmission of heuristic rules depending on team composition, we instead estimate:

$$\hat{\beta}_j^{TM} = c + \delta_0 \bar{\hat{\beta}}_j^{SM} + \delta_1 \hat{D}_j \times \bar{\hat{\beta}}_j^{SM} + \delta_2 \hat{D}_j + \epsilon_j. \tag{17}$$

It is important to note that when estimating the regression above, we condition $\hat{D}_j$ on $\bar{\hat{\beta}}_j^{SM}$, because in our empirical setting we are not aware of the true nature of the team, but instead infer the team nature from estimates based on the data (i.e. we condition on the empirically estimated average single beta to determine the nature of the team).

C.3 IV estimation

We now move to the simulations for our instrumental variables approach following Jegadeesh et al. (2019) to solve the error-in-variables problem we face. For the IV method, we estimate extrapolation betas for each manager by splitting the sample used to estimate Equation (11), obtaining two separate estimates of β_i^{SM} for each manager, $\hat{\beta}_i^{SM,1}$ and $\hat{\beta}_i^{SM,2}$. We split the sample such that for each manager and quarter we randomly split the holdings into two equally sized samples of stocks, such that $\hat{\beta}_i^{SM,1}$ and $\hat{\beta}_i^{SM,2}$ are estimated using two sets of 50 stocks throughout the 40 quarters. For these two disjoint data samples, we calculate two sets of average team betas, $\bar{\hat{\beta}}_j^{SM,1}$ and $\bar{\hat{\beta}}_j^{SM,2}$. Because we estimate the extrapolation betas on disjoint samples, their measurement errors are uncorrelated. As a result, we can use the extrapolation beta of sample one (two) as instrument for sample two (one). Formally, the first and second stage of the IV method are as follows:

$$\begin{aligned} &\text{1st stage: } \bar{\hat{\beta}}_j^{SM,1} = \alpha + \lambda_0 \bar{\hat{\beta}}_j^{SM,2} + u_j, \\ &\text{2nd stage: } \hat{\beta}_j^{TM} = c + \delta_0 \bar{\hat{\beta}}_j^{SM,1,pred} + \epsilon_j, \end{aligned} \tag{18}$$

where *pred* indicates the predicted values from the first stage regressions.

Because the measurement errors of $\bar{\hat{\beta}}_{1,j}^{SM}$ and $\bar{\hat{\beta}}_{2,j}^{SM}$ are uncorrelated, so will $\bar{\hat{\beta}}_{2,j}^{SM}$ and ϵ_j, meaning that δ_0 from Equation (18) will be an unbiased estimator of κ_0 from our true data generating process. To estimate the IV method conditional on the team composition, we follow a similar procedure where we also instrument for the indicator variable and interaction term using the estimates from the disjoint sample. Because these two additional variables are also estimated from the data, they suffer from measurement error too. Formally, we estimate:

$$\text{1st stage: } \begin{cases} \bar{\hat{\beta}}_j^{SM,1} & = \alpha_1 + \lambda_{1,0}\bar{\hat{\beta}}_j^{SM,2} + \lambda_{1,1}\hat{D}_j^2 \times \bar{\hat{\beta}}_j^{SM,2} + \lambda_{1,2}\hat{D}_j^2 + u_{1,j}, \\ \hat{D}_j^1 \times \bar{\hat{\beta}}_j^{SM,1} & = \alpha_2 + \lambda_{2,0}\bar{\hat{\beta}}_j^{SM,2} + \lambda_{2,1}\hat{D}_j^2 \times \bar{\hat{\beta}}_j^{SM,2} + \lambda_{2,2}\hat{D}_j^2 + u_{2,j}, \\ \hat{D}_j^1 & = \alpha_3 + \lambda_{3,0}\bar{\hat{\beta}}_j^{SM,2} + \lambda_{3,1}\hat{D}_j^2 \times \bar{\hat{\beta}}_j^{SM,2} + \lambda_{3,2}\hat{D}_j^2 + u_{3,j}. \end{cases} \tag{19}$$

$$\text{2nd stage: } \hat{\beta}_j^{TM} = c + \delta_0\bar{\hat{\beta}}_j^{SM,1,pred} + \delta_1\hat{D}_j^{1,pred} \times \bar{\hat{\beta}}_j^{SM,1,pred} + \delta_2\hat{D}_j^{1,pred} + \epsilon_j.$$

C.4 OLS and IV results simulations

To show that the OLS estimator is biased whereas the IV estimator is not, we run 2,500 simulations and compare the distribution of the coefficient estimates for both methodologies. The simulations are run such that the true parameters reflect our null hypothesis: $\kappa_0 = 1$ and $\kappa_1 = 0$. The results are depicted in Panel A of Table C1 where we observe that the OLS estimate gives an average coefficient of 0.95, with a standard deviation equal to 0.037, and we reject $\delta_0 = 1$ in 35% of the cases at a 95% significance level. On the other hand, the IV estimator leads to an unbiased estimate of δ_0 and we find an average coefficient equal to 1.00 with a standard deviation equal to 0.04. For the IV estimator, we are only able to reject the null of no transmission coefficient in 5% of the cases. When estimating the model with interaction terms, we also find similar results in Panel B of Table C1, where the IV estimator provides us with an unbiased estimate for δ_0.

One additional concern is that the measurement error could bias the coefficient of the interaction term as well, however given that we test the null of $\kappa_1 = 0$ in this case, the bias would work against us because of the well-known fact that biases resulting from (uncorrelated) measurement error tend to shrink estimates towards zero.[16] When simulating the transmission coefficient under the null $\kappa_1 = 0$, Panel B of Table C1 shows that we do not falsely reject the null using both the OLS and IV estimators.

[Place Appendix Table C1 about here]

C.5 Multiple IV draws from our dataset

The IV methodology used in our analysis relies on a random draw from a subset of manager holdings. To ensure that the IV results from our main analysis are not driven by one particular draw, we randomly draw 2,500 disjoint samples of the holdings from each manager in a given quarter using the same methodology of the main analysis (Section 4). In particular, for each of these draws, we run the same IV regression as the one specified in Column 6 of Table 5.

In Figure C1, we show the distribution of the coefficient on $\hat{\beta}_j^{CF}$ for the 2,500 draws where we exclude draws that produce weak instruments.[17] Recall that our null hypothesis of full transmission of heuristics from individuals to the team implies that this coefficient equals 1. The figure shows that the distribution lies significantly below 1, with a mean coefficient equal to 0.645 and a standard deviation of 0.102. These results support our conclusion that teams are indeed dampening the transmission of individual trading behavior to their respective teams.

[16] In unreported simulations, we confirm that the measurement error works against us when simulating data with $\kappa_1 = -1$.

[17] According to Nelson and Startz (1990), to ensure that instruments are not weak, the correlation between the instrument and instrumented variable, $\rho_{xz} >> \frac{1}{\sqrt{N}}$. For an instrument to be included, we require that ρ_{xz} is at least 6 times greater than $\frac{1}{\sqrt{N}}$. This rule also yields a very similar result to requiring a t-statistic of 4.05 in the first stage regression as suggested in Stock and Yogo (2005) as a rule to screen out weak instruments.

[Place Appendix Figure C1-C2 about here]

We also perform the same analysis as before on the sub-samples of teams for which the team members are on average contrarians and for those which are on average extrapolators. In other words, we run the same IV regression as the one specified in Column 8 of Table 5 for multiple draws. In Figure C2, we find that the distribution of the transmission coefficient is closer to zero for extrapolative teams as opposed to contrarian teams, consistent with our main analysis. The average transmission coefficient for contrarian teams equals 0.70, whereas only 0.17 for extrapolative teams.

Table C1. **The transmission of extrapolation bias to teams: simulation results:** In this table we present the OLS and IV results from 1,000 independent simulations. Panel A (B) shows the results without (with) the interaction terms. In the first column we report the true parameters, whereas in the second and third we present the estimated parameters using OLS and IV procedures respectively. We report average estimates, the standard deviations and average t-statistics over the 1,000 simulations. For the δ_0 parameter, we also provide the percentage of times we reject the null, H_0: $\delta_0 = 1$ at a 95% significance level.

Panel A: Main

	True Coefficient	**OLS**	**IV**
Avg. (c)	$\alpha = 0$	-0.003	0.000
Std. (c)		0.004	0.005
Avg. (T_c)		-0.625	-0.035
Avg. (δ_0)	$\kappa_0 = 1$	0.951	1.000
Std. (δ_0)		0.037	0.040
Avg. (T_{δ_0})		28.903	26.315
% Rejected H_0: $\delta_0 = 1$		0.349	0.054

Panel B: Interaction Terms

	True Coefficient	**OLS**	**IV**
Avg. (c)	$\alpha = 0$	-0.002	0.000
Std. (c)		0.008	0.017
Avg. (T_c)		-0.273	0.022
Avg. (δ_0)	$\kappa_0 = 1$	0.953	1.002
Std. (δ_0)		0.061	0.107
Avg. (T_{δ_0})		16.134	9.903
% Rejected H_0: $\delta_0 = 1$		0.121	0.048
Avg. (δ_1)	$\kappa_1 = 0$	-0.003	0.000
Std. (δ_1)		0.127	0.211
Avg. (T_{δ_1})		-0.025	-0.015
Avg. (δ_2)	$\kappa_2 = 0$	0.000	-0.001
Std. (δ_2)		0.013	0.041
Avg. (T_{δ_2})		-0.034	-0.030

Figure C1. **Empirical simulations transmission coefficient**: This figure shows the distribution of transmission coefficients (δ_0) that we obtain for different random samples using our IV methodology based on Equation (5), excluding the interaction term. The mean of the transmission coefficient equals 0.645 with a standard error of 0.102.

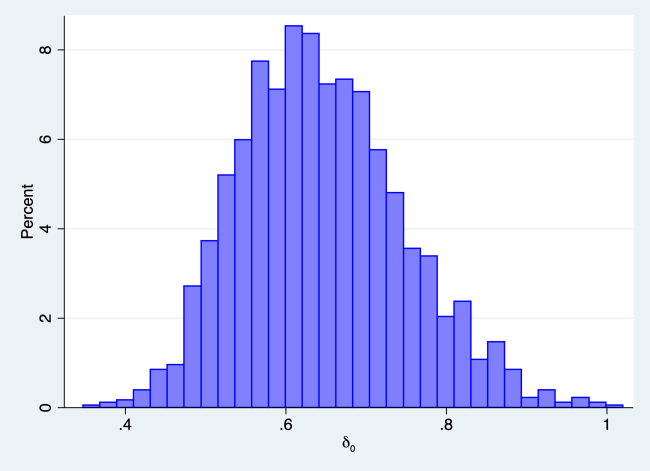
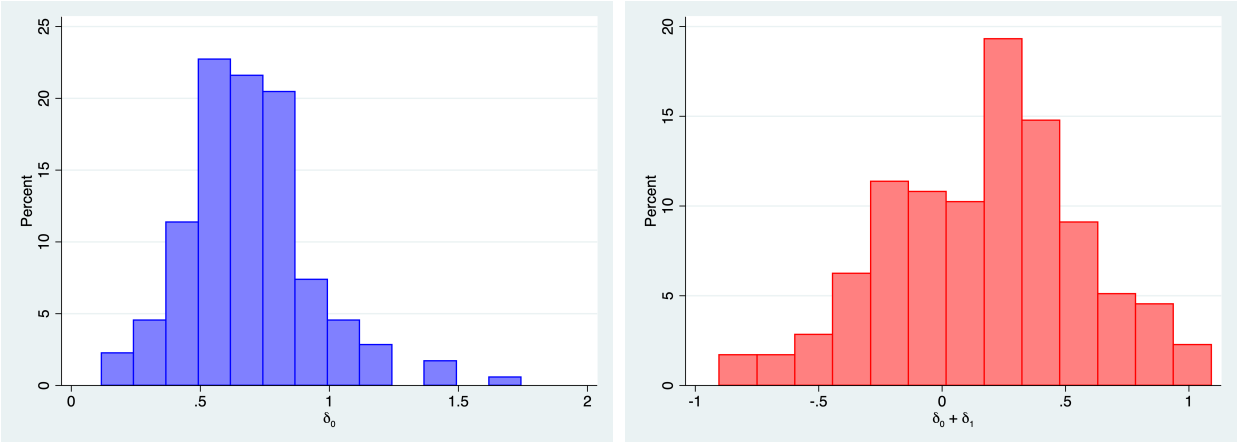


Figure C2. **Empirical simulations transmission coefficients - Contrarians versus Extrapolators**: This figure shows the distribution of transmission coefficients for contrarians (δ_0) and extrapolators ($\delta_0 + \delta_1$) that we obtain for different random samples using our IV methodology based on Equation (5). The average transmission coefficient for contrarians equals 0.70 with an standard error of 0.10 and for extrapolators the average transmission coefficient equals 0.17 with a standard error of 0.40.



(a) Contrarians (b) Extrapolators