Do Teams Alleviate or Exacerbate Biased Beliefs? Evidence from Extrapolation Bias in Mutual Funds*

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Abstract

Whether teams attenuate or exacerbate biases in beliefs which are common at the individual level is an open question. To address this question, we study the investment decisions of professional money managers in the US. We focus on the extrapolation bias, a bias in expectations formation which has been linked to the representativeness heuristic and is pervasive in financial markets. Using a within-subject design, we show that the individual-level extrapolation bias is attenuated by 78% in teams. Additional analysis reveals that this attenuation is not due to learning or differences in the managerial compensation, workload, or investment objectives 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 the extrapolation bias.

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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 depends on the specific nature of the task at hand,[1] thus leading to the question of how teams fare when dealing with complex real-world problems.

Studies have pointed out that team members' ability to identify each other's judgment mistakes can be a key determinant of team's decisions and performance (e.g. Sah and Stiglitz, 1986). However, much work in cognitive psychology shows that in key areas of decision-making individuals share common intuitive rules of thumb, called heuristics, which lead to systematic and widespread errors in expectations (e.g. Tversky and Kahneman, 1974; Kahneman, 2003). The shared use of common heuristics can generate similar biases in beliefs 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 biases in expectations 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 of professional money managers in the US. The investment decisions of money managers represent a natural setting to research how individual biases in expectations formation affect teams' decisions for two main reasons. First, the choice of risky investments that mutual fund managers and their teams confront is a classical example of judgment under uncertainty, an area of decision-making in which heuristics and judgment biases are known to be pervasive at the individual level.[2] Second, 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

[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.

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

make investment decisions individually as well as members of a team. By performing a within-subject comparison of judgment biases between team-managed funds and solo-managed funds, we can recover the impact of teams as an organizational structure on biases in expectations formation, while avoiding that unobservable differences between the managers in solo-managed funds and those in team-managed funds contaminate our analysis.

When studying whether teams attenuate or exacerbate biases in beliefs we concentrate on the extrapolation bias, a bias in belief formation which prior studies have linked to the representativeness heuristic (e.g. Barberis, 2018). Greenwood and Shleifer (2014) use direct measures of investor beliefs from surveys and show evidence of a widespread extrapolation bias, whereby individuals predict higher future stock returns following high past returns, despite the weak empirical evidence of autocorrelation in stock returns. Additionally, theory work shows that incorporating the extrapolation bias into a model of financial markets helps to explain many puzzles about asset prices, ranging from the predictability of returns to the excess volatility of asset prices relative to asset fundamentals (Barberis et al., 2015). Given the pervasiveness of the extrapolation bias at the individual level and its central role in prominent behavioral theories of financial markets, it is key to ask whether teams as an organizational structure help reduce the impact of this bias on financial decisions.[3]

To measure how extrapolation affects a fund manager's decisions, we follow the literature on extrapolation (e.g. Barberis, 2018) and define the extrapolation bias as the sensitivity of a manager's trades to past stock returns. Based on the recent evidence that extrapolative beliefs are more sensitive to recent as opposed to distant returns (e.g. Greenwood and Shleifer, 2014; Da et al., 2021), we measure fund managers' extrapolation bias as the sensitivity of these managers' dollar trades to a weighted sum of past quarterly stock returns, where the higher weight assigned to more recent quarters follows the direct estimates of the structural parameters of extrapolation in Greenwood and Shleifer (2014).[4] Our approach to measuring extrapolation renders a metric that is not only conceptually, but also empirically distinct from the other two return-based determinants of investor trading behavior that the literature has documented, namely momentum trading (Jegadeesh and Titman, 1993) and the disposition effect (Odean, 1998). As we show, these alternative determinants collectively account for

[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 the extrapolation bias includes Greenwood and Shleifer (2014), Barberis et al. (2015), Barberis et al. (2018), Cassella and Gulen (2018), Jin and Sui (2022), Da et al. (2021). Other explanations for the extrapolation bias include a belief in the law of small numbers (Rabin, 2002), and bounded rationality (Hong and Stein, 1999; Fuster et al., 2012; Glaeser and Nathanson, 2017).

[4]For robustness, we repeat the analysis with a simpler metric for past returns that does not rely on the survey evidence, and by using different types of controls in the regressions to estimate managers' extrapolation. The results of our analysis remain the same.

only about 4% of the variation in extrapolation across fund managers.[5]

Using our extrapolation metric, we show that fund managers who extrapolate past returns achieve investment outcomes that are systematically suboptimal from an asset manager's standpoint. Specifically, extrapolative fund managers achieve worse future raw investment returns, worse returns in excess of the funds' primary benchmark, and lower risk-adjusted returns than fund managers who do not extrapolate. Moreover, extrapolative managers are less likely to become top performers, and they receive less capital inflows from their investors, and hence their funds grow less. Given the evidence that fund managers' compensation and career are strongly influenced by fund performance, ranking, and size (e.g. Berk et al., 2017; Ma et al., 2019), these results indicate that fund managers who extrapolate past returns are systematically worse off compared to fund managers who do not extrapolate. Further tests show that risk aversion, hedging motives (e.g. Campbell and Cochrane, 1999; Breeden, 2005), or preference for skewness (e.g. Barberis and Huang, 2008) are unlikely to explain why fund managers who extrapolate achieve worse investment outcomes on average. Overall, this evidence highlights the consistency between our extrapolation metric and the leading interpretation of extrapolation in the literature, namely, that it represents a bias in belief formation that leads to investment mistakes.

In the second and central step of the analysis, we compare the extent to which extrapolation affects investments in solo-managed funds versus team-managed funds. Prior work relies on a between-subject design. In this design, the role of teams is measured as the difference between the observed behavior of team-managed funds and those of solo-managed funds. The lack of random assignment in this setting can pose a major challenge in the identification of the role of teams for decision-making (e.g. Chapter 2, Angrist and Pischke, 2008). If for instance the managers who operate a fund individually exhibit a stronger (weaker) tendency to extrapolate past returns than the managers who work in teams, the between-subject approach would lead to the conclusion that teams attenuate (exacerbate) the extrapolation bias even in the absence of a causal role of teams. 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 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 regressions of team-funds' extrapolation bias on team members'

[5] The results of the regressions are in Table A2. Moreover, we also explicitly control for momentum trading and the disposition effect in all of our tests. Including these controls has no effect on our conclusions.

average individual-level extrapolation bias, teams attenuate the extrapolative behavior that their members exhibit at the individual level by 78% on average. This result (i) is robust to various ways of estimating managers' and teams' extrapolative behavior; (ii) survives controlling for other biases in trading behavior; and (iii) is not due to measurement error as illustrated by an IV procedure in the spirit of Jegadeesh et al. (2019).

Because of our within-subject design, time-invariant observable and unobservable managerial characteristics do not pose a challenge to the identification of the role of teams in decision-making. We then address other identification concerns. First, while we hold fund management constant when comparing solo-managed and team-managed funds, funds are not randomly assigned to teams and individual managers. Thus, key differences between the funds that are managed by teams and the funds that are managed by individuals could reconcile our result. We first note that the team-managed and solo-managed funds in our sample are very similar along a host of readily observable fund characteristics such as fund size, age, and expense ratios. These similarities already suggest that heterogeneity in the team-managed and solo-managed funds cannot explain our result. We then conduct a more in-depth analysis concerning a key fund characteristic, namely the fund's managerial compensation structure, that could account for our result. Specifically, fund managers might be incentivized to exert more effort when making investment decisions in a team if compensation is more sensitive to performance in team-managed as opposed to solo-managed funds. In turn, work in psychology suggests that more effortful investment choices could reduce the use of heuristics and extrapolation that we observe in teams (e.g. Kahneman, 2003; Frederick, 2005). Such a bias reduction would be consistent with our evidence, but it would not be due to teams as an organizational structure per se. 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, by and large, the compensation structure does not differ when managers operate individually or in a team. Overall, fund heterogeneity in general, and heterogeneity in managerial compensation in particular, cannot reconcile our main finding.[6]

Second, individual behavior and team behavior are sometimes measured at separate points in time in our sample. Thus, time-varying managerial characteristics or time-varying fund policies could explain our results. In particular, to the extent that individual management occurs before team-based management, the most natural explanation for our finding is a learning story (De Groot and de Groot, 1978; Levitin, 2002). Simply put, managers

[6] We also conduct further analysis on whether systematic differences in investment mandates between solo-managed funds and team-managed funds, in particular with respect to fund style and fund families, could reconcile our results. We find that such style migrations and fund family transitions do not have a mediating role for the reduction of extrapolation bias in teams.

accrue expertise during their solo-management years and such expertise could lead to a lower extrapolation bias when these managers later go on to operate in a team. Similarly, fund families can learn about fund managers' biases while observing these managers operate individually. These managers may later be subject to policies or oversight that would curb such biases precisely at the time we observe these managers operate in a team. The observed reduction of extrapolation bias would therefore not be due to teams per se, but rather due to learning by managers and institutions. One straightforward way to assess this explanation is to note that a learning story predicts the attenuation of extrapolation bias in teams only in those instances in which managers first operate individually, and hence they and their fund families have the opportunity to learn, and only later join a team. On the contrary, the learning story does not predict a reduction of extrapolation bias in a team when team management precedes individual management. Importantly, we find the attenuation of extrapolation bias both when team-based management follows individual management and when the former leads the latter. We therefore conclude that learning about investor biases cannot reconcile our result.

Third, an additional identification challenge stems from the changes in the work environment that a manager is exposed to over time. Whereas there can be many such changes, the type that can confound our result is one that correlates with individuals' propensity to rely on heuristics and mental shortcuts. With respect to this issue, Stanovich and West (2008) propose that an individual's reliance on heuristic rules typically increases when his workload rises. So, our results of lower extrapolation in teams may not be due to team per se, but rather due to a reduction in a manager's workload that occurs at the same time in which managers operate as part of a team. To investigate this explanation, we use the number of stocks that a manager oversees across all the portfolios he manages or co-manages over time as a proxy for the manager's workload. We find that the attenuation of extrapolation bias does not depend on a reduction in workload when fund managers operate in a team. We therefore conclude that changes in the work environment are unlikely to explain our results.

In the last part of the paper, we ask what mechanism can generate the documented reduction in expectations biases when managers operate in teams. Like the literature, we conjecture that errors in expectation formation such as the extrapolation bias can be due to heuristics and cognitive mistakes. A long tradition in psychology and economics proposes that such mistakes can be conceptualized by means of a dual-system framework (e.g. Sloman, 1996; Kahneman, 2003; Ilut and Valchev, 2020). In such a framework, two concurrent forms of cognition exist, namely, intuition (henceforth System I) and deliberation (henceforth System II). System I is responsible for incorrect probabilistic assessments and expectations errors. Engaging System II can help mitigate the errors of System I by means of effortful cognitive reflection (e.g. Frederick, 2005). System II is more likely to engage in deeper cognitive reflection when it receives cues indicating an impending cognitive mistake (e.g. Kahneman, 2000; Stanovich and West, 2008; Sloman, 2014). Therefore, we propose that teams may be able to reduce the influence of extrapolation on decision-making by creating cues that elicit cognitive reflection.

We argue that teams can provide two types of cues that lead to cognitive reflection. 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 situational cue "internal reflection". 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 "external screening". 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 be a key factor for external screening, because those team members that do not extrapolate may find it easier to point to the mistakes of extrapolators. On the contrary, the attenuation of extrapolation bias that is due to internal reflection depends less on team members' heterogeneity in extrapolation, because internal reflection stems primarily 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 contributes to the literature in economics that studies the impact of teams on decision-making (e.g. Holmstrom, 1982; Sah and Stiglitz, 1986; Meyer, 1994; Gershkov and Winter, 2015; Friebel et al., 2017; Lyons, 2017). 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 a team or individuals' participation to groups helps improve rational self-interested choice in strategic interactions.[7] 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).[8] A few papers investigate the role of teams outside of the laboratory. Like our work, some of these papers too use the mutual fund industry as an empirical setting.[9] In this literature, recent work by Harvey

[7] See for instance Bornstein and Yaniv (1998), Kocher and Sutter (2005), Cooper and Kagel (2005), Charness et al. (2007b), Sutter (2009).

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

[9] Examples of earlier work on teams based on field data include Prather and Middleton (2002), Bär et al. (2011).

et al. (2021) and Evans et al. (2021) investigates the role of teams (the former) and team diversity (the latter) for investment creativity and performance. We differ from these two papers, because our focus is on whether teams as an organizational structure help reduce the impact of heuristics and biases on decision-making. Cici (2012) and Fedyk et al. (2020) study the incidence of the disposition effect and overconfidence in investment teams. This work differs fundamentally from ours due to the different origin that is often attributed to the disposition effect (prospect theory preferences, e.g. Shefrin and Statman, 1985) or overconfidence (motivated reasoning and ego utility, e.g. Bénabou and Tirole, 2002) vis-a-vis extrapolation bias (biased expectations due to cognitive mistakes).

Our paper also offers a methodological improvement over existing literature that studies investment teams with field data. In this literature, the impact of teams is measured by means of a between-subjects design where all teams are compared to all individuals. The lack of subjects' random assignment in this setting can pose a major challenge to the identification of the role of teams for decision-making. This is especially true in an environment such as the mutual fund industry where theory suggests that team and solo managers may differ from each other along important dimensions (e.g. Huang et al., 2019). We circumvent the lack of random assignment that permeates field data by relying on a within-subject design in which the same agents are observed both when undertaking individual decisions and when making decisions as part of a group. Our approach, joint with the large set of robustness checks we conduct to assess potential confounding effects, speaks more directly to the causal role that teams as a managerial structure play in decision-making.[10]

Our study also 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 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 (e.g. Edelen et al., 2016; Akepanidtaworn et al., 2022). Our contribution to this line of work is twofold: (i) we quantify the extent to which fund managers' trading behavior conforms to a key source of biased beliefs, namely, the extrapolation bias (e.g. Greenwood and Shleifer, 2014; Barberis et al., 2015; Barberis, 2018); (ii) we address the deeper question of whether the adoption of

[10]Single-to-team switches are also considered in recent work, e.g. Fedyk et al. (2020) and Harvey et al. (2021), as a strategy to address the lack of managers' random assignment to teams and individual funds. This strategy is different from the within-subject analysis we implement, because a managerial switch introduces new managers into the fund, altering at the same time a fund managerial structure and the human capital (skill, expertise) employed in the management of the fund. Evans et al. (2021) also use a within-subject design for a part of their analysis, but they apply it to measure the impact of team diversity, rather than teams per se, on decision-making.

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.

Finally, our study relates to the growing empirical literature that examines the role of diversity for team decision-making and performance. Most of the literature uses team members' heterogeneity with respect to observable demographic or socioeconomic characteristics such as gender (e.g. Adams and Ferreira, 2009; Gompers and Wang, 2017; Dahl et al., 2021), ethnicity (e.g. Alesina and Ferrara, 2005; Hjort, 2014), or ideology and political affiliation (e.g. Costa and Kahn, 2003; Evans et al., 2021) as a gauge into the underlying heterogeneity in team members' information, preferences, or beliefs. Our approach to measuring heterogeneity in beliefs within a team is different, because we gain a direct insight into team members' heterogeneity in the extrapolation bias by analyzing these members' trading behavior when they make investment decisions individually. We then ask how heterogeneity in the individual-level extrapolation bias affects the ability of teams to cope with the extrapolative behavior of their members. Our results indicate that such a heterogeneity is not a necessary condition for teams to successfully reduce the impact of extrapolation bias on decision-making.

2. Data and Empirical Methodology

2.1. Construction of the Main Dataset

Our analysis focuses on US active domestic equity mutual funds from 1980Q1 to 2018Q4. 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, fund styles, and fund's primary prospectus benchmark), CRSP's mutual fund data (fund holdings and fund characteristics), and Thomson Reuters (fund holdings).

In our first step of the data preparation procedure, 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 the Appendix A1, 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 80% of our initial universe of CRSP US active domestic equity mutual funds.

In the second step of the data preparation, 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 reason is that the CRSP mutual fund holdings data only starts in 2003 and its coverage is smaller than Thomson Reuters until 2008. However, after 2008, CRSP has better coverage than Thomson Reuters (e.g. 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 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 $0.98 billion. Of all the mutual funds, 65% are managed by teams rather than individual managers. Figure A1 of the Appendix shows the steady 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 experience has a median equal to 28 quarters, or 7 years. Each fund holds a median of 55 stocks.

2.2. Measuring Extrapolation

To directly measure extrapolative belief formation of mutual fund managers, we would need extensive data on mutual fund managers' stock-level expectations. This data, to the best of our knowledge, is 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 investors' extrapolative beliefs affect trading decisions, in that fund managers who extrapolate past returns buy (sell) stocks when these stocks have done well (poorly) in the recent past.[11]

Therefore, to obtain a measure of extrapolation for the managers of fund j, we estimate a panel-level regression of the fund's trades on past stock returns, where observations are either pooled over the entire history of a fund or over a moving window. More formally, we estimate the following regression:

$$trade_{s,j,t+1} = \alpha_j + \beta_j^X r_{s,t-4\to t} + \gamma_j' C_{s,t} + \eta_j' F_{s,j,t} + \theta_{j,t} + e_{s,j,t+1} \quad \text{for} \quad j = 1, \ldots, J. \tag{1}$$

In words, for each fund j we regress the change in the fund's position in stock s between the end of quarter t and the end of quarter $t+1$, $trade_{s,j,t+1}$, on that stock's past return at time t, $r_{s,t-4\to t}$, plus time fixed effects $\theta_{j,t}$, additional variables that account for flow-induced trading $F_{s,j,t}$, and a host of standard stock characteristics $C_{s,t}$. We briefly describe the variables in the regression below, and provide further details in Appendix A2.1.[12]

We compute $trade_{s,j,t+1}$ as the split-adjusted change in the holdings of stock s held by fund j at time $t+1$, where the trade value is in dollars based on the price of the share at time $t+1$, $P_{s,t+1}$, and normalized by the fund TNA at that time (as in, e.g., Fang et al., 2014; Gantchev et al., 2021):

$$trade_{s,j,t+1} = \frac{(shares_{s,j,t+1} - shares_{s,j,t}^{split-adj})P_{s,t+1}}{TNA_{j,t+1}}. \tag{2}$$

The main parameter of interest in Equation (1) is β_j^X, which measures fund managers' tendency to extrapolate as the sensitivity of managers' trading behavior to past stock returns. We refer to β_j^X as the *extrapolation beta* of fund j.

To better micro-found our measure of extrapolation, we measure fund managers' tendency to extrapolate past stock returns based on insights from Greenwood and Shleifer (2014). They use direct data on beliefs from surveys to show that extrapolators have declining memory, with more recent returns playing a much bigger role in shaping extrapolative beliefs than distant ones. Thus, following Greenwood and Shleifer (2014) we measure past returns as a weighted sum of past quarterly stock returns (r_t) with exponentially declining weights:

$$r_{s,t-4\to t} = \sum_{j=0}^{k} w_j r_{t-j} \tag{3}$$

[11] In the remainder of this section, for ease of exposition we refer to funds rather than managers. In practice, we apply the same method to measure extrapolation in a fund led by a team of managers, or by individual managers.

[12] Later for robustness we repeat the analysis with alternative specifications involving either a different definition of the left-hand side variable or changes in the right-hand side variables of Equation (1). Results remain the same.

where $w_j = \frac{\lambda^j}{\sum_{i=0}^{k} \lambda^i}$ and λ equals extrapolators' memory parameter. Following the estimates of the parameter λ from Greenwood and Shleifer (2014), Table 4, we set $\lambda = 0.56$, i.e., the average parameter they estimate across six distinct surveys of investor expectations. This memory parameter implies a steep decline in weights in Equation (3), whereby the most recent four quarterly stock returns explain 90% of the variation in extrapolators' beliefs about future asset returns.[13] We therefore set $k = 3$, i.e., we use four quarters of past stock returns.

In the remainder of the paper we refer to funds with an extrapolation beta above zero ($\beta_j^X > 0$) as *extrapolators*, while we borrow from Conrad and Kaul (1998), Shefrin (2008), and Barberis and Shleifer (2003) and refer to those managers for which $\beta_j^X \leq 0$ (i.e., those managers who bet on a price correction) as *contrarians*.

We estimate fund managers' extrapolative behavior after controlling for a set of stock-level controls, $C_{s,t}$, which includes size, book-to-market, asset growth, profitability, past 12-month return volatility, and past one-month return as a proxy for short term reversal. Moreover, to ensure that we capture managers' extrapolative behavior that is not linked to the beliefs or preferences of their clients, we control for flow-induced trading. Importantly, Lou (2012) shows that in the presence of cross-sectional differences in liquidity costs, fund flows lead to disproportionately buying (selling) certain stocks over others. Therefore, $F_{s,j,t}$ includes two measures of liquidity costs (i) the percentage of all shares outstanding of stock s that is held by fund j at the end of quarter t ($pctown_{s,j,t}$) and (ii) the effective bid-ask spread of stock s ($bidask_{s,t}$). As in Lou (2012), we also include the interaction of both liquidity costs measures with contemporaneous fund flows ($flow_{j,t+1}$).[14] Finally, we include time fixed effects in our regressions to control for trading behavior that is transitory and could be spurred by temporary market conditions.

Thus, formally we have:

$$F_{s,j,t} = \begin{bmatrix} pctown_{s,j,t} \\ bidask_{s,t} \\ pctown_{s,j,t} \times flow_{j,t+1} \\ bidask_{s,t} \times flow_{j,t+1} \end{bmatrix}. \tag{4}$$

[13] Formally, Greenwood and Shleifer (2014) set $k = 59$ (i.e., a total of 60 quarters or 15 years). Thus, based on the value of $\lambda = 0.56$ estimated on average across surveys, the relative weight of the first four quarters in the 15-year sum is $\sum_{j=0}^{3} \frac{\lambda^j}{\sum_{l=0}^{59} \lambda^l} = \frac{1-\lambda^4}{1-\lambda^{60}} = 0.9016$.

[14] Later in Table A4 we show that a more complex specification that allows for asymmetric flow-induced trading as in Lou (2012) generates extrapolation estimates that are virtually identical to the ones used in our main analysis. Unlike Lou (2012), we do not include fund flows and fund-level liquidity costs as separate regressors, because in our fund-level regressions of Equation (1), both flows and fund liquidity are subsumed by the time fixed effects.

In each quarter, the universe of stocks that appear in the cross-section of trades in Equation (1) is based on a definition of the investment universe of fund j that is close to Koijen and Yogo (2019). They propose that a fund's investment universe is made of stocks that the fund has held at any point in time in the previous 11 quarters plus the current quarter. We follow their approach, but also include in the tests those stocks that the fund will start owning at some point in the subsequent 11 quarters. This definition of the investment universe accounts for the fact that a stock can enter the investment universe before fund managers' first purchase of that stock, and the decision not to yet purchase that stock at time t also contains information that is useful to measure fund managers' extrapolation. In the Appendix, Table A4, we show that our results are robust to other definitions of the investment universe, as well as to a specification where we abstract from defining the investment universe and only include actual managers' trades.

Table 1 presents summary statistics about β_j^X. Panel A shows the cross-sectional properties of our extrapolation metric. There is a substantial heterogeneity across funds, with 46% of the sample characterized by an extrapolation beta larger than zero, and the remaining 54% showing contrarian behavior. Panel B shows pairwise correlations between a fund's extrapolative behavior and variables summarizing other aspects of the fund's trading behavior. The most noteworthy correlations concern the relation between β_j^X and other aspects of a fund's trading behavior that are related to past stock returns, such as momentum trading (Jegadeesh and Titman, 1993) and the disposition effect (Odean, 1998).[15] The correlations between extrapolative trading and momentum trading is fairly low (19%). This low correlation is to be expected. Unlike momentum, that makes prescriptions about how to trade stocks that place in the top and the bottom of the cross-sectional return distribution, our measure of extrapolation concerns a fund's trading behavior over the entire investment universe of the fund. Moreover, whereas momentum trading is based on past-year cumulative returns, in our regressions we measure extrapolation in the spirit of Greenwood and Shleifer (2014) as fund managers' response to a weighted sum of past stock returns with declining weights. Extrapolation is also empirically different from the disposition effect, because β_j^X holds a small negative correlation of -18% with a manager's disposition effect.[16]

Table 1 also offers summary statistics for three alternative extrapolation metrics which we estimate for robustness. These metrics are: (i) a no-momentum variant of our extrapolation

[15] Momentum trading is measured as the loading of a fund's monthly return on the momentum factor in a Carhart (1997) four-factor time-series regression. The disposition effect is measured following Odean (1998). Details are in the Appendix A2.2.

[16] Note that these low correlations between the extrapolation metrics and other aspects of the fund's trading behavior are not driven by outliers, because we winsorize all our variables that are described in Table 1 at the 0.5th and 99.5th percentile, based on the full sample.

measure, whereby we estimate Equation (1) after excluding winners and losers in the cross-section; (ii) a variant of the extrapolation metric whereby we examine extrapolation by means of the relation between past returns and future fund weight changes as opposed to dollar trades; and (iii) a variant of the extrapolation metric that shuts down the memory decay in Equation (3) and uses realized stock returns over the past year. Importantly, Panel B shows that these alternative extrapolation metrics also have a low correlation with momentum trading and the disposition effect, as well as with other aspects of factor-trading behavior. Moreover, there is a large pairwise correlation amongst the four extrapolation metrics. Therefore, in the remainder of the paper we use the specification outlined above as our baseline extrapolation beta, β_j^X, and use the alternative metrics later for robustness.[17]

[Place Table 1 about here]

3. Extrapolation and Investment Performance

Previous work on the extrapolation bias (e.g. Barberis, 2018) suggests that, insofar as return extrapolation reflects an investor bias, it should lead fund managers who extrapolate past returns to achieve worse outcomes compared to managers who do not extrapolate.

To test whether extrapolation makes mutual fund managers indeed worse off, we rely on insights from Ma et al. (2019). They use regulation introduced by the SEC in 2005 that requires funds to disclose details on their management and their 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. Many funds also disclose that their managers' compensation can grow substantially when their fund beats the fund's benchmark or peer funds. Finally, Berk et al. (2017) show that fund managers' performance is not only important for managers' compensation, but also for their career prospects.

Given that fund managers receive a large portion of their compensation based on their fund's investment performance relative to the fund benchmark and to the fund peers, we argue that if return extrapolation among fund managers is a bias, it should lead to lower fund returns relative to the fund's benchmark and compared to same-style funds. Figure 1 presents introductory evidence in support of our conjecture. In Panel A we estimate funds' extrapolative behavior over the full sample, and then sort funds into either two groups (extrapolators and contrarians, left panel), or five groups based on quintile breakpoints

[17] More details on the alternative metrics for extrapolation can be found in Section 5.

(right panel). The figure reports style-adjusted average yearly gross fund performance in each of the aforementioned groups, as well as the difference between the top and the bottom group. Performance is measured in a variety of ways: (i) raw returns; (ii) fund returns in excess of the benchmark (henceforth, benchmark-adjusted returns); (iii) CAPM alpha; (iv) Fama-French 3-factor model (FF3, Fama and French, 1993); and (iv) Fama-French 5-factor model (FF5, Fama and French, 2015).[18] Across sorts and performance metrics, the graphical evidence in Panel A provides strong support for the negative relation between extrapolation and investment outcomes. For instance, in the top left panel, we observe that funds whose trades are consistent with extrapolation feature underperformance relative to their style peers, their benchmarks, and funds with similar risk exposures. On the contrary, funds that display contrarian behavior outperform across a variety of metrics. The right side of Panel A lends further support to our conjecture, because it shows that funds' investment outcomes worsen monotonically when going from low extrapolation beta (Q1, contrarian funds, whose average beta is -0.52) to high extraopolation beta (Q5, whose average beta is 0.84). Panel B repeats the analysis in a predictive setting, where we ask whether a recursively estimated extrapolation metric can predict future fund returns. This second approach allows to study the relation between extrapolation and performance without the look ahead bias that full-sample estimates of extrapolative behavior introduce. The results remain very similar to the ones of Panel A.

To investigate our hypothesis more formally, we perform multivariate predictive regressions of future gross yearly fund returns on the lagged fund's extrapolative behavior, recursively estimated over prior 8 quarters, plus a set of controls.[19] In the regression, we enact the within-style analysis that reflects peer benchmarking in managerial compensation by means of style $\times$ time fixed effects. Moreover, we control for fund characteristics that prior literature has linked to fund performance (details in Appendix A2.2). To obtain inference that is robust to unmodeled dependencies in fund returns over time within a fund or across funds at a given point in time, we follow Petersen (2009) and cluster standard errors both at the fund level and by time.[20]

The results of the analysis are in Table 2. In Column 1 to 5 of Table 2, we analyze the relationship between fund extrapolation and returns, using either simple fund returns, benchmark adjusted returns, CAPM alphas, FF3 alphas, and FF5 alphas. Across all specifi-

[18] The construction of these performance metrics is standard. Thus, further details are offered in Appendix A2.2.

[19] Because funds' extrapolative behavior is measured considering also stocks that enter a fund's portfolio in future quarters, some look-ahead bias remains in this analysis. In Table A4 we remove this residual form of look-ahead bias by measuring extrapolation only based on existing fund holdings and contemporaneous first-time purchases, and the results remain unchanged.

[20] Other modeling choices, such as the use of Newey-West standard errors, render the same results.

cations, the relationship between extrapolation and performance is negative and statistically significant, which indicates that funds with stronger extrapolative behavior underperform their peers, consistent with extrapolation leading to worse outcomes for fund managers. Focusing for brevity on the benchmark-adjusted return regression in Column 2, we find that the extent of the underperformance is also sizable in economic terms. We find that a one standard deviation increase in extrapolative behavior is associated with a 0.14 percentage point decrease in the fund's benchmark adjusted return, or a 23% decline relative to the average benchmark-adjusted return in the sample which equals 0.71%.

These results are robust to the addition of several controls, such as the fund's past expense ratio, size, and number of stocks managed.[21] The results are also robust to the inclusion of controls that account for alternative ways in which past stock returns can affect investor trading, namely, fund managers' disposition effect and a fund's momentum trading. With regards to the latter, we also perform an additional check in Column 6 of Table 2. In particular, to reduce the concern that our extrapolation metric captures fund managers' propensity to follow a momentum strategy, and that the negative impact of extrapolation on performance is due to momentum crashes (Daniel and Moskowitz, 2016), we exclude momentum crashes from our estimation.[22] When excluding momentum crashes from the sample, we find that the relation between fund's momentum trading and future performance improves, as one would expect. More importantly, if extrapolation captured a form of momentum trading, excluding momentum crashes should lead to a smaller negative coefficient on the extrapolation beta in Column 6. Instead, the coefficient on the extrapolation beta remains negative and strongly statistically significant and, if anything, becomes slightly larger in magnitude.

In Section 5 of the paper, we also confirm that the negative impact of extrapolation on managerial financial outcomes extends beyond performance. In particular, based on evidence in Ma et al. (2019) that managerial performance to a lesser extent also depends on achieving top-performer status and growing fund TNA, we ask whether extrapolation hurts either of these objectives. These additional tests confirm that extrapolation has a negative impact on managers. Further tests in Section 5 also investigate if managers' risk attitudes and preferences can help reconcile the negative relation between extrapolation and fund performance. Managers who extrapolate might for instance do so in an attempt to reduce the volatility of their compensation, or extrapolation might lead to lower fund returns because it offers hedging properties against bad states of nature. We find no evidence that this is

[21]The effects of the control variables go in the same direction as found in the literature. For instance, fund size and fund age are negatively correlated with fund performance (e.g. Pástor et al., 2015; Cremers and Petajisto, 2009; Gil-Bazo and Ruiz-Verdú, 2009).

[22]Following Daniel and Moskowitz (2016), we exclude the year 2001, 2002, and the second and third quarters of 2009.

the case. Overall, these results strengthen the interpretation that our extrapolation metric captures a form of bias in investor beliefs that is consistent with the leading interpretation of extrapolation in the literature.

[Place Figure 1 about here]
[Place Table 2 about here]

4. Extrapolation Bias and Asset Management Teams

Having shown that the extrapolation bias among fund managers is consistent with a bias in belief formation, we ask to what extent teams attenuate or exacerbate this bias. A simple approach used in prior literature consists of a between-subject design (e.g. Chen et al., 2004; Bär et al., 2011). Following this design, one compares how prevalent the extrapolation bias is amongst mutual funds that are team-managed versus the ones that are managed individually. However, this approach only allows to identify the causal impact of teams on the extrapolation bias if the funds that are treated (i.e., the funds that are team-managed) and the funds that are not (i.e., the ones that are managed individually) are identical in every respect except for the fact that some funds receive the treatment and some funds do not (e.g., see Chapter 2, Angrist and Pischke, 2008).

Some differences between solo-managed funds and team-managed funds can be controlled for explicitly, such as differences in fund style, fund size, and compensation. Other differences are more elusive. Specifically, compositional differences can exist between the managers of solo-managed funds and the managers of team-managed funds, which can greatly complicate the analysis and the interpretation of the results of a between-subject analysis. Suppose for instance that the managers who operate individually do not hold extrapolative beliefs, whereas the managers who work in team-managed funds hold such beliefs. In this case, the between-subject approach mentioned above would lead to the conclusion that teams exacerbate decision biases. This result would, however, not be due to teams per se. Rather, it would stem from the fundamental and unobservable differences in individual-level behavior that permeate the sample. Similarly, suppose that the managers who manage in a team display no extrapolation bias at the individual level, while the managers working at single-managed funds display a strong extrapolation bias. In this case, the between-subject approach would make it more likely that the researcher concludes that teams greatly attenuate the extrapolation bias.[23]

[23]Empirically, both cases are possible. For instance, theoretical work of Huang et al. (2019) predicts that high-skilled managers are less likely to join team-managed funds. If extrapolation is negatively correlated

The compositional differences that can bias the result of a between-subject approach are difficult to measure empirically, because most of the time the researcher is unable to observe the individual behavior of those managers who operate in teams. To address this challenge, we propose a within-subject design, whereby the trading behavior of a team-managed fund is compared with the behavior that the members of that same team show when they manage a fund individually. This setup does not require that individuals are randomly assigned to teams (Charness et al., 2012) and naturally reduces concerns of fundamental differences, observable and unobservable, between managers operating alone and managers operating in a team. To make this comparison operational, we identify a restricted sample of mutual fund teams whose managers have all at some point in their career managed a fund by themselves. We measure extrapolation in every team and in the funds that the members of the team manage individually. We then compare extrapolation in teams with extrapolation observed at the individual level by the members of the team.[24]

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 identify actual team-managed funds, we require the management teams to operate for at least four consecutive quarters to be included in the restricted sample.[25] In total we have 467 unique managers, 847 unique funds, and 308 unique teams that satisfy these conditions. That is, the restricted sample makes up 33% of our original sample of mutual funds. When measuring extrapolation for a manager or a team, we pool all mutual funds of the same manager or team and estimate the extrapolation metric at the manager or team level following the regression model outlined in Equation (1).

Table 3 shows the summary statistics for the restricted sample, and Panel A covers the solo-managed funds and Panel B the team-managed funds. A comparison between the restricted sample and the full domestic US equity sample in Table 1 shows many similarities. Funds in both the full and the restricted sample share a similar distribution of fund TNA, number of stocks held, and fund fees. Similarly, the fraction of extrapolators in the restricted

with skill, extrapolative managers might be more likely to be in team-managed funds. On the other hand, Kocher et al. (2006) show that some individuals have a strong preference for working in teams. If extrapolation is negatively correlated with the preference to work in a team, extrapolative managers might be less likely to join team-managed funds.

[24]The analysis is supplemented by a large set of robustness checks, presented later, which address the possibility that confounding effects can drive the results of our analysis.

[25]For instance, if a manager switch occurs within a given quarter, Morningstar Direct reports both managers simultaneously even though they operated consecutively. We have verified a few of such cases using fund's SAI through the SEC's EDGAR.

sample is also close to what we document for the full sample in Table 1. The main difference between the two samples is the size of the teams, which is smaller in the restricted sample. However, this is not surprising, since the likelihood that all the members of a team have at some point in their career managed a fund alone declines with the size of the team. Overall, this comparison indicates that the 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 is 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 teams for the extrapolation bias.

The second more ambitious approach estimates quantitatively the degree to which team members transmit 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 behavior (i.e., the statistical team counterfactual) with the behavior observed when the same individuals complete the same task as part of a team. 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 with the following regression:

$$\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^E + \delta_2 D_j^E + \delta_3 C_j + \epsilon_j, \tag{5}$$

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 helps to answer 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 the 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 bias 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 the evidence that the extrapolation 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 teams exacerbate the extrapolation bias, that is, $\delta_0 + \delta_1 > 1$.

Second, the regression framework outlined above can contrast the transmission of extrapolation bias 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 teams inherit contrarian behavior that is present at the individual level. Related, the coefficient δ_1 sheds light on whether teams can discriminate between behavior that decreases performance, such as the extrapolation bias, and behavior that enhances performance in the cross-section of funds, such as contrarian trading.

In this part of the analysis we rely on full-sample estimates of team-level and individual-level extrapolative behavior. This choice is due to data limitations. Simply put, there are too few cases of teams for which all members manage a fund individually at the same time, while also participating in the team. Using full-sample extrapolation estimates, however, implicitly treats extrapolation as a time-invariant feature of decision-making, be that in teams or at the individual level. Later, in the robustness section, we ask whether our results change if we would account for possible time variation in extrapolation, for instance due to learning. Related, we investigate whether the relative timing of individual and team-based asset management has any impact on our result. We find that our results are unlikely to stem from our simple design choice.

4.3. Empirical Approach: IV Methodology

Whereas we first estimate Equation (5) with standard OLS, 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 it is likely to be affected by measurement error. In the presence of measurement error in one of the regressors (uncorrelated with the error term), the coefficient estimates for that regressor are downward-biased (Champernowne, 1972). As a result, measurement error could 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 uncorrelated because both variables are estimated over 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, we randomly partition a fund's stock holdings in every quarter into two subsamples. We then separately estimate Equation (1) in both subsamples. 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 then use $\hat{\beta}_j^{CF,2}$ as an instrument for $\hat{\beta}_j^{CF,1}$ in the following 2SLS regression:

$$
\begin{aligned}
&\text{1st stage: } \begin{cases}
\hat{\beta}_j^{CF,1} & = c_1 + \lambda_{1,0}\hat{\beta}_j^{CF,2} + \lambda_{1,1}\hat{\beta}_j^{CF,2} \times D_j^{E,2} + \lambda_{1,2}D_j^{E,2} + \lambda_{1,3}C_j + u_{1,j} \\
\hat{\beta}_j^{CF,1} \times D_j^{E,1} & = c_2 + \lambda_{2,0}\hat{\beta}_j^{CF,2} + \lambda_{2,1}\hat{\beta}_j^{CF,2} \times D_j^{E,2} + \lambda_{2,2}D_j^{E,2} + \lambda_{2,3}C_j + u_{2,j} \\
D_j^{E,1} & = c_3 + \lambda_{3,0}\hat{\beta}_j^{CF,2} + \lambda_{3,1}\hat{\beta}_j^{CF,2} \times D_j^{E,2} + \lambda_{3,2}D_j^{E,2} + \lambda_{3,3}C_j + u_{3,j}
\end{cases} \\
&\text{2nd stage: } \hat{\beta}_j^{TM} = \alpha + \delta_0\hat{\beta}_j^{CF,1,pred} + \delta_1\hat{\beta}_j^{CF,1,pred} \times D_j^{E,1,pred} + \delta_2 D_j^{E,1,pred} + \delta_3 C_j + \epsilon_j,
\end{aligned}
\tag{6}
$$

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

In Internet Appendix IA1, following Jegadeesh et al. (2019), we provide the results of simulations that are aimed at testing whether the approach outlined in Equation (6) generates unbiased estimates of the coefficients in the second-stage regression. 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 of actual teams versus counterfactual teams in Table 4. Furthermore, we report the result of a difference-in-means test for a host of fund characteristics, as well as for the extrapolative behavior, of teams and counterfactuals. Generally speaking, observable fund characteristics do not differ between teams and their counterfactuals.[26] This is true for the entire sample. It is also true when we split the sample based on whether managers extrapolate at the individual level (i.e., $\beta^{CF} > 0$) or not (i.e., $\beta^{CF} < 0$).

More importantly, the last panel of Table 4 shows that in teams whose managers extrapolate at the individual level, extrapolative behavior is substantially reduced. Specifically, while the extrapolation beta of the counterfactual team equals 0.18 in teams whose members extrapolate on average, extrapolation in the actual team is close to absent. This reduction of extrapolative behavior is statistically significant at the 1% level. In stark contrast, contrarians' managers tendency to extrapolate remains sizable when these managers join a team. These findings offer introductory evidence that when managers join a team, their biased behavior gets considerably attenuated.

[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 (5):

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

The nested regression does not directly show how the extrapolation bias is transmitted to a team, because the regression does not differentiate extrapolative counterfactual teams from teams of contrarians. However, this simpler specification helps illustrate how measurement error can lead to quantitatively inaccurate conclusions on the role of teams for decision-making. Columns 1 and 2 show that the OLS estimates indicate that the transmission of individual behavior 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.

[26] The exception is the number of stocks that are in managers' portfolios. In teams whose managers extrapolate at the individual level, we find that they hold 77 stocks when operating individually and 85 stocks when they operate in a team. This difference is economically small and, as we argue in detail in Section 5, a larger number of stocks overseen by a team does not help reduce extrapolation in teams vis-a-vis solo-managed funds.

Without further analysis, a 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.[27] 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 obtained with the simpler OLS analysis, because the null hypothesis of a full transmission of individual behavior to the team is rejected at the 10%- and 5% significance level, respectively.[28]

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 concerning the transmission of extrapolation bias from individuals to teams indicate a large attenuation of individual biases. In particular the sum $\delta_0 + \delta_1$, which captures the effect of teams on extrapolation bias, is equal to about 0.35. This results is economically important, because it implies that on average extrapolative behavior at the individual level is attenuated in teams by close to 65%. This large positive team effect is also statistically significant. In particular, in all specifications we reject the hypothesis that extrapolation is fully transmitted to teams i.e., the hypothesis that $\delta_0 + \delta_1 = 1$, while we are unable to reject the hypothesis that $\delta_0 + \delta_1 = 0$, i.e., we cannot reject the null that teams fully attenuate the extrapolation bias at standard levels of significance. These results are robust to the inclusion of a large set of controls in the regressions, such as the team's average TNA, average experience, average disposition effect, and investment styles.

[Place Table 5 about here]

In constructing the team counterfactuals, we assign all the members of a team equal

[27]The critical value for the weak instrument test based on correlations proposed by Nelson and Startz (1990) is 0.057 and is based on the number of teams. We find a correlation between the two sets of disjoint extrapolation betas of 0.46.

[28]In Internet Appendix IA1.5, we show that the results for the IV methodology are not sensitive to the choice of the random sample that is used in the IV procedure. We repeat the analysis for 2,000 randomly disjointed, drawn samples. We find that, as long as the IV passes a standard weak-instrument diagnostic, for example Nelson and Startz (1990), the results are similar.

weights. These equal weights implicitly assume that all managers carry the same weight in a team's decision. In reality, some team members may have more influence on the decisions of a team than others. To address this concern, we compute alternative team counterfactuals which assign higher weights to those team members that have more experience and, as such, may have a larger influence on the decisions of a team. Formally, we compute three alternative team counterfactuals as weighted averages of team members' extrapolation, where managers' weights are based on: (i) quarters of industry experience; (ii) the number of funds managed; (iii) and the aggregate size of the funds managed prior to team formation. All three metrics are measured as of the first quarter in which a fund manager appears in the team.[29] Table A3 of the Appendix shows the results of our main analysis using these alternative team counterfactuals. The conclusions remain unchanged for all three metrics. So, taking all findings together, we show evidence of the attenuation of extrapolation bias in teams.

5. Robustness

We perform several robustness checks which we summarize here for convenience and present in detail later in this section. Our robustness checks have two main goals. Our first goal is to probe the measurement of extrapolation for robustness. We therefore modify several aspects of the econometric procedure to measure fund managers' extrapolative behavior. We show that our results remain unaltered across a large set of such modifications, that is, the extrapolation bias has a negative predictive power for future investment performance and this bias is largely attenuated in teams. In addition, we strengthen the interpretation of extrapolation as a bias by (i) showing that alternative fund performance metrics that influence managerial compensation are negatively associated with extrapolation; (ii) showing that the lower average returns that are earned by extrapolative managers are difficult to explain based on investor preferences such as risk aversion, hedging motives, or preferences for skewness.

The second goal of this section is to ask whether confounding effects may drive the observed reduction of extrapolation bias in teams. In this respect, time-varying managerial characteristics or time-varying fund policies can confound our results. Whereas there can be many changes in the characteristics of managers and the policies implemented by fund families over time, for our purpose we are primarily interested in those changes that can explain why biases seem less pronounced in teams. We therefore argue a learning story that involves managers or fund families learning about the extrapolation bias is the most impor-

[29] For example, if weights are based on quarters of experience, and one manager has 1 quarter of experience and the other one 19 at the time of team formation, then the $\hat{\beta}_j^{CF}$ metric is constructed with weights that are equal to 5% and 95%, respectively.

tant one, and we devote several tests to investigate such a story. Furthermore, differences in fund characteristics between the solo-managed and team-managed funds in our sample could exist that explain our main result. We argue that some differences in fund characteristics have a better potential to explain our result. In particular, differences in managerial incentives between solo-managed and team-managed funds could explain our result: incentives are generally thought of to induce more rational behavior, and thus if incentives were steeper in teams than in individual funds, such a difference in incentive structure could explain the result. We investigate this explanation by hand-collecting data from mutual funds' statement of additional information (SAI) filed with the SEC. Our analysis does not deliver evidence that incentives are stronger in teams than in individual funds.

Aside from these two important checks, we also consider several additional confounders: (i) fund managers could experience a systematic decrease in workload when managing in a team, allowing them to have better judgement at a time of lower cognitive overload; (ii) differences in style between solo-managed funds and team funds could reconcile the result; (iii) managers' work tasks at different firms when operating individually and as part of a team could explain our result. As we discuss later, we do not find empirical evidence consistent with these alternative explanations.

5.1. Is Extrapolation Truly a Bias?

5.1.1. Alternative Performance Metrics

Besides fund returns, past literature argues that to a lower extent managerial compensation is based on attracting fund flows (Ma et al., 2019) and the probability of becoming a top performer (Guercio and Tkac, 2008). We therefore test for the relation between extrapolation and these additional outcomes in Table 6.

Fund flows are important for two reasons. First, fund managers can have direct incentives to increase the fund's total net assets to increase their compensation. Second, despite the negative performance associated to extrapolation, managers may still extrapolate if they were catering to the tastes of their investors. In Columns 1 and 2 of Table 6, we find that extrapolative funds experience outflows relative to other funds. This result holds both when analyzing percentage flows and when using a ranking measure for fund flows, that takes a higher value for the funds that receive the larger inflow.

We then analyze top performance for two reasons. First, some managerial contracts explicitly reward top performance (e.g. Ma et al., 2019, Internet Appendix example 1). Second, Guercio and Tkac (2008) find that the gap between funds with a 5-star and 4-star Morningstar rating is much larger than that of 4-star and 1-star rated funds when it comes to attracting investor flows, where 5-star rated funds are those rated in the top 10% on a proprietary style and risk-adjusted basis. Thus, we ask whether extrapolative behavior hinders funds' ability to attain a top-fund status. We measure top performance as a dummy variable that is equal to one if a fund's raw return gross of fees ranks in the top 10% (Column 3) or 5% (Column 4) of the performance distribution in a fund's Morningstar style classification over the following year. Once again, the evidence points to a negative relation between extrapolation and fund managers' outcomes, because extrapolation predicts a lower probability of achieving top-fund status.

[Place Table 6 about here]

5.1.2. Extrapolation, Beliefs, and Preferences

The results so far support the interpretation of our extrapolation metric as a proxy for fund managers' tendency to form biased beliefs about future stock returns. However, our measure is based on investor trading behavior, and trading decisions are likely driven by both beliefs and preferences. Therefore, it is possible that our measure of extrapolation reflects fund managers' preferences rather than the tendency to form incorrect expectations. Although it is difficult to rule out that our extrapolation metric captures preferences, we investigate empirically how plausible this alternative interpretation is in practice.

To this end, we note that insofar as our extrapolation metric reflects managers' preferences, then the managers that we identify as extrapolators are likely to trade-off the lower expected payoffs associated with extrapolation (Table 2) against other features of the distribution of payoffs that they value based on their preferences and that they are able to achieve by buying (selling) stocks with higher (lower) past returns. We consider three distinct possible trade-offs. First, we hypothesize that fund managers who extrapolate may accept lower expected performance and yet trade based on past returns because this trading behavior leads to less volatile returns, which is desirable based on a broad notion of managers' aversion to risk. Second, we posit that some managers may prefer a more positively skewed performance distribution. Prior work indicates that this preference for lottery-like outcomes exchanges extremely positive low-probability events against lower expected payoffs on average (Barberis and Huang, 2008, e.g.). Thus, managers who trade in the direction of extrapolation may do so because, while this behavior lowers expected investment performance, it also leads to more positively skewed performance. Finally, preferences can lead some fund managers to buy and sell stocks based on these stocks' past returns if this investment behavior helps hedge against underperformance in bad states of the economy, when the marginal utility of consumption is higher (e.g. Breeden, 2005; Campbell and

Cochrane, 1999), and agents may face increased income risk (Betermier et al., 2012) or a more negatively-skewed income distribution (Betermier et al., 2012; Guvenen et al., 2014).

Importantly, the three potential trade-offs mentioned above can be investigated empirically by extending the scope of the tests presented earlier. Specifically, to test whether those who we identify as extrapolators achieve lower payoffs in exchange for lower compensation volatility, we study the relation between extrapolation and the volatility of fund performance, measured as the standard deviation of monthly benchmark adjusted returns and FF5 alphas over the next 12 months. Since managers' compensation is tied to their performance, lower performance volatility translates into a lower volatility of managers' compensation. Similarly, when testing whether extrapolation leads to extremely positively skewed payoffs, we ask whether extrapolation helps funds rank among the very top of their Morningstar style category in the following year (i.e., top 5 funds or top 10 funds), as this placement coincides with extremely positive financial outcomes. Finally, to investigate whether extrapolation generates hedging benefits, we study the performance of funds whose trades are extrapolative vis-a-vis other funds in bad states of nature, either identified by a negative return to the market or by the recessionary state of the economy in that year.

In Panel A of Table 7, we test whether extrapolation is associated with lower return volatility. We find no evidence that extrapolation leads to significantly lower volatility of returns. Further tests in Table A5 also show that extrapolation leads to lower benchmark-adjusted alphas.

In Panel B of Table 7, we test whether extrapolation places managers at the very top of the performance distribution, but we find no evidence that this is the case, rather, we find evidence that the opposite is true. In Panel C of Table 7, we analyze the relationship between extrapolation and fund performance, but zoom in on years in which the CRSP weighted-market index was negative. We find that extrapolation does not lead to better performance during times of poor market returns. When repeating the analysis in Panel D for years corresponding to NBER recessions, we find insignificant effects of extrapolation on performance. However, when comparing the results in Panel D with the earlier unconditional results of Table 2, extrapolative funds achieve worse performance in recession years as they do on average. So, we conclude that extrapolation does not lead to better hedging properties.

All in all, these results do not offer evidence for a preference-based interpretation of extrapolation. Instead, these results reinforce the notion that the negative relation between extrapolation and performance is due to managers' biases in expectations formation.

[Place Table 7 about here]

5.1.3. Measuring Extrapolation

In this section we provide robustness to the way we measure extrapolation and show that our results are robust to: (i) the exclusion of momentum stocks from the sample when measuring extrapolation; (ii) using weight changes instead of portfolio trades; (iii) using realized past year stock returns; (iv) different assumptions about the investment universe; and (v) allowing for asymmetric flow-induced trading.

To construct our no-momentum extrapolation metric, we re-estimate the extrapolation metric for a fund by excluding stocks that are classified as momentum stocks at the end of quarter $t-1$.[30]

To show that our results are robust to alternative ways of measuring managers' trading decisions, we also re-estimate our extrapolation metric using portfolio weight changes as our left-hand side variable in Equation (1). 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. Therefore, to capture the component of funds' trading that is due to active choices by fund managers, we define weight changes as:

$$\Delta w_{s,j,t+1} = w_{s,j,t+1} - \frac{(1 + r_{s,t \to t+1})}{(1 + r^P_{j,t \to t+1})} w_{s,j,t}, \tag{7}$$

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 of stocks.

Furthermore, to investigate whether our results hinge on the use of decaying weights in the estimation of managers' extrapolative behavior (Equation 3), we re-estimate our extrapolation metric using simple realized past year stock returns as our main regressor of interest in Equation (1).

Table 8 replicates our main results using the alternative extrapolation metrics. Panel A studies the relation between extrapolation and managerial performance. Panel B asks whether extrapolation is reduced in teams. Across all of the six distinct tests conducted in Panel A, the relation between extrapolation and performance appears negative, with five out of six showing a statistical significant relation. More importantly, Panel B shows that the evidence that teams help alleviate extrapolation bias is confirmed when using the alternative

[30] Following the literature on momentum, momentum stocks are those stocks that are in the top or bottom deciles of the distribution of cumulative stock returns at time $t-1$ (Jegadeesh and Titman, 1993; Asness, 1995; Fama and French, 1996). We define a stock's cumulative return as the past 1-year return of the stock, with a 1-month gap between the cumulation ends and portfolio formation.

extrapolation metrics presented in this section.

[Place Table 8 about here]

Likewise, to show that our results do not rely on the specific form of the investment universe that we choose for our main extrapolation metric, we also provide robustness to the investment universe that is used to estimate the extrapolation beta. In particular, in Table A4 of the Appendix, we show that our results remain unaltered when we assume that the investment universe only incorporates those stocks that the investors start to hold in the following year, instead of the following 11 quarters. Additionally, we show that our results are robust to a specification where we abstract from defining the investment universe and only include actual managers' trades.

Finally, in our main specification of fund trades, extrapolative behavior is measured while controlling for flow induced trading. However, the analysis in Lou (2012) shows that the trading that is induced by inflows and outflows can be different. Therefore, in Table A4 of the Appendix, we show that our results are robust to such asymmetric flow-induced trading. Specifically, in Equation (1) we include a dummy variable that indicates whether the fund received inflows from quarter t to $t+1$, $D_{j,t+1}^{inflows}$, interacted with the variables in $F_{s,j,t}$. The dummy $D_{j,t+1}^{inflows}$ equals one if $flow_{j,t+1} > 0$ and zero otherwise. Using this approach we re-estimate our extrapolation metric and perform all our analyses again. The results stay the same.

5.2. Can Other Channels Explain the Team Effect?

5.2.1. Learning and Experience

Suppose that fund managers learn from experience that the extrapolation bias hurts their performance, and hence they progressively extrapolate less. If fund managers manage first a fund individually, and only later join a team, then learning outside of the team could explain why the extrapolation bias is reduced drastically in teams. By the same token, a fund family might learn about a manager's extrapolative behavior early during the manager's tenure. Later, the fund family could use this information to implement policies or incentives that curb the manager's extrapolation, and such policies might happen concurrently with the manager's participation in a team fund.

To address this concern, we perform a number of tests. First, we note that for learning outside of the team to be an explanation of our findings, fund managers should on average exhibit a much larger accrued experience by the time they work in a team compared to the time when these managers manage a fund individually. Summary statistics on fund managers' industry tenure when managing individually and when in a team are in Table 3. These statistics indicate that there is only a difference of three quarters between the average experience that managers have accrued when we observe them acting in teams as opposed to individually. This small difference provides preliminary evidence against a learning explanation. We then provide a first formal test and estimate the following regression:

$$\begin{aligned}\hat{\beta}_j^{TM} &= \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^E \times \Delta Experience_j + \delta_2\hat{\beta}_j^{CF} \times D_j^E + \delta_3\hat{\beta}_j^{CF} \times \Delta Experience_j \\ &+ \delta_4 D_j^E \times \Delta Experience_j + \delta_5 D_j^E + \delta_6 \Delta Experience_j + \delta_7 C_j + \epsilon_j, \qquad (8)\end{aligned}$$

where $\Delta Experience_j$ indicates the difference in team members' industry experience (a proxy for learning) when managing as part of team j versus when they manage alone. If experience and learning drive our results, we expect the coefficient on the double-interaction term δ_1 to be statistically significant and negative, i.e., a stronger reduction of the extrapolation bias should occur when fund managers have accumulated more experience prior to joining a team. Panel B of Table 9 shows the results of the estimation. The coefficient δ_1 is statistically indistinguishable from zero and therefore we do not find evidence in favor of the learning story outlined above.

In our second test, we argue that the learning argument described above is natural when fund managers first work individually and then join a team. On the other hand, if working in a team precedes working individually, learning would be achieved during the years of team management, and thus learning predicts that one should observe a lower tendency to extrapolate in single-managed funds than in team-managed funds. In our sample, 65 belong to the case in which individual precedes team-managed management, whereas 54 belong to the case in which team-managed precedes individual management. We flag all teams that belong to the former case using a dummy D_j^{ES} and all the teams that belong to the latter case using a dummy D_j^{ET}. We then estimate the following regressions:

$$\begin{aligned}\hat{\beta}_j^{TM} &= \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^E \times D_j^{ES} + \delta_2\hat{\beta}_j^{CF} \times D_j^E + \delta_3\hat{\beta}_j^{CF} \times D_j^{ES} \\ &+ \delta_4 D_j^E \times D_j^{ES} + \delta_5 D_j^E + \delta_6 D_j^{ES} + \delta_7 C_j + \epsilon_j, \qquad (9)\end{aligned}$$

$$\begin{aligned}\hat{\beta}_j^{TM} &= \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^E \times D_j^{ET} + \delta_2\hat{\beta}_j^{CF} \times D_j^E + \delta_3\hat{\beta}_j^{CF} \times D_j^{ET} \\ &+ \delta_4 D_j^E \times D_j^{ET} + \delta_5 D_j^E + \delta_6 D_j^{ET} + \delta_7 C_j + \epsilon_j. \qquad (10)\end{aligned}$$

The coefficient δ_1 on the double interaction term in either of the regressions above is the main coefficient of interest. In Equation (9), the coefficient captures whether the transmission of extrapolation bias is different for teams whose managers start out as single managers (i.e., $D_j^{ES} = 1$) versus other teams. Learning predicts that δ_1 is negative, i.e., compared to other teams, teams in which all managers had the opportunity to learn from their individual experiences prior to joining the team should exhibit the smallest transmission of extrapolation bias to teams. Conversely, the coefficient δ_1 in Equation (10) tests whether the attenuation of extrapolation bias is different for teams whose managers start out as part of a team. A learning story predicts that δ_1 is positive. To see why, suppose that fund managers learn that extrapolation is harmful while working in a team, and later move to a solo-managed fund. Compared to other managers, these managers are likely to exhibit a weaker extrapolative behavior when they move on to work individually, thus rendering a positive coefficient δ_1.

Panel C of Table 9 reports the results of the estimation of Equation (9) and Panel D of Equation (10). In both panels, the triple interaction term is not statistically significant, indicating that there is no evidence of differences in the attenuation of extrapolation bias that a learning explanation would suggest. Whereas the lack of statistical significance could be due to our small sample, the result appears overall inconsistent with a learning story. The coefficient δ_1 is positive in Panel C, but negative in Panel D, whereas learning implies a negative δ_1 in Panel C, but a positive δ_1 in Panel D. Overall, these results do not support learning as a plausible explanation for our finding.

In our final check we focus on the sample of *non-learners*, i.e., managers whose extrapolative behavior is not reduced considerably over the years in which these managers operate individually. If learning outside of the team is the driver of our result, we expect not to find an attenuation of extrapolation bias in the teams composed of non-learner managers. To identify 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 fund alone. We then select the non-learners as the managers who extrapolate in both subsamples.[31]

Figure 2 presents scatter plots that relate team-level extrapolative behavior to team members' average extrapolative behavior at the individual level. The figure portrays this relation for all teams (Panel A), the teams whose members extrapolate on average (i.e., extrapolative teams, Panel B), and for the extrapolative teams that consists of the non-learners and their teams only (Panel C). Panel A and B present in graphical form the evidence on the attenuation of extrapolative behavior in teams that we present in Table 5.

[31]To validate our definition of non-learners, we compare the extrapolation metric of the early and late sample and find for this group an average value of 0.19 for the early sample and an average value of 0.24 for the late sample. A difference in means test reveals that these values are statistically indistinguishable from each other (t-stat $= -1.29$).

More importantly, a comparison between Panel B and C reveals that the extrapolation bias is attenuated both in teams of learners and in teams of non-learners in virtually the same way. 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. Overall, this result confirms the robustness of our finding to a learning story. As a matter of fact, the finding that teams alleviate biases even among managers that display little tendency to learn when operating alone suggests that teams could serve as a stronger device to curb biases than individual experience.

[**Place Figure 2 about here**]
[**Place Table 9 about here**]

5.2.2. Portfolio Managers' Compensation

A second alternative interpretation of our findings is that the attenuation of extrapolation bias is driven by systematic differences in portfolio managers' compensation between team-managed 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.[32] 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. 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 managers working in a team with the compensation

[32] 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.

structure of the counterfactual, that is, the average compensation structure of the managers that compose the team when they manage alone.

We have a total of 203 teams that appear in our restricted sample as of 2006q1, of which 99 teams are defined as extrapolative teams. Of these teams, we were able to collect data on 125 teams, out of which 53 are extrapolative teams. Table 10 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. Because teams face compensation structures 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 are unlikely to rationalize our result.

[Place Table 10 about here]

5.2.3. Manager Workload and Bounded Rationality

Prior work shows 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, our evidence that biases are reduced when managers operate in teams may stem from a loosening of such constraints in team-managed funds compared to solo-managed funds.

A loosening of the 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 is the case, its indeed 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, but rather stem from differences in workload between solo and team 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 more binding in solo-managed funds as opposed to team-managed funds. If this is the case, the reduction of extrapolation bias could also be achieved in single-managed funds if these 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 at a given point in time with the number of stocks that the manager oversees in any of the portfolios he manages, be that alone or in a team. We start by constructing a time series of the total number of stocks the manager has in their investment universe at any given quarter t.[33] 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 in the investment universe of a team to one of the team managers, we assume that a manager who holds 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 oversees at the same time. For example, if a manager simultaneously manages 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 define the workload as:

$$Workload_{i,t} = \sum_{j \in S_{i,t}} U_{j,t} + \sum_{j \in C_{i,t}} \max \left[\frac{U_{j,t}}{NumManagers_{j,t}} - Overlap_{i,j,t}, 0 \right], \tag{11}$$

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 additional assignments of a manager do not decrease their workload.

We obtain the workload of the members of team j when operating in the team and when 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 team j versus when they operate in the counterfactual team, which we define as $\Delta Workload_j$. The summary statistics in Panel A of Table 9 highlight the importance of

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

this robustness check. We observe that managers have lighter workloads when managing in teams compared to when they manage individually. On average, managers in team-managed funds oversee 37 fewer stocks compared to when they manage funds on their own.

Although the simple comparison of workloads in teams and solo-managed funds provides some support for the idea that workload explains our result, we then perform additional analysis. Specifically, for workload reduction to rationalize our result, one should find that the observed reduction of extrapolation bias in teams is particularly pronounced when the workload at the time of team management declines. Thus we estimate a regression that is similar to the one in Equation (8), where we replace $\Delta Experience_j$ with $\Delta Workload_j$. If the differential workload of managers drives our results, we expect coefficient δ_1 to be statistically significant and positive. In words, if a decrease in workload during team-management is responsible for the attenuation of extrapolation bias in teams, then extrapolation bias ought to be reduced more in teams where $\Delta Workload_j$ is low, i.e. teams where such a reduction in workload is more pronounced. Panel F of Table 9 shows that δ_1 is statistically indistinguishable from zero. We take this as evidence that managers' reduction in workload once they join the team does not drive our results.

5.2.4. Style Migrations

An additional alternative interpretation of our findings is that the attenuation of extrapolation bias is driven by team members systematically migrating from one style to another when transitioning from single to team management. If differences in extrapolative behavior exist between 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. Adding stock controls reduces the extent to which differences in extrapolation across managers are due to funds' size-based and value-based style classifications. Third, we formally test whether our results of bias reduction in teams are due to style migrations by performing a double interaction regression:

$$\begin{aligned}\hat{\beta}_j^{TM} &= \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^E \times D_j^{SM} + \delta_2\hat{\beta}_j^{CF} \times D_j^E + \delta_3\hat{\beta}_j^{CF} \times D_j^{SM} \\ &+ \delta_4 D_j^E \times D_j^{SM} + \delta_5 D_j^E + \delta_6 D_j^{SM} + \delta_7 C_j + \epsilon_j, \qquad (12)\end{aligned}$$

where D_j^{SM} is an indicator variable that is equal to one in the case of a style migration for team j. In order to construct this indicator variable, 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 available in Morningstar. Out of 308 teams, 73 experienced style migrations, or 24% of the teams (Panel A, Table 9). If the style migrations drive our results, we expect the coefficient δ_1 to be statistically significant and negative. Panel E of Table 9 shows that δ_1 is statistically indistinguishable from zero. We take this as evidence that style migrations do not drive our results.

5.2.5. Mutual Fund Family Switches

A final alternative interpretation of our finding is that the attenuation of extrapolation bias is driven by team members systematically moving from one mutual fund family to another when transitioning from single to team management. If fund families have different policies with respect to the abatement of extrapolation bias, then fund managers switching between mutual fund families could generate evidence consistent with our findings.

To address this concern, we first identify the mutual fund family of the teams and solo managers as the family for which these managers worked most of the time during their years while part of a team and while managing individually, respectively. A mutual fund family switch is then defined as a discrepancy between the mutual fund family of the team and the family of the managers when they operate individually.[34] We then estimate a regression with a similar double-interaction term as in Equation (12), where we replace D_j^{SM} with D_j^{FS}, and where D_j^{FS} indicates a fund family switch for team j.

Table 9 shows that only in 40% of the cases there is a switch between the mutual fund family going from solo to team management. If mutual fund family switches drive our results, we expect the coefficient δ_1 to be statistically significant and negative. Panel G of Table 9 shows that δ_1 is statistically indistinguishable from zero. We therefore conclude that switches in mutual fund families cannot explain our result.

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 provides insights into optimal team design and how teams can effectively deliver on the

[34] For instance, if manager A and B work primarily for Vanguard during their solo years, but as a team the managers work for Fidelity, then we define this case as a discrepancy between the mutual fund family of the team and the solo managers. Likewise, if manager A during its solo years as well as the team primarily work for Vanguard, but manager B works primarily for Fidelity during its solo years, then we also define this case as a family switch.

promise of attenuating individual-level biases.

6.1. Conceptual Framework

A large body of work in psychology and behavioral economics argues that 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, 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 from System II 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 often relies on heuristic rules. Heuristics 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 suboptimal investment decisions (Benartzi and Thaler, 2007).[35] Like the literature, we argue that the extrapolation bias among asset managers stems from the influence of System I on these managers' investment decisions.[36]

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 depending on 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

[35]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. (2021) offer examples of how simple rules of judgment can do as well or better than complex rule-based approaches to decision-making.

[36]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).

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 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.[37] 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.

To see this difference, consider the workings of the internal reflection mechanism in a team only composed 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

[37] 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).

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:

$$\hat{\beta}_j^{TM} = \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^{AE} + \delta_2\hat{\beta}_j^{CF} \times D_j^{M} + \delta_3 D_j^{AE} + \delta_4 D_j^{M} + \delta_5 C_j + \epsilon_j, \quad (13)$$

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 $\delta_2 < \delta_1$; in words, there will be less transmission of individual-level behavior to mixed teams compared to all-extrapolator teams.

Table 11 presents the estimation of the model in Equation (13). We find in all specifications that $\delta_1 < \delta_2$ and δ_1 is statistically significant at the 1% significance level, whereas δ_2 is statistically indistinguishable from zero. Because of the small sample at our disposal and measurement error that biases the coefficients δ_1 and δ_2 downwards, we cannot reject the hypothesis that $\delta_1 = \delta_2$ at conventional statistical levels. However, δ_1 is almost twice as large as δ_2 in absolute terms, suggesting a stronger 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. While this result offers a first glance into how teams can reduce extrapolation bias, we leave a deeper analysis to future work.

[Place Table 11 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 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 from 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 first step towards 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, what role do agency considerations play in the attenuation of biases that we find? And what situational factors (e.g., the characteristics of an investment or task) or team members' personality traits are most useful for obtaining a reduction in cognitive biases in team decision-making? How are behavioral biases that stem from investor preferences (e.g., realization utility, belief-based utility, prospect theory) dealt with in teams? These are only some of the questions that we leave to future work.

Appendix

Appendix A1 Preparing the mutual fund dataset

In this appendix, we explain how we merged the CRSP, Morningstar, and Thomson Reuters databases. 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 merged database to mutual fund holdings data from s12 filings, obtained from CRSP and Thomson Reuters.

A1.1 Cleaning raw CRSP database

We download the monthly returns (mret), size per share class (mtna), tickers (ticker), and cusip numbers (ncusip) from the raw CRSP database over the period 1979M1-2019M9. We first delete observations for which total net asset values or returns are missing within a given month. The number of observations then equals 6,986,661 and there are 66,453 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 2,970 *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 24,699 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 3,834 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 12,131 *CRSP fundno*'s with time-varying cusips.

3. There are 8,185 combinations of cusip and month that correspond to more than one *CRSP fundno*.

4. There are 247 cusips with multiple *CRSP fundno*'s.

A1.2 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 over the period 1980M1-2019M12. 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.

A1.3 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.

Finally, we select CRSP fund share classes that are defined as domestic equity, but exclude the index funds. We only keep quarterly observations and the period we consider is 1980Q1-2018Q4. We end up with a final dataset of 436,984 observations. The merge matches 80% of the 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.

A1.4 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 A2 Control Variable Description

A2.1 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 (weighted) yearly stock 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. 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, we include a stock's past one month return to control for short-term reversals, because stocks with low past one-month returns have high returns in the subsequent period (Lehmann, 1990).

An additional 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 for stock characteristics such as size, book to market, profitability, and investment (asset growth).

We furthermore control for flow-induced trading. Lou (2012) shows that flows in the presence of liquidity costs lead to disproportionately buying and selling of certain stocks over others. In particular, he shows that funds do not increase their holdings proportionally after inflows, but expand the set of stocks they invest in. Moreover, he shows that funds buy (sell) less of more illiquid stocks after inflows (outflows). We therefore follow Lou (2012) and include two measures of liquidity costs as controls (i) the percentage of all shares outstanding of a stock that is held by the fund and (ii) the effective bid-ask spread of a stock. We obtain the bid-ask spread for each stock from the Open Source Asset Pricing website (Chen and Zimmermann, 2022). We also interact both measures of liquidity costs with contemporaneous fund flows. Unlike Lou (2012), we do not include fund flows and fund-level liquidity costs as separate independent variables, because they are subsumed by the time fixed effects that we include in our regressions.

A2.2 Mutual Fund Performance Measures and Control Variables

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 three distinct metrics: (i) fund returns, (ii) whether a fund is a star fund in a given quarter, and (iii) fund flows.

Regarding the fund returns we focus on fund raw returns and benchmark adjusted returns (defined as the fund return minus the return of the primary prospectus benchmark) as well as three measures of risk-adjusted returns: CAPM, Fama-French 3-factor (Fama and French, 1993), and Fama-French 5-factor alphas (Fama and French, 2015). We consider returns gross of fees as these are the returns that are relevant for manager's compensation. Because gross returns are not directly observable in CRSP data, we follow past work (e.g. Fama and French, 2010) and add the most recently available expense ratio to fund net returns. To estimate a fund's risk-adjusted return 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 year by subtracting the portion of fund returns that are the result of factor exposures from the fund's returns over that period. Using these factor exposures, we also estimate fund manager risk as the standard deviation of monthly 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 year. A star fund is one that ranks in the top 10% of yearly returns in its respective Morningstar style category. 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, we also analyze how extrapolation is related to mutual fund flows. Flows for fund j are defined as dollar inflows or outflows in a year (DF_{jt}), as a percentage of yearly lagged fund size, ($TNA_{j,t-1}$) :

$$flow_{j,t} \;=\; \frac{DF_{j,t}}{TNA_{j,t-1}}. \tag{A1}$$

When estimating how extrapolation affects mutual fund flows, we also control for the fund's CAPM alpha over the past year, because Barber et al. (2016) and Berk and Van Binsbergen (2016) document that CAPM alphas predict mutual fund flows.

When studying how mutual fund manager performance depends on extrapolative behavior, it is important to control for other variables that are related to both factors to ensure our results are not driven by omitted variables. To this end, we start by controlling for fund characteristics, such as expense ratios, fund size, and past fund flows. 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. 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, 2019). Furthermore, funds with large outflows can experience high trading costs due to fund liquidity constraints that alter fund performance (Coval and Stafford, 2007).

We also control for manager characteristics, such as managers' experience and trading behavior, which can be related to the tendency to extrapolate returns and have been extensively documented as having a relationship with fund performance (Golec, 1996; Chevalier and Ellison, 1999). To the extent that experienced managers are less likely to suffer from behavioral biases, manager experience controls for this effect. We also control for trading behavior by including mutual fund turnover ratios from CRSP and the number of stocks held in the mutual fund portfolio, because high trading activity may relate to performance as documented by Wermers (2000) and Cremers and Petajisto (2009). The age of the fund is typically negatively related to fund performance (e.g. Cremers and Petajisto, 2009), so we control for fund age since inception in our tests as well.

Importantly, we distinguish extrapolation from the disposition effect by controlling for the disposition effect explicitly in all of our tests. We follow Odean (1998) and Cici (2012) and estimate the disposition effect for every fund in our sample in a given quarter. For each fund j in a given quarter t, we start by estimating a funds' proportion of realized gains (PRG) and proportion of realized losses (PRL) from its holdings:

$$PRG_{j,t} = \frac{RG_{j,t}}{RG_{j,t} + UNRG_{j,t}}, \quad PRL_{j,t} = \frac{RL_{j,t}}{RL_{j,t} + UNRL_{j,t}}, \tag{A2}$$

where $RG_{j,t}$ is the number of realized gains, $UNRG_{j,t}$ the number of unrealized gains, $RL_{j,t}$

the number of realized losses, and $UNRL_{j,t}$ is the number of unrealized losses. The disposition effect is then calculated as the difference between these two proportions: $DISP_{j,t} = PRG_{j,t} - PRL_{j,t}$.

To define which positions of a fund are at a loss or gain, we need to define a cost basis for the purchase price of the stocks held by the fund. We follow the Odean (1998) approach by using the average purchase price weighted by the number of shares in each given purchase and corrected for stock splits. Given that we only observe share prices at the end of each quarter, we assume purchases and sales occur at the quarter end.

Similarly, to distinguish extrapolation from momentum trading explicitly, our regressions control for a fund's momentum trading strategy. To this end, we estimate a standard 4-factor Fama-French Carhart (Carhart, 1997) model for each fund over a 5-year rolling window using monthly returns. We use the fund returns' loading on the momentum factor from this model, β_{MOM}, as a control in our tests.

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Table 1. **Summary statistics all active domestic US equity funds**: This table reports the summary statistics of full sample fund extrapolation metrics and factor exposures, and time-series averages of the remaining variables for our domestic US equity fund sample for the period 1980Q1-2018Q4. The fund's *Extrapolation Beta* is the extrapolation metric defined in Section 2.2; *Extrapolation Dummy* is equal to 1 whenever the fund's extrapolation beta is positive; *Extrapolation Beta (No Mom)* is the same metric estimated when excluding momentum stocks; *Extrapolation Beta (Weight Change)* is the same metric estimated with portfolio weight changes on the left-hand side of Equation (1); *Extrapolation Beta (Past Year Ret)* is the same metric estimated using past 1 year realized stock returns as the main regressor on the right-hand side of Equation (1). The alternative extrapolation metrics are described in Section 5.1.1. *Fund TNA* is the fund's total net asset value in millions of dollars; *Team-Managed* is a dummy variable equal to 1 if a fund is managed by a team; *Number of Managers* is the number of managers managing a fund; *Manager Experience* is the average experience of the managers in quarters; *Number of Stocks* is the number of stocks held by a fund; *Fund Age* is the age of the fund since inception in years; *Expense Ratio* is the fund's total expense ratio in percentage points; *Fund Turnover* is the CRSP turnover ratio of a fund; β_{MOM} is the fund's exposure to the momentum risk factor in a Fama-French-Carhart (FFC) model; *Disposition* is the fund-level disposition effect (Section A2.2); *Fund Return* is the fund's yearly raw return; *Benchmark Adj. Return* is the fund's yearly return in excess of the fund's primary prospectus benchmark; *CAPM Alpha* is the cumulative CAPM yearly alpha; *FF3 Alpha* is the cumulative Fama-French (FF) 3-factor yearly alpha; *FF5 Alpha* is the cumulative FF 5-factor yearly alpha; *Flow* is the yearly fund inflow; *Benchmark Adj. Volatility* is the volatility of monthly benchmark-adjusted returns over 12 months; *FF5 Alpha Volatility* is the volatility of monthly FF5 alpha over 12 months. The performance metrics are estimated on gross-of-fee returns and in percentage points. Panel B reports pairwise correlations of the aforementioned extrapolation metrics, and the correlation of funds' extrapolative behavior with a fund's momentum loading, disposition effect, and factor loadings from a FF5 model.

Panel A: Summary statistics

	Mean	St. Dev.	5th Pct.	Median	95th Pct.
Mutual fund characteristics					
Extrapolation Beta	-0.04	0.45	-0.64	-0.01	0.46
Extrapolation Dummy	0.46	0.50	0	0	1
Extrapolation Beta (No Mom)	-0.03	0.65	-0.83	-0.01	0.60
Extrapolation Beta (Weight Change)	-0.03	0.41	-0.62	-0.01	0.44
Extrapolation Beta (Past Year Ret)	0.00	0.10	-0.12	0.00	0.10
Fund TNA	983.78	2075.11	18.47	288.84	4247.04
Team Managed	0.65	0.36	0	1	1
Number of Managers	2.62	1.87	1	2	6
Manager Experience	30.15	14.76	10.58	27.99	58.57
Number of Stocks	84.14	121.73	22	55	228
Fund Age	13.39	11.44	3.89	10.01	36.86
Expense Ratio	1.21	0.38	0.64	1.19	1.87
Fund Turnover	0.81	0.58	0.19	0.69	1.82
β_{MOM}	0.01	0.11	-0.14	0.00	0.20
Disposition	-0.02	0.07	-0.13	-0.01	0.08
Mutual fund performance					
Fund Return	10.41	4.42	2.85	10.76	15.85
Benchmark Adj. Return	0.71	3.03	-2.96	0.47	5.00
CAPM Alpha	0.31	3.51	-4.77	0.31	5.31
FF3 Alpha	-0.25	2.78	-4.45	-0.22	3.64
FF5 Alpha	0.00	3.10	-4.21	-0.20	4.58
Flow	21.51	55.46	-12.27	9.49	87.04
Benchmark Adj. Volatility	1.62	0.83	0.65	1.46	3.05
FF5 Alpha Volatility	1.30	0.61	0.61	1.19	2.32

Panel B: Correlation table

Extrapolation Beta	1										
Extrapolation Beta (No Mom)	0.77	1									
Extrapolation Beta (Weight Change)	0.87	0.66	1								
Extrapolation Beta (Past Year Ret)	0.81	0.51	0.77	1							
β_{MOM}	0.19	0.20	0.15	0.11	1						
Disposition	-0.18	-0.24	-0.13	-0.11	-0.61	1					
β_{MKT}	0.01	0.02	0.02	0.02	-0.10	0.00	1				
β_{SMB}	0.00	-0.01	0.00	0.00	0.23	-0.01	0.16	1			
β_{HML}	-0.21	-0.19	-0.19	-0.17	-0.59	0.51	-0.10	-0.08	1		
β_{RMW}	-0.07	-0.08	-0.06	-0.05	-0.26	0.27	-0.07	-0.08	0.60	1	
β_{CMA}	-0.19	-0.16	-0.19	-0.17	-0.18	0.27	-0.11	-0.19	0.39	0.44	1

Table 2. **Extrapolation as a bias – evidence from investment performance**: This table reports coefficient estimates from a regression of future yearly fund performance on the lagged fund's extrapolative behavior. We analyze fund performance using the following metrics: raw returns in Column 1; returns in excess of the benchmark (BM Adj. Ret) in Column 2; CAPM Alpha in Column 3; Fama-French 3-factor model Alpha in Column 4; Fama-French 5-factor model Alpha in Column. In Column 6, we redo the analysis from Column 5, but we exclude from the sample years of momentum crashes (Daniel and Moskowitz, 2016). Controls in the multivariate regressions are: the fund's expense ratio; the log of the fund's total net assets (TNA) and it's square; the log fund age; the fund's CRSP turnover ratio; the log number of stocks in the fund's portfolio; lagged fund flows; average manager experience; β_{MOM}, the fund's exposure to the momentum risk factor; and the fund's measured disposition effect. The units of all variables are the same as presented in Table 1. All regressions also control for style-quarter fixed effects. Data frequency is quarterly. Standard errors are clustered by quarter and at the fund level, and reported in brackets. More details on the control variables can be found in Appendix A2.2. Significance: ***99%, **95%, *90%.

	Raw Return	**BM Adj. Return**	**CAPM Alpha**	**FF3 Alpha**	**FF5 Alpha**	**FF5 Alpha NC**
	(1)	(2)	(3)	(4)	(5)	(6)
Extrapolation Beta $(t-1)$	-0.215*	-0.322**	-0.254**	-0.291***	-0.281***	-0.302***
	[0.129]	[0.140]	[0.125]	[0.103]	[0.098]	[0.099]
Expense Ratio $(t-1)$	-0.098	0.119	-0.325	-0.081	0.293	0.275
	[0.213]	[0.321]	[0.208]	[0.190]	[0.201]	[0.203]
Log Fund TNA $(t-1)$	-0.152	-0.682***	-0.293*	-0.235	-0.212	-0.224
	[0.175]	[0.224]	[0.164]	[0.148]	[0.157]	[0.151]
Log Fund TNA^2 $(t-1)$	0.012	0.046***	0.024*	0.019	0.023*	0.025**
	[0.013]	[0.017]	[0.013]	[0.011]	[0.012]	[0.012]
Log Fund Age $(t-1)$	-0.018	-0.007	-0.087	-0.035	-0.067	-0.092
	[0.092]	[0.124]	[0.091]	[0.077]	[0.078]	[0.081]
Fund Turnover $(t-1)$	0.116	0.005	-0.277*	-0.347**	-0.094	-0.063
	[0.157]	[0.169]	[0.149]	[0.146]	[0.137]	[0.148]
Log N Stocks $(t-1)$	0.161**	0.100	-0.02	0.024	-0.047	-0.026
	[0.073]	[0.095]	[0.071]	[0.066]	[0.069]	[0.072]
Flow $(t-1)$	-0.004	-0.003	0.007*	0.014***	0.016***	0.016***
	[0.005]	[0.005]	[0.004]	[0.004]	[0.004]	[0.005]
Avg. Manager Exp. $(t-1)$	0.023	0.031	0.015	-0.004	-0.011	-0.018
	[0.045]	[0.055]	[0.048]	[0.041]	[0.042]	[0.042]
β_{MOM} $(t-1)$	-4.337**	-4.594**	0.492	0.923	0.826	2.219
	[2.093]	[1.892]	[1.914]	[1.642]	[1.764]	[1.861]
Disposition $(t-1)$	0.301	0.624	0.313	0.16	0.208	0.22
	[0.458]	[0.477]	[0.438]	[0.373]	[0.377]	[0.403]
Time × Style FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63125	63125	63108	63108	63108	57183
Adj. R-squared	0.8840	0.1031	0.4355	0.2247	0.2269	0.2384

Table 3. **Summary statistics restricted sample**: This table reports the summary statistics of full sample fund extrapolation metrics and time-series averages of the remaining variables for the restricted sample over the period 1980Q1-2018Q4. Panel A reports statistics for the solo-managed funds and Panel B for the team-managed funds. *Extrapolation Beta* is the extrapolation metric defined in Section 2.2 for the solo managers or teams; *Extrapolation Dummy* is equal to 1 whenever the solo manager's or team's extrapolation beta is positive; *Extrapolation Beta (No Mom)* is the same metric estimated when excluding momentum stocks; *Extrapolation Beta (Weight Change)* is the same metric estimated with portfolio weight changes on the left-hand side of Equation (1); *Extrapolation Beta (Past Year Ret)* is the same metric estimated using past 1 year realized stock returns as the main regressor on the right-hand side of Equation (1); *Fund TNA* is the fund's total net asset value; *Manager Experience* is the experience of managers; *Fund Age* is the the age of the fund since inception; *Expense Ratio* is the fund's total expense ratio; β_{MOM} is the fund's exposure to momentum; *Disposition* is the measured disposition effect. The units of all variables are the same as presented in Table 1.

Panel A: Solo Managers

Mutual Fund Characteristics	Mean	St. Dev.	5th Pct.	Median	95th Pct.
Extrapolation Beta	-0.04	0.46	-0.76	-0.01	0.56
Extrapolation Dummy	0.47	0.50	0	0	1
Extrapolation Beta (No Mom)	0.01	0.64	-0.78	0.01	0.77
Extrapolation Beta (Weight Change)	-0.01	0.44	-0.72	0.00	0.58
Extrapolation Beta (Past Year Ret)	0.00	0.08	-0.13	0.00	0.12
Fund TNA	1017.82	3327.68	12.03	232.15	3893.80
Number of Managers	1.00	0.00	1	1	1
Manager Experience	38.44	25.55	7.00	31.95	88.00
Number of Stocks	89.88	193.98	18	52	215
Fund Age	13.92	13.38	2.27	9.74	43.40
Expense Ratio	1.25	0.46	0.57	1.21	2.09
β_{MOM}	0.03	0.13	-0.15	0.01	0.24
Disposition	-0.03	0.08	-0.18	-0.02	0.09

Panel B: Teams

Mutual Fund Characteristics	Mean	St. Dev.	5th Pct.	Median	95th Pct.
Extrapolation Beta	-0.06	0.43	-0.73	-0.02	0.58
Extrapolation Dummy	0.43	0.50	0	0	1
Extrapolation Beta (No Mom)	-0.04	0.62	-0.81	-0.03	0.91
Extrapolation Beta (Weight Change)	-0.03	0.40	-0.69	0.00	0.60
Extrapolation Beta (Past Year Ret)	-0.01	0.09	-0.15	0.00	0.13
Fund TNA	1200.26	2914.56	20.28	300.62	4890.17
Number of Managers	2.09	0.26	2	2	3
Manager Experience	31.36	18.18	8.00	28.33	70.17
Number of Stocks	103.07	214.42	18	54	267
Fund Age	14.93	13.34	2.29	9.83	43.92
Expense Ratio	1.26	0.44	0.56	1.24	2.03
β_{MOM}	0.03	0.15	-0.18	0.02	0.29
Disposition	-0.03	0.09	-0.20	-0.01	0.10

Table 4. **Extrapolation bias in solo-managed funds versus team-managed funds**: This table compares the extrapolation beta of the team, $\hat{\beta}_j^{TM}$ (Team), with its statistical counterfactual $\hat{\beta}_j^{CF}$ (CF), i.e., the average level of extrapolation of team members when they manage a fund alone. We also report the difference between the counterfactual and the team (CF - Team). Similarly, we compare the characteristics of the team with its statistical counterfactual, i.e. the average characteristics of the team members when they operate individually. *Fund TNA* is the team's (counterfactual's) time-series average total net asset value of the funds they manage; *Manager Experience* is the team's (counterfactual's) time-series average experience; *Number of Stocks* is the team's (counterfactual's) time-series average of the number of stocks held; *Fund Age* is the team's (counterfactual's) time-series average age of the funds since inception that they manage; *Expense Ratio* is the team's (counterfactual's) time-series average total expense ratio; β_{MOM} is the team's (counterfactual) time-series average momentum exposure; *Disposition* is the team's (counterfactual's) time-series average measured disposition effect. 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					
	CF	Team	CF - Team	t-stat	Obs.
Extrapolation beta	-0.03	-0.05	0.02	0.92	308
Fund TNA	1310.48	1215.03	95.44	0.63	308
Manager Experience	30.79	32.34	-1.54	-1.51	308
Number of Stocks	73.49	79.65	-6.16	-2.47	308
Fund Age	16.91	16.22	0.70	0.93	308
Expense Ratio	1.25	1.28	-0.03	-1.62	308
β_{MOM}	0.02	0.02	0.00	-0.27	308
Disposition	-0.02	-0.02	-0.01	-1.44	308
Panel B: Contrarian Teams					
	CF	Team	CF - Team	t-stat	Obs.
Extrapolation beta	-0.24	-0.15	-0.09	-2.68	154
Fund TNA	1406.12	1390.54	15.58	0.08	154
Manager Experience	30.21	33.03	-2.83	-1.96	154
Number of Stocks	70.11	74.45	-4.34	-1.70	154
Fund Age	16.36	15.49	0.87	0.87	154
Expense Ratio	1.25	1.31	-0.06	-2.15	154
β_{MOM}	0.01	0.01	0.00	-0.33	154
Disposition	-0.02	-0.01	-0.01	-1.32	154
Panel C: Extrapolative Teams					
	CF	Team	CF - Team	t-stat	Obs.
Extrapolation beta	0.18	0.04	0.14	3.97	154
Fund TNA	1214.84	1039.53	175.30	0.74	154
Manager Experience	31.38	31.64	-0.26	-0.18	154
Number of Stocks	76.88	84.85	-7.97	-1.86	154
Fund Age	17.47	16.94	0.53	0.47	154
Expense Ratio	1.25	1.25	0.00	-0.08	154
β_{MOM}	0.03	0.03	0.00	-0.33	154
Disposition	-0.03	-0.02	0.00	-0.70	154

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 experience of the team members, the time-series average log of fund age of the team-managed portfolio, the time-series average of the exposure to momentum of the team, the time-series average of the disposition effect of the team, and style fixed effects, and are included as reported. Column (1) to (4) perform the analysis using OLS. Column (1) and (2) estimate a simpler model without any interaction terms. 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 Column (5) to (8) the analysis is performed based on an IV methodology in the spirit of Jegadeesh et al. (2019), that is described in detail in Section 4.3. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	OLS				**IV**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\beta}_j^{CF}$	0.4302***	0.3841***	0.5851***	0.5006***	0.7506***	0.7089***	1.0430***	0.9860***
	[0.0635]	[0.0648]	[0.0969]	[0.1015]	[0.1379]	[0.1385]	[0.2323]	[0.2350]
$\hat{\beta}_j^{CF} \times D_j^E$			-0.4675***	-0.4093**			-0.6819**	-0.6196**
			[0.1627]	[0.1667]			[0.3047]	[0.3092]
D_j^E			0.0285	0.0495			0.0349	0.0156
			[0.0565]	[0.0580]			[0.1601]	[0.1599]
Style fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	308	308	308	308	308	308	308	308
Adj. R-squared	0.1277	0.1515	0.1454	0.1645	0.0591	0.0985	0.0185	0.0644
Hypothesis testing:								
p-value $H_0 : \delta_0 = 1$	0.000	0.000	0.000	0.000	0.070	0.036	0.853	0.953
p-value $H_0 : \delta_0 + \delta_1 = 0$			0.369	0.492			0.205	0.195
p-value $H_0 : \delta_0 + \delta_1 = 1$			0.000	0.000			0.025	0.025

Table 6. **Extrapolation as a bias - additional performance metrics**: This table shows robustness to the analysis in Table 2 on the linkage between extrapolation and future fund performance. In Column 1, the dependent variable is fund flows, defined as the fund's net dollar flows over the following year scaled by the prior period TNA. Controls are the same as in Table 2, in addition to lagged CAPM alpha over the previous year (an explanation of this additional control is in Appendix A2.2). In Column 2, we perform a similar regression but with the fund's ascending rank of fund flows by Morningstar style over the following year (i.e. lowest ranked fund has an value of 1). In Column 3 and 4, the dependent variable is an indicator variable that equals one if the fund ranks in the top 10% or 5% of funds of its Morningstar style category over the following year (based on raw fund returns). Standard errors are clustered by quarter and at the fund level, and reported in brackets. Significance: ***99%, **95%, *90%.

	Flow	**Flow Rank**	**Top 10% Fund**	**Top 5% Fund**
	(1)	(2)	(3)	(4)
Extrapolation Beta $(t-1)$	-3.461**	-3.685**	-0.020***	-0.014***
	[1.377]	[1.737]	[0.006]	[0.004]
Expense Ratio $(t-1)$	-2.330*	-11.551***	0.023***	0.022***
	[1.324]	[2.476]	[0.008]	[0.006]
Log Fund TNA $(t-1)$	-9.268***	-18.941***	-0.021***	-0.013**
	[1.336]	[2.118]	[0.007]	[0.006]
Log Fund TNA^2 $(t-1)$	0.555***	1.367***	0.002***	0.001**
	[0.095]	[0.171]	[0.001]	[0.000]
Log Fund Age $(t-1)$	-3.333***	-4.340***	-0.001	-0.001
	[0.707]	[1.611]	[0.004]	[0.003]
Fund Turnover $(t-1)$	-1.155	-8.607***	0.011*	0.007*
	[0.798]	[1.524]	[0.006]	[0.004]
Log N Stocks $(t-1)$	0.641	3.077***	-0.029***	-0.021***
	[0.480]	[1.080]	[0.003]	[0.002]
Avg. Manager Exp. $(t-1)$	0.312	0.985	0.002	0.001
	[0.331]	[0.649]	[0.002]	[0.001]
β_{MOM} $(t-1)$	-8.921	2.493	-0.156***	-0.109***
	[5.951]	[9.549]	[0.045]	[0.028]
Disposition $(t-1)$	-5.065**	-20.240***	0.027*	0.024**
	[2.290]	[4.148]	[0.014]	[0.010]
CAPM Alpha $(t-1)$	1.685***	3.021***		
	[0.120]	[0.191]		
Flow $(t-1)$			0.000	0.000
			[0.000]	[0.000]
Time × Style FE	Yes	Yes	Yes	Yes
Observations	52607	52607	63125	63125
Adj. R-squared	0.0783	0.4397	0.0114	0.0119

Table 7. **Extrapolation and investor preferences**: In Panel A, we regress future annual fund return volatility (i.e., the volatility of a fund's monthly benchmark-adjusted returns and FF5 Alpha) on funds' lagged extrapolative behavior. In Panel B, we estimate a linear probability model in which the dependent variable is a dummy that is equal to one if a fund ranks among the top 10 or top 5 funds of its Morningstar style in a given year, and the main independent variable is the lagged fund's extrapolative behavior. In Panel C, we regress fund investment performance (benchmark-adjusted returns and FF5 alpha) over the following year on lagged funds' extrapolative behavior, but restrict the analysis to the years in which the CRSP weighted market index is negative at the time in which the dependent variable is measured. In Panel D, we repeat the analysis of Panel C, but now restrict the sample to years of NBER recessions, defined as years in which 6 or more months are part of a NBER recession. Regressions also control for style-quarter fixed effects and include the fund-level controls of Table 2. Standard errors are clustered by quarter and at the fund level, and reported in brackets. Significance: ***99%, **95%, *90%.

	Panel A: Managerial Risk		Panel B: Extreme Payoffs	
	BM Adj. Vol	**FF5 Alpha Vol**	**Top 10 Fund**	**Top 5 Fund**
	(1)	(2)	(1)	(2)
Extrapolation	-0.038	0.001	-0.015***	-0.010***
Beta $(t-1)$	[0.047]	[0.047]	[0.004]	[0.003]
Controls	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes
Observations	51553	54299	63125	63125
Adj. R-squared	0.0855	0.0514	0.0372	0.0227

	Panel C: Downside Performance		Panel D: NBER Recessions	
	BM Adj. Return	**FF5 Alpha**	**BM Adj. Return**	**FF5 Alpha**
	(1)	(2)	(3)	(4)
Extrapolation	-0.012	-0.148	-0.415	-0.372
Beta $(t-1)$	[0.326]	[0.186]	[0.500]	[0.380]
Controls	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes
Observations	11141	11141	5094	5094
Adj. R-squared	0.1231	0.2572	0.1677	0.3116

Table 8. **Robustness – alternative extrapolation metrics**: This table shows robustness to three alternative ways to measure extrapolation: i) exclusion of momentum stocks; (ii) using a weight change measure to identify portfolio changes; and (iii) using realized past stock returns. In Panel A, we repeat our analysis from Table 2 on the linkage between extrapolation and fund performance using these alternative extrapolation metrices. In Panel B, we use the alternative extrapolation metrics to repeat the analysis in Table 5 on how extrapolative behavior is transmitted from individual team members to teams. For the *No Momentum* extrapolation metric, 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 stock returns. For the *Weight Change* extrapolation metric, we use active weight changes as specfcied in Equation (7) as the main dependent variable in Equation (1). For the *Past 1 Year Return* extrapolation metric we use realized past year returns for the main independent variable $r_{s,t-4\to t}$ in Equation (1). More details on the construction of the alternative extrapolation metrics are in Section 5.1.1. Standard errors are in brackets, and in Panel A, we cluster standard errors by quarter and at the fund level. Significance: ***99%, **95%, *90%.

Panel A: Fund Performance

	No Momentum		Weight Changes		Past 1 Year Return	
	BM Adj. Return	**FF5 Alpha**	**BM Adj. Return**	**FF5 Alpha**	**BM Adj. Return**	**FF5 Alpha**
	(1)	(2)	(3)	(4)	(5)	(6)
Extrapolation Beta ($t-1$)	-0.257**	-0.179**	-0.253*	-0.318***	-1.090*	-0.863**
	[0.101]	[0.071]	[0.150]	[0.103]	[0.614]	[0.418]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63215	63198	63663	63646	63125	63108
Adj. R-squared	0.1032	0.2277	0.1026	0.2298	0.1029	0.2266

Panel B: Transmission of extrapolation from solo to team-managed funds						
	No Momentum		Weight Change		Past 1 Year Return	
	OLS	**IV**	**OLS**	**IV**	**OLS**	**IV**
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.6275***	0.9046***	0.5639***	0.6936***	0.3939***	0.9168**
	[0.1498]	[0.2009]	[0.1431]	[0.1789]	[0.1350]	[0.3848]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.4710***	-0.6584***	-0.4087**	-0.4330**	-0.3768*	-0.9070***
	[0.1798]	[0.2499]	[0.1855]	[0.1737]	[0.2248]	[0.2964]
D_j^E	0.0274	0.1502	0.0215	-0.0621	0.0126	0.0127
	[0.0861]	[0.2281]	[0.0597]	[0.1685]	[0.0136]	[0.0553]
Style fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Team controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	308	308	308	308	308	308
Adj. R-squared	0.1319	0.0734	0.1064	0.0512	0.0862	0.0396
Hypothesis testing:						
p-value $H_0 : \delta_0 = 1$	0.014	0.635	0.003	0.087	0.000	0.829
p-value $H_0 : \delta_0 + \delta_1 = 0$	0.115	0.254	0.193	0.050	0.925	0.973
p-value $H_0 : \delta_0 + \delta_1 = 1$	0.000	0.001	0.000	0.000	0.000	0.001

Table 9. **Robustness – experience, style, workload, and fund families**: In Panel A, we show summary statistics for the experience, ordering of team and single management experience, style, workload, and fund family measures as specified in Sections 5.2.1 – 5.2.5. In Panels B to G, we estimate a double interaction regression of the form: $\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^E \times R_j + (...) + \delta_7 C_j + \epsilon_j$, where R_j represents alternative interaction terms for our robustness tests. The main coefficient of interest in these regressions is δ_1, which measures if the attenuation of extrapolation bias is stronger or weaker for teams whose members load more on the R_j characteristic. In Panel B, $R_j = \Delta Experience_j$, the difference in experience of managers when they manage in team j versus when they manage alone. In Panel C, $R_j = D_j^{ES}$, a dummy variable that is equal to one if all members of team j started off as solo managers. In Panel D, $R_j = D_j^{ET}$, a dummy variable that is equal to one if all members of team j started off in team-managed funds. In Panel E, $R_j = D_j^{SM}$, a dummy variable that is equal to one if a style migration occurs, i.e., the style classification of team j does not match the prevailing style classification of the funds managed individually by the members of the team. In Panel F, $R_j = \Delta Workload_j$, i.e., the difference in workload faced by the managers of team j when managing as part of that team, and the workload these same managers face when managing a fund alone (further details are in Section 5.2.3). In Panel G, $R_j = D_j^{FS}$, a dummy variable that is equal to one if the mutual fund family of team j does not match with the mutual fund family under which managers operate when solo (i.e. a family switch). In addition to the reported additional regressors, team-level controls include the time-series average log TNA, the time-series average log experience of the team members, the time-series average log of fund age, the time-series average of the exposure to momentum, the time-series average of the disposition effect, and style fixed effects. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

Panel A: Summary Statistics

All Teams	Obs.	Mean	St. Dev.	Min	Max
$\Delta Experience_j$	308	1.78	17.81	-46	88
D_j^{ES}	308	0.21	0.41	0	1
D_j^{ET}	308	0.18	0.38	0	1
D_j^{SM}	308	0.24	0.43	0	1
$\Delta Workload_j$	308	-36.61	115.86	-409	481
D_j^{FS}	308	0.40	0.49	0	1
Contrarian Teams	Obs.	Mean	St. Dev.	Min	Max
$\Delta Experience_j$	154	3.08	17.77	-45	88
D_j^{ES}	154	0.21	0.41	0	1
D_j^{ET}	154	0.16	0.37	0	1
D_j^{SM}	154	0.23	0.42	0	1
$\Delta Workload_j$	154	-31.94	113.83	-408	414
D_j^{FS}	154	0.43	0.50	0	1
Extrapolative Teams	Obs.	Mean	St. Dev.	Min	Max
$\Delta Experience_j$	154	0.48	17.81	-46	64
D_j^{ES}	154	0.21	0.41	0	1
D_j^{ET}	154	0.19	0.39	0	1
D_j^{SM}	154	0.24	0.43	0	1
$\Delta Workload_j$	154	-41.29	118.05	-409	481
D_j^{FS}	154	0.36	0.48	0	1

	Panel B: Experience		Panel C: Enter Single		Panel D: Enter Team	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.5915***	0.5094***	0.6379***	0.5660***	0.5246***	0.4424***
	[0.0949]	[0.0994]	[0.1052]	[0.1093]	[0.1040]	[0.1076]
$\hat{\beta}_j^{CF} \times D_j^E \times R_j$	-0.0058	-0.0059	0.9002	0.7776	-0.0816	-0.147
	[0.0081]	[0.0084]	[0.5463]	[0.5530]	[0.4355]	[0.4422]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.3541**	-0.2973*	-0.5626***	-0.4962***	-0.4881***	-0.4270**
	[0.1671]	[0.1699]	[0.1726]	[0.1764]	[0.1798]	[0.1859]
$\hat{\beta}_j^{CF} \times R_j$	-0.0090**	-0.0092**	-0.4091	-0.4783*	0.4579	0.4623
	[0.0038]	[0.0039]	[0.2761]	[0.2779]	[0.2852]	[0.2851]
$D_j^E \times R_j$	-0.0009	-0.0004	-0.0677	0.001	-0.1151	-0.0712
	[0.0031]	[0.0031]	[0.1451]	[0.1465]	[0.1562]	[0.1583]
D_j^E	0.0179	0.04	0.0382	0.0492	0.0436	0.0572
	[0.0560]	[0.0574]	[0.0635]	[0.0646]	[0.0614]	[0.0627]
R_j	-0.0001	-0.0005	-0.0189	-0.0591	0.069	0.054
	[0.0023]	[0.0024]	[0.0959]	[0.0972]	[0.1103]	[0.1125]
Style fixed effects	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes
Observations	308	308	308	308	308	308
Adj. R-squared	0.1811	0.2004	0.1452	0.1633	0.1463	0.1657

	Panel E: Style Migrations		Panel F: Workload		Panel G: Family Switch	
	(7)	(8)	(9)	(10)	(11)	(12)
$\hat{\beta}_j^{CF}$	0.6496***	0.5688***	0.5634***	0.4819***	0.5497***	0.4788***
	[0.1058]	[0.1098]	[0.0980]	[0.1029]	[0.1422]	[0.1445]
$\hat{\beta}_j^{CF} \times D_j^E \times R_j$	0.2649	0.2931	0.001	0.0014	0.2843	0.2501
	[0.4328]	[0.4352]	[0.0024]	[0.0025]	[0.3529]	[0.3591]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.4993***	-0.4377**	-0.4990***	-0.4123**	-0.5206**	-0.4663**
	[0.1772]	[0.1805]	[0.1697]	[0.1777]	[0.2088]	[0.2152]
$\hat{\beta}_j^{CF} \times R_j$	-0.4560*	-0.4729*	-0.003	-0.0025	0.0647	0.0251
	[0.2565]	[0.2564]	[0.0019]	[0.0019]	[0.1943]	[0.1986]
$D_j^E \times R_j$	-0.0515	-0.0504	0.0007	0.0004	0.0067	-0.0051
	[0.1373]	[0.1372]	[0.0005]	[0.0006]	[0.1169]	[0.1178]
D_j^E	0.0498	0.0694	0.0463	0.0536	0.0246	0.0547
	[0.0635]	[0.0646]	[0.0607]	[0.0634]	[0.0742]	[0.0752]
R_j	0.0694	0.0405	-0.0004	-0.0003	0.0378	0.0637
	[0.0934]	[0.0958]	[0.0004]	[0.0004]	[0.0802]	[0.0806]
Style fixed effects	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes
Observations	308	308	308	308	308	308
Adj. R-squared	0.1577	0.1739	0.1463	0.161	0.1459	0.1646

Table 10. **Robustness – compensation structures**: This table compares the compensation structure of the fund managers in our sample when they manage individually with the compensation structures these managers face when managing as part of a team. The contractual incentives are measured using hand-collected data from the statement of additional information (SAI) that mutual funds file with the SEC. We summarize these incentives using four dummy variables tracking whether 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. For more details on the data, see Section 5.2.2. We report the average extrapolation beta and compensation of the teams, their counterfactual, and 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	-0.056	-0.079	0.036	0.035	0.830	0.860	0.189	0.179	0.682	0.576
s.e.	0.040	0.054	0.014	0.015	0.029	0.029	0.031	0.032	0.032	0.038
CF - Team		0.023		0.001		-0.031		0.010		0.105
t-stat		0.391		0.127		-1.640		0.433		3.046
Obs.		125		125		124		124		124

Panel B: Contrarian Teams

	Extrapolation beta		**Fixed pay**		**Performance pay**		**AUM pay**		**Ownership**	
	CF	Team	CF	Team	CF	Team	CF	Team	CF	Team
Mean	-0.249	-0.156	0.026	0.028	0.829	0.866	0.203	0.165	0.723	0.610
s.e.	0.051	0.058	0.015	0.017	0.037	0.035	0.042	0.039	0.040	0.050
CF - Team		-0.093		-0.002		-0.037		0.037		0.113
t-stat		-1.548		-0.251		-1.258		1.027		2.497
Obs.		72		71		71		71		69

Panel C: Extrapolative Teams

	Extrapolation beta		**Fixed pay**		**Performance pay**		**AUM pay**		**Ownership**	
	CF	Team	CF	Team	CF	Team	CF	Team	CF	Team
Mean	0.205	0.024	0.051	0.045	0.823	0.846	0.178	0.206	0.609	0.531
s.e.	0.042	0.098	0.028	0.028	0.049	0.049	0.049	0.056	0.053	0.061
CF - Team		0.181		0.006		-0.023		-0.028		0.078
t-stat		1.681		0.292		-1.164		-1.175		1.472
Obs.		53		52		51		51		48

Table 11. **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 experience of the team members, the time-series average log of fund age of the team-managed portfolio, the time-series average of the disposition effect of the team, 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)
$\hat{\beta}_j^{CF}$	0.5851***	0.5076***	0.4974***
	[0.0969]	[0.0981]	[0.1014]
$\hat{\beta}_j^{CF} \times D_j^{AE}$	-0.5854***	-0.5095***	-0.5327***
	[0.1850]	[0.1840]	[0.1882]
$\hat{\beta}_j^{CF} \times D_j^{M}$	-0.3135	-0.271	-0.3022
	[0.3018]	[0.2995]	[0.3062]
D_j^{AE}	0.0937	0.1062	0.1309*
	[0.0719]	[0.0711]	[0.0740]
D_j^{M}	-0.0263	-0.0146	-0.0124
	[0.0677]	[0.0669]	[0.0689]
Team controls	No	Yes	Yes
Style FE	No	No	Yes
Observations	308	308	308
Adj. R-squared	0.1459	0.175	0.1677
Hypothesis testing:			
p-value $H_0 : \delta_1 = \delta_2$	0.4055	0.459	0.4857

Table A1. **Extended correlation matrix**: This table shows the correlations of the variables that we use for our analyses that are present in the summary statistics Table 1.

Extrapolation Beta	1																						
Extrapolation Dummy	0.51	1																					
Extrapolation Beta (No Mom)	0.77	0.41	1																				
Extrapolation Beta (Weight Change)	0.87	0.47	0.66	1																			
Extrapolation Beta (Past Year Ret)	0.81	0.40	0.51	0.77	1																		
Fund TNA	-0.03	-0.06	-0.03	-0.04	-0.04	1																	
Team Managed	0.00	0.04	-0.02	-0.01	0.01	-0.04	1																
Number of Managers	0.00	0.00	-0.02	0.00	-0.01	0.06	0.56	1															
Manager Experience	-0.02	-0.05	-0.03	-0.03	-0.01	0.09	-0.02	0.00	1														
Number of Stocks	0.04	0.01	0.04	0.04	0.02	0.10	0.07	0.22	-0.07	1													
Fund Age	-0.03	-0.04	-0.02	-0.04	-0.03	0.34	-0.12	-0.06	0.04	-0.07	1												
Expense Ratio	0.03	0.00	0.04	0.02	0.04	-0.28	-0.08	-0.12	-0.03	-0.27	-0.14	1											
Fund Turnover	0.08	0.07	0.09	0.06	0.05	-0.14	-0.09	-0.04	-0.13	-0.05	-0.10	0.22	1										
β_{MOM}	0.19	0.24	0.20	0.15	0.11	-0.06	0.00	-0.02	-0.05	0.03	-0.05	0.05	0.40	1									
Disposition	-0.18	-0.21	-0.24	-0.13	-0.11	0.06	0.11	0.12	0.07	0.02	-0.02	-0.08	-0.37	-0.61	1								
Fund Return	-0.04	-0.03	-0.05	-0.04	0.01	0.15	-0.01	0.00	0.10	0.02	0.14	-0.01	-0.04	0.00	0.05	1							
Benchmark Adj. Return	-0.04	-0.08	-0.03	-0.06	-0.01	0.11	0.00	-0.01	0.07	-0.02	0.02	0.09	0.05	0.14	-0.02	0.47	1						
CAPM Alpha	-0.03	-0.03	-0.03	-0.04	0.01	0.11	0.03	-0.01	0.04	-0.08	0.08	0.02	-0.01	0.13	-0.02	0.41	0.56	1					
FF3 Alpha	-0.03	0.00	-0.03	-0.05	0.00	0.12	0.00	-0.01	0.08	-0.03	0.08	-0.03	-0.04	0.13	-0.03	0.40	0.52	0.74	1				
FF5 Alpha	-0.01	0.04	0.00	-0.03	0.02	0.10	-0.01	-0.01	0.06	-0.05	0.07	0.05	0.08	0.20	-0.11	0.41	0.53	0.62	0.90	1			
Flow	0.03	0.04	0.01	-0.01	0.02	0.02	0.07	-0.02	0.01	0.01	-0.16	-0.01	0.03	0.01	-0.01	0.00	0.04	-0.01	0.02	0.02	1		
Benchmark Adj. Volatility	-0.07	-0.07	-0.02	-0.08	-0.05	-0.08	-0.15	-0.17	-0.02	-0.23	0.00	0.40	0.28	0.15	-0.15	0.17	0.37	0.19	0.12	0.27	-0.05	1	
FF5 Alpha Volatility	-0.07	-0.05	-0.04	-0.09	-0.04	-0.09	-0.15	-0.17	-0.05	-0.26	0.02	0.34	0.30	0.20	-0.18	0.24	0.38	0.24	0.23	0.38	-0.04	0.83	1

Table A2. **The explanatory power of momentum trading and the disposition effect for extrapolative behavior**: This table estimates regressions of our extrapolation metrics on a fund's momentum trading behavior and on a fund's disposition effect. We measure momentum trading as the loading of a fund's return on the momentum factor (Carhart, 1997). We measure the disposition effect following Odean (1998) and Cici (2012) (see Section A2.1 for more details). Following the analyses elsewhere in the paper, we show the results for four distinct measures of extrapolation: i) the main one, that uses all stocks traded by a fund; ii) an alternative one (No Momentum), that excludes momentum stocks; (iii) an alternative metric that uses weight changes as the way to measure funds' trading behavior (Weight Change); and (iv) an alternative metric for extrapolation that relies on past 1-year returns without using the structural estimates of extrapolators' memory in Greenwood and Shleifer (2014) (Past 1-year Return). More details about these metrics are in Section 2.2 and 5.1.1. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	Main Specification	No Momentum	Weight Change	Past 1-year Return
	(1)	(2)	(3)	(4)
β_{MOM}	0.536***	0.563***	0.427***	0.061**
	[0.157]	[0.194]	[0.142]	[0.027]
Disposition	-0.752***	-1.802***	-0.429**	-0.097*
	[0.253]	[0.387]	[0.215]	[0.055]
Constant	-0.057***	-0.073***	-0.044***	-0.006**
	[0.011]	[0.015]	[0.010]	[0.002]
Observations	2057	2057	2057	2057
R-squared	0.0438	0.0625	0.0253	0.0139
Adj. R-squared	0.0428	0.0616	0.0244	0.013

Table A3. **Transmission of extrapolation bias - alternative counterfactuals**: In this table we estimate the transmission of extrapolation bias from solo managers to their respective teams using different measures for the counterfactual $\hat{\beta}_j^{CF}$. Whereas in our main specifications $\hat{\beta}_j^{CF}$ is the simple average of the extrapolative behavior of each individual team member, here we use weighted averages based on each team members' quarters of experience, number of individual funds managed, and size of individual funds managed, all measured at the time of team formation. We estimate the following regression as in Table 5: $\hat{\beta}_j^{TM} = \alpha + \hat{\beta}_j^{CF}(\delta_0 + \delta_1 D_j^E) + \delta_2 D_j^E + \delta_3 C_j + \epsilon_j$. Team controls ($C_j$) are the time-series average log TNA of the team-managed portfolio, the time-series average log experience of the team members, the time-series average log of fund age of the team-managed portfolio, the time-series average of the exposure to momentum of the team, the time-series average of the disposition effect of the team, and style fixed effects, and are included as reported. Column (1), (3) and (5) 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 (2), (4) and (6) 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). Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	Experience		**Number of Funds**		**Size of Funds**	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.3679***	0.4738***	0.3644***	0.5469***	0.3535***	0.5946***
	[0.0655]	[0.1097]	[0.0640]	[0.1034]	[0.0631]	[0.1040]
$\hat{\beta}_j^{CF} \times D_j^E$		-0.4353***		-0.4803***		-0.5356***
		[0.1605]		[0.1578]		[0.1520]
D_j^E		0.1248**		0.0265		0.0122
		[0.0571]		[0.0578]		[0.0567]
Style fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Team controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	308	308	308	308	308	308
Adj. R-squared	0.1423	0.1699	0.1443	0.1656	0.1418	0.1713
Hypothesis testing:						
p-value $H_0 : \delta_0 = 1$	0.000	0.000	0.000	0.000	0.000	0.000
p-value $H_0 : \delta_0 + \delta_1 = 0$		0.742		0.451		0.597
p-value $H_0 : \delta_0 + \delta_1 = 1$		0.000		0.000		0.000

Table A4. **Measuring extrapolation – alternative assumptions**: This table shows robustness to some of the assumptions that we make to measure extrapolation in our main analysis. Specifically, in the analysis labeled *Horizon Universe* we re-estimate managers' extrapolative behavior using an investment universe that only incorporates the stocks that the investor start to hold within the next year, but we keep all the stocks the investor has held in the previous 11 quarters as in Koijen and Yogo (2019). For the analysis labeled *No Universe*, we estimate extrapolation based only on actual trades. This approach effectively takes the definition of the appropriate investment universe off the empiricists' hands. Finally, in the analysis labeled *Separate Inflows/Outflows*, we relax an implicit assumption in Section 2.2, namely, that flow-induced trading is symmetric for inflows and outflows. To better incorporate the evidence in Lou (2012), we model the impact of inflows and outflows on a fund's trading behavior by incorporating on the right hand side of Equation (1) a dummy variable that indicates whether the fund received inflows in quarter $t+1$, $D_{j,t+1}^{inflows}$, interacted with the stock-level variables defined in $F_{s,j,t}$ (Section 2.2, Equation (1)). In Panel A, we repeat our analysis from Table 2 on the linkage between extrapolation and fund performance using these alternative extrapolation metrices. In Panel B, we use the alternative extrapolation metrics to repeat the OLS regressions of Table 5 on how extrapolative behavior is transmitted from individual team members to teams. Standard errors are in brackets, and in Panel A, we cluster standard errors by quarter and at the fund level. Significance: ***99%, **95%, *90%.

Panel A: Fund Performance

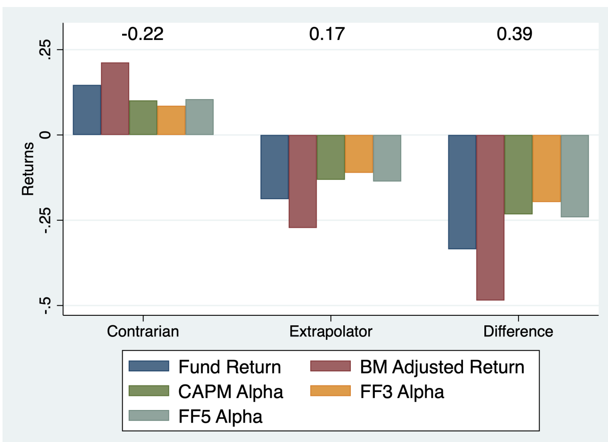
	Horizon Universe		No Universe		Separate Inflows/Outflows	
	BM Adj. Return	**FF5 Alpha**	**BM Adj. Return**	**FF5 Alpha**	**BM Adj. Return**	**FF5 Alpha**
	(1)	(2)	(3)	(4)	(5)	(6)
Extrapolation Beta $(t-1)$	-0.271**	-0.196**	-0.229***	-0.168***	-0.315**	-0.294***
	[0.106]	[0.077]	[0.082]	[0.055]	[0.138]	[0.097]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63663	63646	63663	63646	63663	63646
Adj. R-squared	0.1029	0.2297	0.1031	0.2299	0.1031	0.2298

Panel B: Transmission of extrapolation from solo to team-managed funds						
	Horizon Universe		No Universe		Separate Inflows/Outflows	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.7996***	0.7120***	0.4774***	0.4546***	0.5595***	0.4568***
	[0.1300]	[0.1344]	[0.1002]	[0.1017]	[0.0968]	[0.1011]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.7545***	-0.6864***	-0.3129**	-0.3064**	-0.4744***	-0.3935**
	[0.1750]	[0.1790]	[0.1532]	[0.1553]	[0.1623]	[0.1664]
D_j^E	0.0664	0.0834	0.0341	0.04	0.0835	0.1126*
	[0.0810]	[0.0830]	[0.0895]	[0.2059]	[0.0569]	[0.0581]
Style fixed effects	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes
Observations	308	308	308	308	303	303
Adj. R-squared	0.161	0.1796	0.1117	0.1262	0.1598	0.1802
Hypothesis testing:						
p-value $H_0 : \delta_0 = 1$	0.124	0.033	0.000	0.000	0.000	0.000
p-value $H_0 : \delta_0 + \delta_1 = 0$	0.700	0.833	0.157	0.000	0.5138	0.632
p-value $H_0 : \delta_0 + \delta_1 = 1$	0.000	0.000	0.000	0.000	0.000	0.000

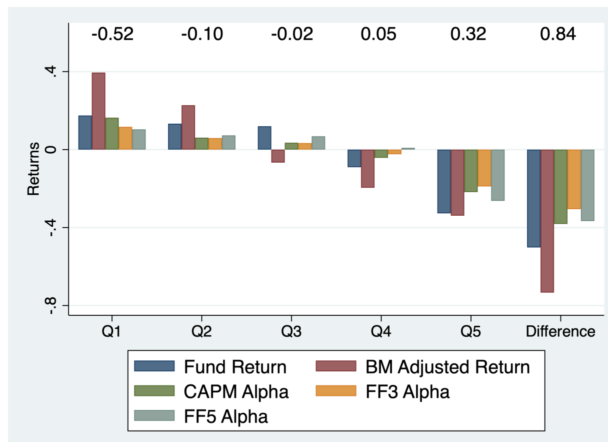
Table A5. **Benchmark-adjusted alphas**: This table shows similar regressions as Table 2 where we instead regress benchmark-adjusted returns over the following year, defined as the return of the fund minus the return of it's primary prospectus benchmark: $r_{i,t}^{adj} = r_{i,t} - r_t^{BM}$, on the factor returns of a CAPM, FF3 and FF5 model. We find that extrapolation is negatively associated to benchmark-adjusted alphas, and the results are similar to those of Table 2 because the risk factors in these models span a large portion of the benchmark returns. Standard errors are in brackets and clustered by quarter and at the fund level. Significance: ***99%, **95%, *90%.

	CAPM	**FF3**	**FF5**
	(1)	(2)	(3)
Extrapolation Beta $(t-1)$	-0.286**	-0.314***	-0.302***
	[0.124]	[0.106]	[0.098]
Expense Ratio $(t-1)$	-0.283	-0.081	0.274
	[0.217]	[0.193]	[0.205]
Log Fund TNA $(t-1)$	-0.301*	-0.257*	-0.244
	[0.172]	[0.152]	[0.161]
Log Fund TNA^2 $(t-1)$	0.025*	0.019	0.024*
	[0.013]	[0.012]	[0.013]
Log Fund Age $(t-1)$	-0.076	0.014	-0.062
	[0.092]	[0.082]	[0.084]
Fund Turnover $(t-1)$	-0.321**	-0.399***	-0.195
	[0.151]	[0.144]	[0.137]
Log N Stocks $(t-1)$	-0.068	0.062	-0.009
	[0.077]	[0.070]	[0.071]
Flow $(t-1)$	0.008**	0.014***	0.015***
	[0.004]	[0.004]	[0.004]
Avg. Manager Exp. $(t-1)$	-0.006	-0.009	-0.002
	[0.048]	[0.041]	[0.042]
β_{MOM} $(t-1)$	1.741	0.855	0.311
	[1.754]	[1.582]	[1.665]
Disposition $(t-1)$	0.373	0.12	0.206
	[0.435]	[0.380]	[0.381]
Time × Style FE	Yes	Yes	Yes
Observations	59797	59797	59797
Adj. R-squared	0.1546	0.1724	0.1679

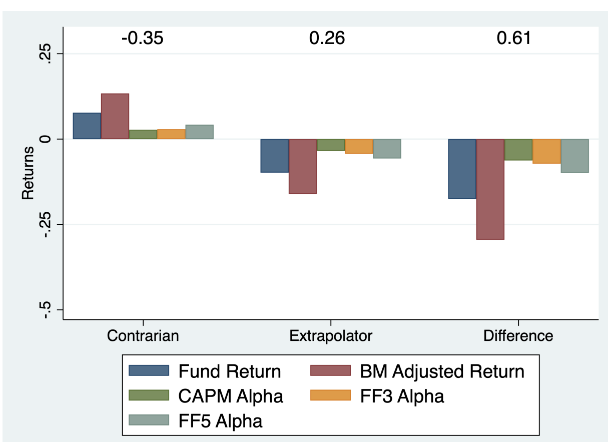
Figure 1. **Extrapolation and performance - graphical evidence**: In Panel A we estimate funds' extrapolative behavior over the full sample, and then sort funds into either two groups (A1, extrapolators and contrarians, left panel), or five groups based on quintile breakpoints (A2, right panel). Each subpanel reports style-adjusted average yearly gross fund performance in each of the aforementioned groups, as well as the difference between the top and the bottom group. Performance is measured in a variety of ways (from left to right in each group): (i) raw returns (blue); (ii) benchmark-adjusted returns (red); (iii) CAPM alpha (green); (iv) Fama-French 3-factor alpha (FF3, Fama and French, 1993) (orange); (iv) Fama-French 5-factor alpha (FF5, Fama and French, 2015) (grey). Above each group, we report the respective average extrapolation beta. Panel B repeats the analysis in a predictive setting, where we ask whether a recursively estimated extrapolation metric can predict future fund returns. Further details on the construction of the performance metrics are in Appendix A2.2.



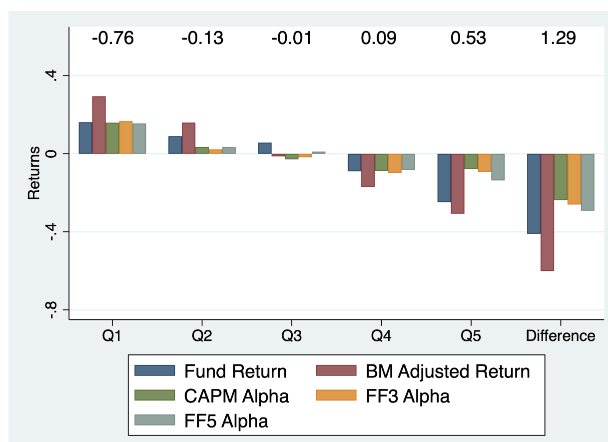
(a) Panel A1: Extrapolators and contrarians (full sample)



(b) Panel A2: Quintile sorts (full sample)

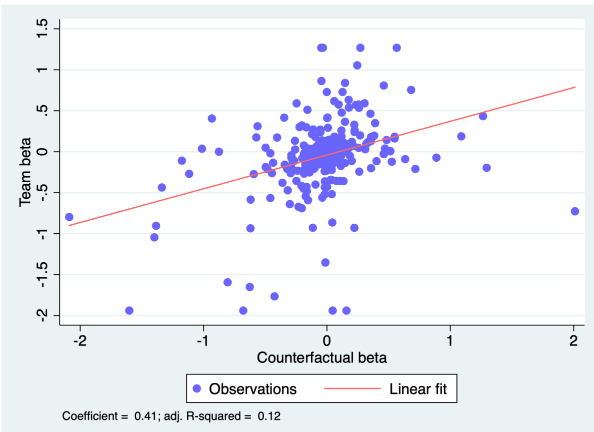


(c) Panel B1: Extrapolators and contrarians (recursive)

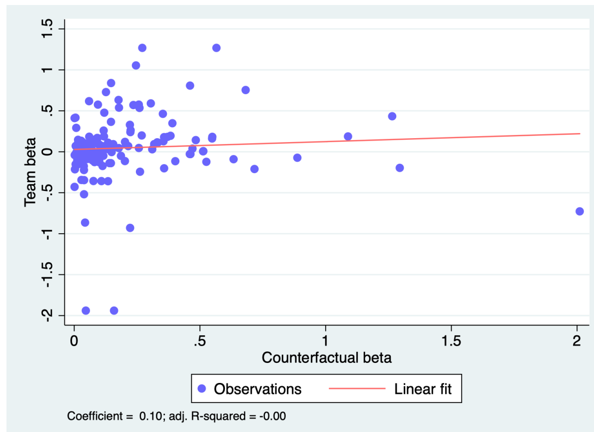


(d) Panel B2: Quintile sorts (recursive)

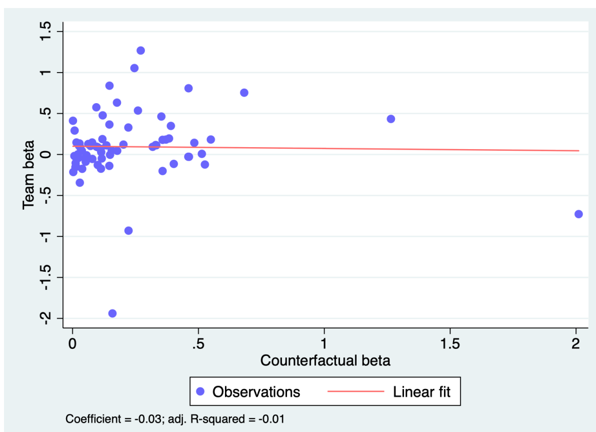
Figure 2. **Extrapolation in teams for non-learner managers**: This figure plots the team-level extrapolation metric ($\hat{\beta}_j^{TM}$) against the average extrapolation that team members display when managing a single-managed fund ($\hat{\beta}_j^{CF}$). The plot is presented for all teams (Panel A), all extrapolative teams (Panel B), and the extrapolative teams that consist of "non-learner" managers (Panel C). A team is defined as extrapolative if its members extrapolate on average (i.e., $\hat{\beta}_j^{CF} > 0$). The non-learners sample is defined as the set of managers who extrapolate in both the first half and the second half of their tenure as individual fund manager.



(a) Panel A: All Teams

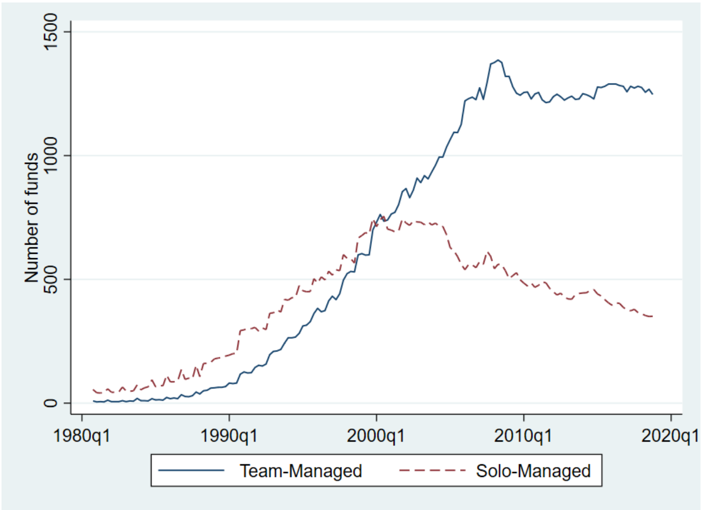


(b) Panel B: Extrapolators

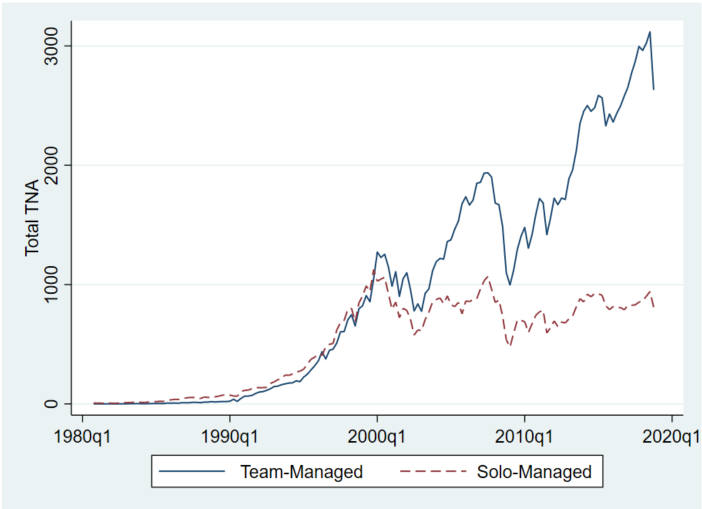


(c) Panel C: Extrapolative Non-Learners

Figure A1. **The growth of team-based asset management**: This figure shows the time-series of the total number of 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 in the Morningstar and CRSP merged database.



(a) Number of funds by type



(b) TNA in $billion by fund type

Internet Appendix

Appendix IA1 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 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 1,000 random draws and re-estimating our main results, we find that our conclusions remain unchanged.

IA1.1 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:

$$r_{s,t} = \gamma_s r_{m,t} + \varepsilon_{s,t}, \tag{IA1}$$

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 $\mathcal{N}(0, 0.15)$. For the single factor return, 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:

$$trade_{s,i,t+1} = \beta_i^{SM} r_{s,t-4\to t} + e_{s,i,t+1}, \tag{IA2}$$

where β_i^{SM} equals the true extrapolation beta of manager $i = 1, \ldots, N$, $r_{s,t-4\rightarrow t}$ the annual past return of stock s, and $e_{s,i,t+1}$ 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 $\sigma_\beta = 0.5$. 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. To keep transactions with a similar distribution as in our empirical setting, we then simulate the noise term as a standard normal distribution with mean $\mu_e = 0$ and standard deviation $\sigma_e = 1$. As these sets of parameters do not result in a large measurement error bias, we also use $\sigma_e = 2$ and $\sigma_\beta = 0.2$ to generate more measurement error for illustrative purposes.

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:

$$trade_{s,j,t+1} = \beta_j^{TM} r_{s,t-4\rightarrow t} + e_{s,j,t+1}, \tag{IA3}$$

where

$$\beta_j^{TM} = \alpha + \kappa_0 \bar{\beta}_j^{SM} + \upsilon_j, \tag{IA4}$$

$$\bar{\beta}_j^{SM} \equiv \frac{\sum_{i=1|j}^{2} \beta_{i|j}^{SM}}{2}, \tag{IA5}$$

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 (IA4) 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:

$$\beta_j^{TM} = \alpha + \kappa_0 \bar{\beta}_j^{SM} + \kappa_1 \bar{\beta}_j^{SM} \times D_j + \kappa_2 D_j + \upsilon_j, \tag{IA6}$$

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 ($\kappa_0 + \kappa_1$).

IA1.2 OLS Estimation

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

$$\hat{\beta}_j^{TM} = c + \delta_0 \hat{\bar{\beta}}_j^{SM} + \epsilon_j. \tag{IA7}$$

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

$$\hat{\beta}_j^{TM} = c + \delta_0 \hat{\bar{\beta}}_j^{SM} + \delta_1 \hat{\bar{\beta}}_j^{SM} \times \hat{D}_j + \delta_2 \hat{D}_j + \epsilon_j. \tag{IA8}$$

It is important to note that when estimating the regression above, we condition $\hat{D}_j$ on $\hat{\bar{\beta}}_j^{SM}$, 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).

IA1.3 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 (IA2), 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 extrapolation betas, $\hat{\bar{\beta}}_j^{SM,1}$ and $\hat{\bar{\beta}}_j^{SM,2}$. 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:

$$\begin{aligned} &\text{1st stage: } \bar{\hat{\beta}}_j^{SM,1} = \alpha + \lambda_0 \bar{\hat{\beta}}_j^{SM,2} + u_j, \\ &\text{2nd stage: } \hat{\beta}_j^{TM} = c + \delta_0 \bar{\hat{\beta}}_j^{SM,1,pred} + \epsilon_j, \end{aligned} \tag{IA9}$$

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 (IA9) 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:

$$\begin{aligned} &\text{1st stage: } \begin{cases} \bar{\hat{\beta}}_j^{SM,1} & = \alpha_1 + \lambda_{1,0}\bar{\hat{\beta}}_j^{SM,2} + \lambda_{1,1}\bar{\hat{\beta}}_j^{SM,2} \times \hat{D}_j^2 + \lambda_{1,2}\hat{D}_j^2 + u_{1,j} \\ \bar{\hat{\beta}}_j^{SM,1} \times \hat{D}_j^1 & = \alpha_2 + \lambda_{2,0}\bar{\hat{\beta}}_j^{SM,2} + \lambda_{2,1}\bar{\hat{\beta}}_j^{SM,2} \times \hat{D}_j^2 + \lambda_{2,2}\hat{D}_j^2 + u_{2,j} \\ \hat{D}_j^1 & = \alpha_3 + \lambda_{3,0}\bar{\hat{\beta}}_j^{SM,2} + \lambda_{3,1}\bar{\hat{\beta}}_j^{SM,2} \times \hat{D}_j^2 + \lambda_{3,2}\hat{D}_j^2 + u_{3,j} \end{cases} \\ &\text{2nd stage: } \hat{\beta}_j^{TM} = c + \delta_0\bar{\hat{\beta}}_j^{SM,1,pred} + \delta_1\bar{\hat{\beta}}_j^{SM,1,pred} \times \hat{D}_j^{1,pred} + \delta_2\hat{D}_j^{1,pred} + \epsilon_j. \end{aligned} \tag{IA10}$$

In all regressions, we take into account the issue of weak instruments as, for instance, raised in Stock and Yogo (2005). To this end, we exclude instruments that have a t-statistic in the first stage that is below 4.05.

IA1.4 OLS and IV Results Simulations

To show that the OLS estimator is biased whereas the IV estimator is not, we run 1,000 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 $\kappa_1 = 0$. We start with the OLS and IV estimates of the true team effect in Equation (IA4). The results are depicted in Panel A of Table IA1 where we observe that the OLS estimate gives an average coefficient of 0.986, with a standard deviation equal to 0.021, and we reject $\delta_0 = 1$ in 18% 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 1.000 with a standard deviation equal to 0.022. For the IV estimator, we are only able to reject the null of a full transmission in 5% of the cases. When estimating the model with interaction terms, we also find similar results in Panel B of Table IA1, where the IV estimator provides us with an unbiased estimate for δ_0. We also run simulations with different parameter values to exacerbate the measurement error bias to illustrate the effectiveness of the IV estimator: whereas the OLS estimate gets more downward-biased if measurement error increases, the IV estimator gives an unbiased estimate equal to the true parameter $\kappa_0 = 1$.

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 $\kappa_1 = 0$, 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.[38] When simulating the transmission coefficient under the null $\kappa_1 = 0$, Panel B of Table IA1 shows that we do not falsely reject the null using both the OLS and IV estimators.

[Place Appendix Table IA1 about here]

IA1.5 Multiple IV Draws

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 1,000 disjoint samples of the holdings from each manager in a given quarter using the same methodology of the main analysis (Section 4.2-4.3). 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 IA1, we show the distribution of the coefficient on $\hat{\beta}_j^{CF}$ for the 1,000 draws where we exclude draws that produce weak instruments.[39] 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 slightly below 1, with a mean coefficient equal to 0.78 and a standard deviation of 0.24.

[Place Appendix Figure IA1-IA2 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 IA2, we find that the distribution of the transmission coefficients

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

[39] 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 7 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.

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 0.94 with a standard deviation of 0.37. These estimates imply that we cannot reject the null of a full transmission for contrarians. On the other hand, the average transmission coefficient for extrapolators equals 0.32 with a standard deviation of 0.31. These estimates imply that we do reject the null of a full transmission of behavioral biases to the team, whereas we are not able to reject the null of no transmission at all.

Table IA1. **The transmission of extrapolation bias to teams — IV 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 preceding columns we present the estimated parameters using OLS and IV procedures respectively for three different sets of parameters, σ_β and σ_e. 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: Team Effect

		$\sigma_\beta = 0.5$, $\sigma_e = 1$		$\sigma_\beta = 0.5$, $\sigma_e = 2$		$\sigma_\beta = 0.2$, $\sigma_e = 1$	
	True Coefficient	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**
Avg. (α)	0	-0.001	0.000	-0.003	0.000	-0.004	0.000
Std. (α)		0.007	0.008	0.010	0.011	0.008	0.008
Avg. (T_α)		-0.094	0.003	-0.252	0.004	-0.527	-0.001
Avg. (δ_0)	1	0.986	1.000	0.947	1.001	0.920	1.001
Std. (δ_0)		0.021	0.022	0.033	0.034	0.055	0.059
Avg. (T_{δ_0})		49.513	46.618	34.664	30.444	19.213	17.457
% Rejected H_0: $\delta_0 = 1$		0.175	0.053	0.586	0.054	0.499	0.057

Panel B: Interaction Terms

		$\sigma_\beta = 0.5$, $\sigma_e = 1$		$\sigma_\beta = 0.5$, $\sigma_e = 2$		$\sigma_\beta = 0.2$, $\sigma_e = 1$	
	True Coefficient	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**
Avg. (α)	0	-0.001	0.000	-0.003	0.002	-0.004	0.002
Std. (α)		0.009	0.012	0.013	0.020	0.010	0.014
Avg. (T_α)		-0.073	0.003	-0.196	0.106	-0.412	0.100
Avg. (δ_0)	1	0.986	1.000	0.947	1.005	0.920	1.009
Std. (δ_0)		0.028	0.034	0.042	0.056	0.068	0.091
Avg. (T_{δ_0})		35.927	30.284	25.137	18.833	14.732	11.277
% Rejected H_0: $\delta_0 = 1$		0.131	0.052	0.387	0.041	0.357	0.042
Avg. (δ_1)	0	0.000	0.001	-0.001	0.032	-0.001	0.079
Std. (δ_1)		0.081	0.112	0.112	0.194	0.229	0.434
Avg. (T_{δ_1})		-0.004	0.004	-0.006	0.125	-0.004	0.143
Avg. (δ_2)	0	0.000	0.000	0.000	-0.023	0.000	-0.022
Std. (δ_2)		0.034	0.058	0.048	0.111	0.039	0.094
Avg. (T_{δ_2})		0.005	-0.008	0.007	-0.175	0.004	-0.201

Figure IA1. **IV — empirical simulations**: This figure shows the distribution of transmission coefficients (δ_0) that we obtain for different random samples using our IV methodology based on Equation (6), excluding the interaction term. The mean of the transmission coefficient equals 0.78 with a standard deviation of 0.24. The transmission coefficient estimated in the main specification in Column 6 of Table 5 equals 0.71.

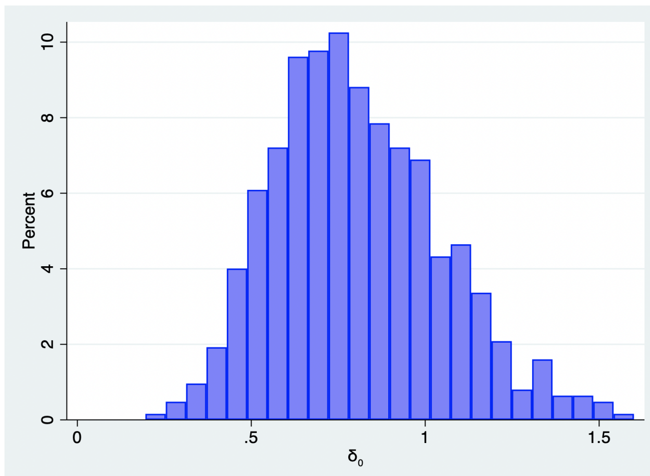
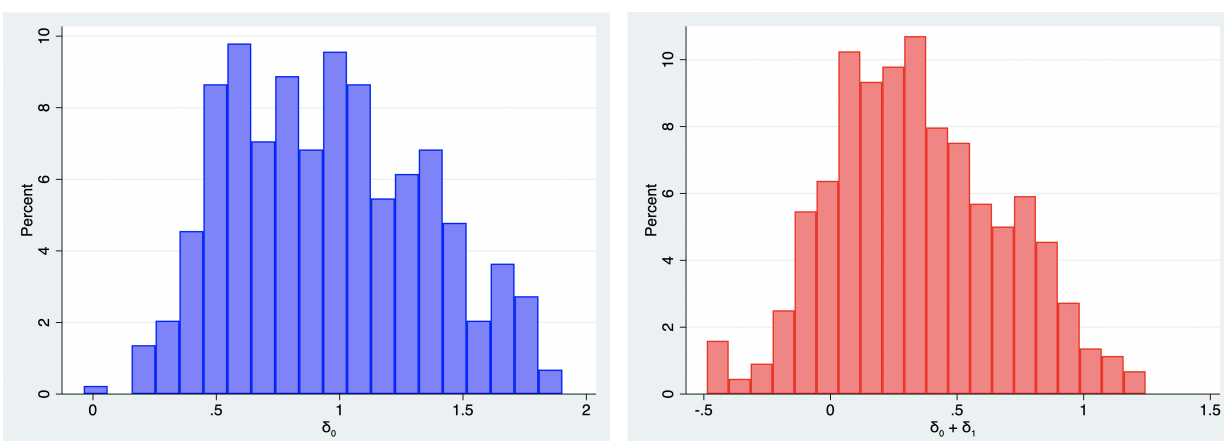


Figure IA2. **IV empirical simulations — contrarians versus extrapolators**: This figure shows the distribution of transmission coefficients for contrarians (δ_0) and extrapolators ($\delta_0 + \delta_1$) that we obtain for different random samples using our IV methodology based on Equation (6). The average transmission coefficient for contrarians equals 0.94 with a standard deviation of 0.37 and for extrapolators the average transmission coefficient equals 0.32 with a standard deviation of 0.31. The transmission coefficient estimated in the main specification in Column 8 of Table 5 equals 0.99 for contrarians and 0.36 for extrapolators.



(a) Contrarians

(b) Extrapolators

References Internet Appendix

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