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Abstract

We investigate how teams impact return extrapolation, a bias in belief formation which is pervasive at the individual level and crucial to behavioral asset-pricing models. Using a sample of US equity money managers and a within-subject design, we find that teams attenuate their own members' extrapolation bias by 75%. This reduction is not due to learning or differences in compensation, workload, or investment objectives between solo-managed and team-managed funds. Rather, we provide supportive evidence that team members engaging in deeper cognitive reflection can explain the bias reduction.

Keywords: expectation formation, extrapolation, heuristics, teams.

JEL classification: G23, G41, D91.

Resumen

Investigamos la manera en que los equipos influyen en la extrapolación de rentabilidades, un sesgo en la formación de creencias que es generalizado a nivel individual y crucial para los modelos conductuales de valoración de activos. Utilizando una muestra de gestores de fondos de inversión en acciones de EEUU, encontramos que los equipos atenúan el sesgo de extrapolación de sus propios miembros en un 75%. Esta reducción no se debe al aprendizaje ni a diferencias en la remuneración, la carga de trabajo o los objetivos de inversión entre los fondos administrados individualmente y los administrados en equipo. En cambio, proporcionamos evidencias que apoyan la hipótesis de que la reducción del sesgo proviene de los miembros del equipo que participan en una reflexión cognitiva más profunda.

Palabras clave: formación de expectativas, extrapolación, heurística, equipos.

Códigos JEL: G23, G41, D91.

1. Introduction

In key areas of decision-making such as choice under uncertainty, individuals often rely on simple heuristics when forming expectations about the future (e.g., Tversky and Kahneman, 1974; Shleifer, 2000; Barberis and Thaler, 2003). In particular, recent evidence indicates that when forming expectations about the future value of an asset, investors extrapolate past returns, i.e., their expectations about future stock returns are a positive function of recent stock returns. Extrapolative expectation formation represents a widespread feature of expectations formation in financial markets (e.g., Greenwood and Shleifer, 2014; Da, Huang, and Jin, 2021), despite the fact that in the data past stock returns have only limited predictive ability for future stock returns. Thus, return extrapolation leads some investors to hold biased beliefs about the future value of stocks, and recent theoretical work demonstrates that this extrapolation bias can reconcile various asset pricing facts, ranging from excess volatility in aggregate stock returns to return predictability at the asset-class level and in the cross-section (e.g., Barberis and Shleifer, 2003; Barberis, Greenwood, Jin, and Shleifer, 2015, 2018; Barberis, 2018; Jin and Sui, 2022).[1]

The starting point of this paper is the observation that the evidence on the pervasiveness of the extrapolation bias is based on surveys that extract individuals' own expectations about future asset returns. Yet, many financial decisions are often made not by individuals, but by teams. What is more, a large experimental literature in economics indicates that teams' behavior can differ systematically from the behavior displayed by individuals.[2] As the presence of teams as an organizational structure keeps increasing in the stock market, for instance via the growing adoption of teams among money managers (see, e.g., Figure A1), we argue that it is key to investigate to what extent teams inherit or correct their own members' tendency to extrapolate.

The answer to this question is not obvious. 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, the pervasiveness of the extrapolation bias among individuals is likely to generate similar biases in beliefs among team members and hence hinder teams' ability to identify such mistakes. In fact, if team interactions induce groupthink (Janis, 1972; Bénabou, 2013), teams can even exacerbate rather than reduce the extrapolation

[1] For more empirical evidence on return extrapolation, see Cutler, Poterba, and Summers (1990); De Long, Shleifer, Summers, and Waldmann (1990); Frankel and Froot (1990); Hong and Stein (1999); Barberis and Shleifer (2003); Glaeser and Nathanson (2017); Cassella and Gulen (2018); Adam, Marcet, and Beutel (2017); DeFusco, Nathanson, and Zwick (2022). See Barberis (2018) for a review.

[2] See for instance Hill (1982), Kerr, MacCoun, and Kramer (1996) and Charness and Sutter (2012) for a survey of the literature. In these studies, teams are compared to individuals in a variety of tasks. In some tasks, teams fare better than in individuals (learning and concept-attainment tasks), while individuals achieve better outcomes in other situations (e.g., when dealing with problems that do not have a clear answer).

bias that is exhibited by their members. Overall, whether teams attenuate or exacerbate the extrapolation bias which is pervasive at the individual level is an open empirical question.

In this study, we investigate this question by using field data, as we study 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.[3] 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 manager makes investment decisions individually as well as a member of a team. By performing this within-subject comparison, we can recover the impact of teams as an organizational structure on the extrapolation bias, while avoiding that unobservable differences between the managers in solo-managed funds and those in team-managed funds contaminate our analysis.

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 two other 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 only about 3% 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 stand-

[3]See Benartzi and Thaler (2001), Malmendier and Nagel (2011), Bordalo, Gennaioli, and Shleifer (2018).

[4]When estimating the return-extrapolation metric for a fund or a manager, we control for known determinants of fund trading behavior, such as stock characteristics and flow-induced trading. For robustness, we also 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.

[5]The results of the regressions are in Table A1. 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.

point. 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 influenced by fund performance, ranking, and size (e.g., Guercio and Tkac, 2008; Ibert, Kaniel, Van Nieuwerburgh, and Vestman, 2018; Ma, Tang, and Gómez, 2019), these results indicate that fund managers who extrapolate past returns are systematically worse off compared to fund managers who do not extrapolate. Although extrapolation in our sample is linked to worse financial outcomes, consistent with biases in beliefs, our extrapolation metric is based on trades as opposed to direct survey-based measures, thus it could reflect differences in risk attitudes across fund managers. Further tests show that risk aversion, hedging motives (e.g., Breeden, 1979; Campbell and Cochrane, 1999), or preference for skewness (e.g., Barberis and Huang, 2008) are unlikely to explain why fund managers who extrapolate achieve worse investment outcomes. 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 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 team-based asset management alleviates or exacerbates biases relative to solo asset management, in a setting where 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 teams' extrapolation bias on team members' average individual-level extrapolation bias, teams attenuate the extrapolative behavior that their members exhibit at the individual level by 75% 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, Noh, Pukthuanthong, Roll, and Wang (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 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 explain the reduction of extrapolation bias 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 and team behavior sometimes occur at different points in time, raising concerns about time-varying managerial characteristics or fund policies influencing the results. One plausible explanation is a learning story, suggesting that managers accumulate expertise during solo-management years, leading to a lower extrapolation bias when they later operate in a team. Similarly, fund families observing individual managers' biases may implement policies to

[6]We also conduct analysis on whether systematic differences in investment mandates between solo-managed and team-managed funds, in particular with respect to fund style and fund families, could reconcile our result. We find that such style migrations and fund family transitions cannot explain the reduction of extrapolation bias in teams.

curb such biases when they operate as part of a team. The 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 reduction of extrapolation bias in teams only when managers first operate individually, giving them and their fund families the opportunity to learn, before joining a team. However, our findings indicate a reduction in extrapolation bias regardless of whether team-based management follows or precedes individual management. Thus, learning about investor biases cannot reconcile our results.

Third, an additional identification challenge stems from changes in the manager's work environment 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 their 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 total assets or number of stocks that a manager oversees across all the portfolios he manages as a proxy for his 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 cannot reconcile our result.

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., 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 that suggest an upcoming cognitive mistake (e.g., Kahneman, 2000; Stanovich and West, 2008; Sloman, 2014). Therefore, we propose that teams may be able to reduce the extrapolation bias by creating cues that stimulate 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 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 extrapolative behavior has for internal reflection as opposed to external screening, we test which of these two mechanisms is better supported by the data. While our tests show suggestive evidence in favor of the internal reflection hypothesis and less supportive of the external screening hypothesis, we leave a deeper examination of the channel underlying the reduction of extrapolation bias in teams to future work.

Our study provides new evidence concerning the incidence of investment 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 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, Ince, and Kadlec, 2016; Akepanidtaworn, Di Mascio, Imas, and Schmidt, 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 teams in the asset management industry reduces or exacerbates this return extrapolation bias.

Our paper also 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, Heinz, Krueger, and Zubanov, 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). Outside of the lab, Harvey, Liu, Tan, and Zhu (2021) and

[7]See for instance Cooper and Kagel (2005) and Charness, Rigotti, and Rustichini (2007).

Evans, Prado, Rizzo, and Zambrana (2021) use data on mutual funds to investigate whether teams and team diversity affect creativity and performance. A recent working paper by Fedyk, Patel, and Sarkissian (2020) studies overconfidence among investment teams. We differ fundamentally from their work, in that the literature attributes a very different origin to overconfidence (preferences and ego utility, e.g., Bénabou and Tirole, 2002) vis-a-vis extrapolation bias (biased expectations due to cognitive mistakes). As a result, our paper speaks to how teams tackle biases in investor behavior that are due to heuristics rather than preferences.

Our paper also offers a methodological improvement over existing literature that studies 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.[8] The lack of subjects' random assignment in this setting can pose a 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 teams and solo managers may differ from each other along important dimensions (e.g., Huang, Qiu, Tang, and Xu, 2019) that can correlate with biases in investor behavior. 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.[9,10]

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).

[8] Examples of earlier work on teams based on field data include Prather and Middleton (2002), Bär, Kempf, and Ruenzi (2011).

[9] 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. However, 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.

[10] Later, in Section 7.2 we discuss the broader generalizability of our findings.

In our first step of the data preparation procedure, we merge the CRSP and Morningstar Direct mutual fund databases. The merger is based on work by Berk and van Binsbergen (2015) and Pástor, Stambaugh, and Taylor (2015). While a detailed description of the merger is in the Internet Appendix IA1, we provide a brief summary here. We first clean CRSP and Morningstar separately by following Pástor et al. (2015). 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) 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 $1.36 billion. Of all the mutual funds, 68% 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 30 quarters, or 7.5 years. Each fund holds a median of 60 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 IA2.[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., Gantchev, Giannetti, and Li, 2021):[13]

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

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.

[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.

[13] The output of the regressions is nearly identical if we instead use the lagged price $P_{s,t}$ and lagged TNA $TNA_{j,t}$ in Equation (2).

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:

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

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.[14] 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) 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 ($flow_{j,t+1}$). Thus, formally we have :[15]

[14] Formally, based on the value of $\lambda = 0.56$ estimated on average across surveys, the relative weight of the first four quarters in the sum is $\sum_{j=0}^{3} \frac{\lambda^j}{\sum_l \lambda^l} = 1 - \lambda^4 = 0.9016$.

[15] 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.

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

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.

In each quarter, the stocks that appear in the cross-section of trades in Equation (1) is based on a definition of the relevant 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.

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 50% of the sample characterized by an extrapolation beta larger than zero, and the remaining 50% 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 the disposition effect (Odean, 1998) and momentum trading (Jegadeesh and Titman, 1993).[16] Our extrapolation metric has a small correlation with both the disposition effect (-13%) and momentum trading (15%) and, as we show in Table A1 of the Appendix, momentum and the disposition effect capture about 3% of the cross-sectional variation in extrapolation. These low correlations are to be expected. Extrapolation and the disposition effect only have a small correlation in Liao, Peng, and Zhu (2022), who examine the relation between the two in a large sample of retail investors. Similarly, the weak relation between extrapolation and momentum is expected, given the conceptual and empirical differences between the two forms of trading behavior. In particular: (i) unlike momentum, that makes prescriptions about how to trade

[16] 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 IA2.

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; (ii) 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 exponentially declining weights; (iii) in keeping with Barberis (2018)'s view of extrapolation as a form of delayed over-reaction, our extrapolation metric captures investors' behavior in quarter $t+1$ based on returns realized a quarter earlier, while momentum trading suggests what stocks to buy at $t+1$ based on stock returns at that point in time. To further emphasize the distinction between extrapolation and momentum, we also conduct a return spanning test in Appendix A1 and show that momentum does not span a long-short strategy that goes long (short) the most (least) extrapolative funds.

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 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). 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 in Section 5. There we also explore more subtle alternatives where we show that our main results are robust to other definitions of the investment universe, to a specification where we abstract from defining the investment universe and only include actual managers' trades, and alternative controls for flow-induced trading.

[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 Guercio and Tkac (2008), Ibert et al. (2018), and Ma et al. (2019), who show that managers' compensation is tied to fund performance as well as the growth of the funds' assets.[17] Based on these insights, we test the implications of extrapolation for fund performance, fund flows, and the likelihood of achieving top-fund status.

We begin our analysis on the implications of extrapolation for managers' compensation in Figure 1. The figure presents graphical evidence on the relation between extrapolation and fund performance. 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 (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. This positive relation between contrarian behavior and fund performance is consistent with the view that in a market with extrapolators, rational investors trade as contrarians (e.g., Barberis et al., 2015). 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 -0.48) to high extraopolation beta (Q5, whose average beta is 0.78). 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.

[17]Ma et al. (2019) use data from mutual funds' disclosure to the SEC to analyze funds' stated compensation criteria. Their results stresses the importance of performance for compensation. Ibert et al. (2018) use data from a sample of Swedish mutual funds to document the sensitivity of managers' labour income to fund size and fund benchmark-adjusted returns. Guercio and Tkac (2008) show that achieving top-fund status within the same-style category has a positive impact on managers' compensation and possibly their career prospects.

[18]The construction of these performance metrics is standard. Thus, further details are offered in Appendix IA2.

[Place Figure 1 about here]

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 IA2). 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, we analyze the relationship between fund extrapolation and returns, using either simple fund returns, benchmark adjusted returns, CAPM alphas, Fama-French 3-factor alphas, or Fama-French 5-factor alphas. Across all specifications, 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.

[Place Table 2 about here]

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

[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.

[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. Cremers and Petajisto, 2009; Pástor et al., 2015).

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

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 Table 3, we also confirm that the negative impact of extrapolation on managerial financial outcomes extends to other determinants of managers' pay such as fund flows (which determine fund size and fund revenues), and the achievement of top-fund status. In Columns 1 and 2, 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. In Columns 3 and 4, we investigate whether extrapolative behavior hinders funds' ability to attain a top-fund status. Following Guercio and Tkac (2008), 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 3 about here]

On the one hand, the results so far offer a qualitative indication that managers who extrapolate past returns underperform their peers, receive lower inflows and thus grow less, and are more likely to miss star-fund bonuses. On the other hand, given our focus on the implications of extrapolation for managerial pay, the magnitude of the regression coefficients reported in Table 2 and 3 by themselves do not have a straightforward quantitative interpretation. Simply put, we are not interested in whether the performance of a fund worsens due to extrapolation. Rather, in the context of our analysis, the relevant economic effect of extrapolation concerns the extent to which the manager of a fund is himself made worse off by extrapolation. To assess quantitatively whether extrapolative behavior has an economically meaningful impact on managers, one needs estimates of the sensitivity of managerial compensation to performance and flows. In this respect, we rely on insights in by Ibert et al. (2018), who measure such sensitivities using a dataset linking managerial compensation to managerial performance. Using their estimates of such sensitivity, and our estimates of the impact of extrapolation on performance and flows, we are able to provide estimates of the overall impact of extrapolation on managers' income and wealth accumulation. Such estimates, presented in Appendix IA3, indicate that extrapolation can have a non-negligible impact on managers: a one-standard deviation increase in extrapolation could reduce average yearly income by 4 to 6%, and has a similar impact on the overall wealth accumulated from income. These estimates may very well be a lower bound for the impact of extrapolation on manager's wealth accumulation, because: (i) they do not incorporate the impact of top-fund status on performance and (ii) they do not account for the broader implications that extrapolation can have for fund managers' careers (e.g., through the probability of being laid off).

Further tests in Appendix A2 also investigate if managers' risk attitudes and preferences 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 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.

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, Hong, Huang, and Kubik, 2004; Bär et al., 2011). Following this design, one compares how prevalent the extrapolation bias is among 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]

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, Gneezy, and Kuhn, 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.

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.[24] In total we have 467 unique managers, 847 unique funds, and 308 unique teams that satisfy these conditions. That is, the mutual funds in our restricted sample make 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

[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 with skill, extrapolative managers might be more likely to be in team-managed funds. On the other hand, Kocher, Strauß, and Sutter (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]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.

regression model outlined in Equation (1). The choice of pooling is because of data limitations. Simply put, there are too few observations where teams are operating at the exact same time when all of their members manage a fund individually. 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 or the relative timing of individual and team-based asset management. We find that our results are unlikely to stem from our simple design choice.

Table 4 shows the summary statistics for the restricted sample, where 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 manager experience, number of stocks held, and fund fees. Similarly, the fraction of extrapolators in the restricted 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 4 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. By keeping the fund managers constant in both types of funds, this comparison allows us to draw some potential 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.

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 IA4, 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 5. 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.[25] 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 5 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 reduced to only 0.09, or a reduction of 50%. In stark contrast, contrarian managers' tendency to extrapolate remains similar when these managers join a team. These findings provide suggestive evidence that when managers join a team, their biased behavior gets considerably attenuated.[26]

[Place Table 5 about here]

Although the initial analysis suggests teams can counter biases, this analysis does not yet address 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 6 summarizes the results.

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

[25] 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 87 stocks when they operate in a team. This difference is economically small and, as we argue in detail in Internet Appendix IA6, a larger number of stocks overseen by a team does not help reduce extrapolation in teams vis-a-vis solo-managed funds.

[26] We report more detailed statistics about the estimated extrapolation betas in teams and their counterfactuals in Table IA4 of the Internet Appendix.

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

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. 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 adopt 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 are shrunk toward zero due to measurement error and that an IV procedure can correct for this issue. Based on the IV, we are not able to reject the null hypothesis of a full transmission of individual behavior to the team at any level of significance.[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 0.45. This result is economically important, because it implies that extrapolative behavior at the individual level is attenuated in teams by close to 55%. 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 $\delta_0 + \delta_1 = 1$, at standard levels of significance. These results are robust to the

[27] The critical value for the weak instrument test based on correlations proposed by Nelson and Startz (1990) and applied in Jegadeesh et al. (2019) is 0.057 and is based on the number of teams. We find a correlation between the two sets of disjoint extrapolation betas of 0.53.

[28] In Internet Appendix IA4.5, 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.

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 6 about here]

In constructing the team counterfactuals, we assign all the members of a team equal 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. The second goal is to ask whether confounding effects drive the observed reduction of extrapolation bias in teams. In this respect, time-varying managerial characteristics or fund policies can confound our results. Whereas there can be various changes in the characteristics of managers and fund family policies over time, 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 important one, and we devote several tests to rule out this story. Furthermore, differences in fund characteristics between the solo-managed and team-managed funds could explain our main result. We argue that some differences in fund characteristics have a better potential to explain our

[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.

result. In particular, differences in managerial incentives between solo-managed and team-managed funds could explain our result: incentives are generally thought of inducing more rational behavior, and thus if incentives were steeper in teams than in individual funds, such a difference in incentive structure could explain the attenuation of extrapolation observed in teams. We investigate this explanation by hand-collecting data from mutual funds' statement of additional information (SAI) filed with the SEC. Our analysis shows that incentives in teams are not stronger than those in individual funds.

Aside from these two important checks, we also consider additional confounders: (i) fund managers may 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 and team funds could reconcile the result; (iii) managers' employment at different investment 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. Measuring Extrapolation

In this section we show that our results are robust to alternative measurements of extrapolative behavior. We consider three such alternatives: (i) to further ensure that we are not picking up funds' momentum trading, we exclude momentum stocks from the sample when measuring extrapolation;[30] (ii) to show that our way to measure funds' trading behavior does not drive our results, we measure extrapolation in regressions where funds' trading in a stock is measured as the active change in portfolio weights by the fund, i.e., the change in weights that is obtained after accounting for the mechanical change in weights that occurs due to cross-sectional heterogeneity in stock returns;[31] (iii) to alleviate concerns that the use of decaying weights drives our results, we re-estimate our extrapolation metric using past-year stock returns (i.e., we use the same weights for returns in the past 4 quarters).

[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 $t-1$ (e.g., Jegadeesh and Titman, 1993; Fama and French, 1996).

[31] A simple quarter-on-quarter change in portfolio weights is not appropriate, because portfolio weights can change even in the absence of active managerial decisions depending on 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 buying or selling of stocks 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.

Table 7 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 and statistically significant. More importantly, Panel B shows that the evidence that teams help alleviate the extrapolation bias is confirmed when using the alternative extrapolation metrics presented in this section.

[Place Table 7 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 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.

The learning explanation hinges on the existence of differences in the timing of individual management and team-based management for a given manager. However, summary statistics on fund managers' industry tenure when managing individually and when operating in a team document that there is only a small difference of four quarters between the median experience that managers have accrued when we observe them acting in teams as opposed to individually (Table 4). This small difference provides preliminary evidence against a learning explanation. Moreover, in Table 8, we repeat our main tests to examine individual and team-based behavior at comparable points in time. For instance, in the first two columns, we restrict the measurement of teams' extrapolative behavior to a period when at least one of the team managers is also managing a fund individually. The number of teams that survive when looking at this case equals 280, which means that teams and individual management often happen at a similar point in time. We then re-estimate the regression of Table 6, and find that the result of bias attenuation in teams remains the same. An alternative approach in Column 3-6 that restricts the measurement of teams' extrapolative behavior to a time frame that starts (ends) when the counterfactual team starts (ends) gives similar results.[32]

[Place Table 8 about here]

We also perform additional tests to examine the link between our main finding that teams reduce extrapolation and the timing of individual versus group-based management. These tests are based on the intuition that, for learning to explain team-level reduction in biases, individual management should *precede* team-based management. If so, the reduction of the extrapolation bias in teams should only occur in cases in which managers first manage individually and later manage as part of a team. However, as we detail in Internet Appendix IA5, we do not find support for this hypothesis. Hence, these findings alleviate concerns that managerial learning, or learning by fund families (either about managers, or about the negative impact of extrapolation altogether), is responsible for our result.

In our third and final test, we probe the learning story by focusing 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 no attenuation of the extrapolation bias in the teams composed of non-learner managers. To identify non-learners, we first estimate fund managers' extrapolative behavior separately for

[32]To be precise, the start date of the counterfactual team is determined by averaging the start dates of individual operation for each team member, and the end date is determined in a similar manner. To illustrate, suppose that Manager A starts individual fund operations in 2005Q1 and ends in 2015Q1, while Manager B starts operating individually in 2009Q1 and ends in 2019Q1. In this scenario, the start date of the counterfactual team would be 2007Q1, and the end date would be 2017Q1.

the first half (the early sample) and the second half (the late sample) of the sample period in which these managers managed a fund alone. We then select the non-learners as the managers who extrapolate in both subsamples.[33]

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 6. 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]

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

[33] 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 $= -1.29$).

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.[34] 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) (only) 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 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 these teams, we were able to collect data on 128 teams, out of which 65 are extrapolative teams. Table 9 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 9 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

[34] 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.

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 or team j at a given point in time as the TNA that the manager or team oversees across all portfolios. We compute the workload of the team as the time-series average of TNA during the team's existence. For the counterfactual workload, we take the equally weighted time-series average of the team members' workload when they operate individually. In others words, we assume that a team equally divides TNA across its 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 10 highlight the importance of 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 \$0.71 billion less in TNA compared to when they manage funds on their own.

If this difference in workload explains our finding, we 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 the following regression:

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

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 explains 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 C of Table 10 shows that δ_1 is statistically indistinguishable from zero. Moreover, Internet Appendix IA6 shows that we obtain similar results if we use as a proxy for changes in workload the number of stocks that are overseen by managers when they operate in a team versus when they operate individually. 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 similar regression as in Equation 8, where we replace $\Delta Workload_j$ with D_j^{SM}, and D_j^{SM} is an indicator variable 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 10). If the style migrations drive our results, we expect the coefficient δ_1 to be statistically significant and negative. Panel B of Table 10 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.[35] We then estimate a regression with a similar double-interaction term as in Equation (8), where we replace $\Delta Workload_j$ with D_j^{FS}, and where D_j^{FS} indicates a fund family switch for team j.

Table 10 shows that 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 D of Table 10 shows that δ_1 is statistically indistinguishable from zero. We therefore conclude that switches in mutual fund families cannot explain our result.

[**Place Table 10 about here**]

6. Channel

6.1. Conceptual Framework

The question of what mechanism delivers the documented bias attenuation is important. Answering this question provides insights into optimal team design and how teams can effectively deliver on the promise of attenuating individual-level biases. To tackle this question, we rely on a large body of work that argues that human cognition can be described through a dual-system model (Stanovich and West, 2000; Kahneman and Frederick, 2002; Kahneman, 2003). This model involves intuition (System I) and deliberation (System II). Since the cognitive resources of System II are limited, cognition has organized its activity by off-loading some tasks from System II to System I. This division of labour however leads individuals to systematic biases, because System I affords a speedy processing of incoming information by adopting simple rules of thumbs, henceforth heuristics, which often lead to deviations from rational expectations.[36] Heuristics such as the representativeness heuristic, and the work of System I, are believed to be a driver of the extrapolation bias.[37]

Since we aim at explaining how extrapolation bias is reduced in teams, and extrapolation is likely to derive from System I, we rely on work in psychology by Frederick (2005). He labels *cognitive reflection* as the ability to successfully engage System II to override the incorrect judgments of

[35]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.

[36]Although intuitive judgment can lead in some circumstances to suboptimal outcomes, this need not be the case in all settings (e.g., Gigerenzer and Goldstein, 1996).

[37]For instance, in Barberis, Shleifer, and Vishny (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; Glaeser and Nathanson, 2017).

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. Therefore, in the context of team-based asset management, what teams may be able to provide to achieve successful cognitive reflection is a set of relevant cues that help their members 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, leading a manager who is prone to return extrapolation in his or her solo-managed fund to reassess this tendency when operating as part of the team. We label this channel *internal reflection*. Of course, teams can also provide a second set of cues, in that team members who extrapolate past returns can recognize and suppress this mistake thanks to other team members' critical assessment.[38] We refer to this alternative channel as *external screening*.

6.2. Empirical Strategy

Internal reflection and external screening mechanism can be distinguished empirically in our setting, since 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 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 (i.e., there is larger transmission of individual behavior) in mixed teams as opposed to all-extrapolator teams.

External screening makes the opposite prediction. 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

[38] 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. See for instance (Marschak and Radner, 1972; Dessein and Santos, 2006).

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 (i.e., there is less correlation of extrapolation in homogeneous teams) 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:

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

where D_j^{AE} is a dummy that equals one if all team members are extrapolators, while D_j^{M} is a dummy that equals one if the team consists of both extrapolators and contrarians.

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

If internal reflection is mainly responsible for the reduction of biases, then we expect to find $\delta_0 + \delta_1 < \delta_0 + \delta_2$ or, more simply, $\delta_1 < \delta_2$; and there will be less transmission of individual-level behavior to the homogeneous extrapolative teams compared to mixed teams. On the contrary, external screening indicates that $\delta_0+\delta_2 < \delta_0+\delta_1$, or $\delta_2 < \delta_1$; 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 (9). We find in all specifications that $\delta_1 < \delta_2$ and δ_1 is statistically significant at the 5% or 1% significance level, whereas δ_2 is only significant at the 10% significance level in two out of three specifications. 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 1.5 times 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. Discussion

7.1. Beliefs versus preferences

Throughout the paper, we interpret the documented reduction of extrapolation in teams as evidence that teams curb biases in beliefs. We adopt this interpretation because the most common interpretation in the literature is that extrapolation arises from biases in beliefs (e.g. Barberis and Shleifer, 2003; Barberis et al., 2018; Liao et al., 2022). More recently, Jiang, Liu, Peng, and Yan (2022) link extrapolation and the larger weight placed on recent returns to investor memory, further strengthening the interpretation that biased cognition and biased recall of past data, rather than preferences, drive extrapolation. However, our measures of extrapolation are based on trades, as opposed to direct survey data on beliefs. So it is possible that our tests speak to how teams aggregate investor preferences rather than biases in beliefs.

While it is possible that preferences influence our tests, there are several considerations that make this alternative interpretation difficult. First, in a recent paper Liao et al. (2022) argue that the extrapolation bias entails a dependence of trading on past returns that is very specific to biases in beliefs, namely, that past returns enter trading decisions with a decaying weight. Following this insight, all of our main results are based on the use of decaying weights, and we select the weights assigned to past returns based on models of extrapolation that Greenwood and Shleifer (2014) calibrate to direct survey data on investor beliefs. This empirical design choice helps to strengthen the interpretation that extrapolation is belief-based and that our results reflect the reduction of biases in beliefs in teams.

To further strengthen the case that our evidence captures the reduction of biases in beliefs, we follow Da et al. (2021) and Liao et al. (2022), who argue that initial buys are more likely to be linked to beliefs rather than to preferences.[39] Therefore, we repeat our team regressions by only relying on first-purchases to measure fund manager's extrapolative behavior.[40] Table A5 of the Appendix shows that the conclusion that extrapolation bias is reduced in teams is robust to this alternative measurement.

[39] Initial purchases also help address the concern that extrapolation captures prospect-theory preferences with narrow framing as in Barberis and Huang (2001). In the model, good stock returns lower stock-specific loss aversion and increase the desire to purchase a stock, because of the cushioning role that the past stock returns provide. This cushioning role is only present if the investor previously held the stock. As a result, initial purchases, which by definition offer no cushioning, allow for addressing this preference-based explanation.

[40] Liao et al. (2022) use a non-parametric extrapolation metric based on retail investors' initial buys. In our setting, this non-parametric approach would not allow to control for institutional factors such as flow-induced trading and investment mandates that are important when studying mutual funds. Our regression-based approach allows to rely only on initial buys, while at the same time controlling for relevant alternative factors.

We also note that to the extent conditional skewness correlates with past returns, preferences for skewness as in Barberis and Huang (2008) could explain investors' buying of stocks on the back of good returns. With respect to this explanation, Boyer, Mitton, and Vorkink (2010) run predictive regressions of future skewness on a set of lagged stock characteristics. They find that idiosyncratic volatility is the strongest predictor of future skewness in the cross-section. Since we control for idiosyncratic volatility when we estimate fund managers' extrapolation, our measures of extrapolation are unlikely to proxy for skewess preferences. Moreover, Boyer et al. (2010) also show that past returns do not predict skewness after controlling for volatility, making it even more difficult to explain extrapolative trades with skewness preferences.[41]

To further rule out that our extrapolation metric captures preferences rather than beliefs, we perform additional tests. We argue that if our extrapolation metric reflects managers' preferences, then the managers we identify as extrapolators likely trade-off lower expected payoffs (see Table 2) for other valued features of the payoff distribution. We test three possible trade-offs: a risk-return tradeoff where extrapolators sacrifice performance but achieve less volatile compensation, a skewness-preferences tradeoff where lower performance on average is associated with a positively skewed performance distribution (e.g. Barberis and Huang, 2008), and hedging motives, whereby extrapolators fair better than non-extrapolators in bad states of the economy (e.g. Campbell and Cochrane, 1999; Guvenen, Ozkan, and Song, 2014). For brevity, we present the results in the Appendix A2, along with details about the data construction. All these tests do not show evidence of any of the above preference-based trade-offs. In sum, while we cannot entirely rule out a preference-based interpretation for our finding, we argue that many of our results point to a belief-based story.

7.2. Internal Validity versus External Validity

When using field data to perform empirical analysis, one is able to study decision-making in a setting that is more relevant for real-world decisions. However, field data poses several challenges, the largest of which is the lack of random assignment. When random assignment is not possible, different types of agents can be placed or self-select into either the treated or the control group, inducing differences in extrapolative behavior of teams and solo-managed funds that are not per se due to teams. In such settings, the comparison of all teams and all solo-managed funds that is used in the literature (e.g., Bär et al. (2011); Cici (2012)) can generate biased estimates of the role

[41] An investor with cognitive limitations may misinterpret the relation between past returns and skewness, and could mistakenly believe that past returns predict skewness. However, this biased belief about skewness would still be consistent with our proposed explanation of extrapolative behavior stemming from biases in beliefs.

of teams for decision-making. Our within-subject design, jointly with the large set of robustness checks we perform, boosts the internal validity of our conclusions compared to earlier work.

In spite of its advantages, a within-subject analysis inevitably shifts the focus from the full sample to a subset of managers who can be observed both in a team and individually. This subset represents only a portion of the asset management industry, leading to questions of external validity. Namely: (i) Can the fact that we operate on a selected sample of managers invalidate the conclusions we draw about the role of teams in the broader sample of mutual fund managers? (ii) How relevant is the reduction of extrapolation that occurs in teams for the mutual fund industry as a whole, as opposed to the subset that we use in our tests? (iii) Can we generalize the findings in the paper to settings other than the mutual fund industry?

As far as question (i) is concerned, a regression on a selected sample results in a biased estimate of the coefficient of interest relative to the full sample if the corresponding regressor also appears in the selection equation (e.g., Certo, Busenbark, Woo, and Semadeni, 2016). Selection can be framed in the spirit of Heckman (1974) as a latent-variable model describing the process by which some managers work individually, some managers work in teams, and some work in both contexts, with only the latter entering our restricted sample. So, relying on the special sample introduces biases in our estimation of the role of teams for extrapolation if extrapolation affects participation in the special sample. To investigate whether this is the case, in Appendix Figure A2, we compare extrapolation among the managers who only operate individually, and those managers who operate both individually and in teams (i.e., the managers in our restricted sample). If extrapolative behavior at the individual level contributes to the selection of a given managerial structure, we would expect to see a large difference between the individual extrapolative behavior that is displayed by these two sets of managers. Instead, there is a striking resemblance between the two distributions and their moments,[42] indicating that it is unlikely that extrapolation is a strong determinant of the selection into team-managed funds or solo-managed funds.

With respect to (ii), Appendix A7 presents suggestive evidence that teams may indeed reduce the extrapolation bias in the mutual fund industry at large. Briefly, we run predictive regressions of future changes in extrapolation at the fund level on lagged changes in managerial structure for the full set of mutual funds described in Section 2.[43] Across different specifications, we find that the introduction of team management in a fund predicts a reduction in the extrapolative behavior

[42] A difference in means test reveals that the means of the two distributions are statistically indistinguishable from each other (t-stat = 1.14).

[43] By using changes in outcomes and regressors, we already control for time-invariant (un)observed fund characteristics.

of the fund in the future. Of course, these estimates only reflect correlations that are consistent with our previous findings, and we leave a deeper investigation to future literature.

With respect to (iii), we caution that it is difficult to make conclusive statements on a broader generalizability of our findings. However, recent experimental work points to the fact that it is likely that teams help reduce the extrapolation bias at a broader level. Specifically, Enke, Graeber, and Oprea (2023) show that relative to other institutional arrangements, teams reduce a large set of individual-level cognitive mistakes including the neglect to account for mean reversion (Tversky and Kahneman, 1974). Such a reversal neglect is linked to extrapolation, since both imply a belief that a recent phenomenon will continue in the future more than is warranted under rational expectations. Moreover, Enke et al. (2023) also show that teams are better in Frederick (2005) cognitive reflection tests, in line with our interpretation that teams reduce extrapolation because they induce deeper thinking.

8. Conclusion

The return extrapolation bias is pervasive and known to negatively impact individuals' investment behavior and financial outcomes. However, it is an open question whether the extrapolation bias is 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 that extrapolation stems from biased beliefs. 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 organizational structures contribute to the attenuation or exacerbation of behavioral 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 bias reduction in teams? How are biases that stem from investor preferences (e.g., realization utility or prospect theory) dealt with in teams? These are only some of the questions that we leave to future work.

Appendix

Appendix A1 Spanning tests of extrapolative funds

We conduct a spanning test to examine the ability of momentum and the Fama-French 5 factors to explain a long-short strategy that goes long the highest 20% extrapolative funds and short the lowest 20% extrapolative funds. In order to address the discrepancy between academic momentum and practical momentum, we also include as an explanatory variable the return on a long-short portfolio that goes long (short) in the funds with the highest (lowest) momentum factor exposures. Table A2 shows that the momentum factor explains only a small fraction in the variation of the returns to the extrapolation strategy. Moreover, across all specifications, the factors do not span the long-short strategy, in that the alpha is consistently negative and statistically significant across all specifications.

Appendix A2 Beliefs versus preferences

In this appendix, we layout the results of additional tests to further rule out that our extrapolation metric captures preferences rather than beliefs. In Section 7, we argued that insofar our extrapolation metric reflects managers' preferences, then the managers that we identify as extrapolators are likely to trade-off the lower expected payoffs (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: i) lower volatility of returns, ii) preferences for skewness, and iii) hedging properties.

These three potential trade-offs 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 A6, we test whether extrapolation is associated with lower return volatility. We find no evidence that extrapolation leads to significantly lower volatility of returns. In Panel B of Table A6, 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 A6, 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.

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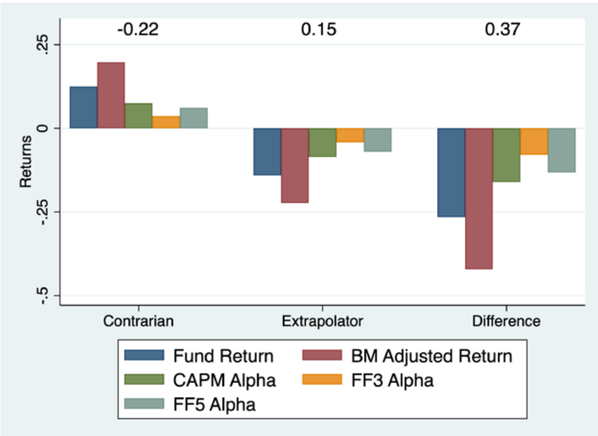
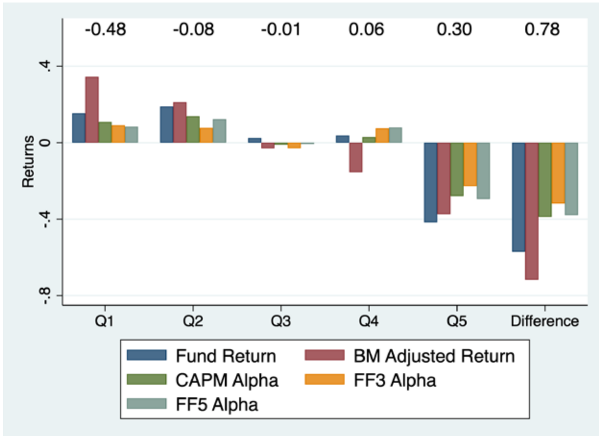
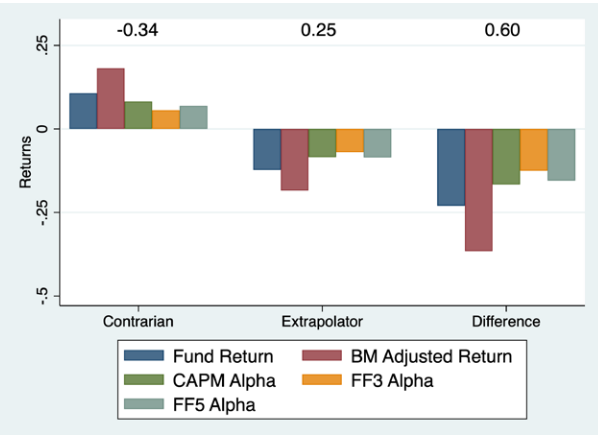
Figure 1. **Extrapolation and performance - graphical evidence**: In Panel A we estimate funds' extrapolative behavior over the full sample, and then sort funds into either two groups (A1, extrapolators and contrarians, left panel), or five groups based on quintile breakpoints (A2, right panel). Each 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 IA2.



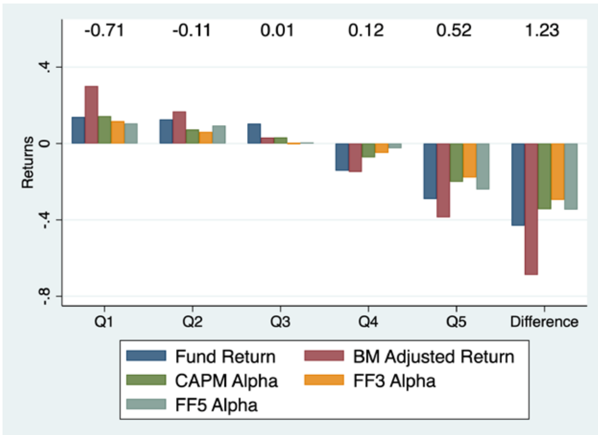
(a) Panel A1: Extrapolators and contrarians (full sample)



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

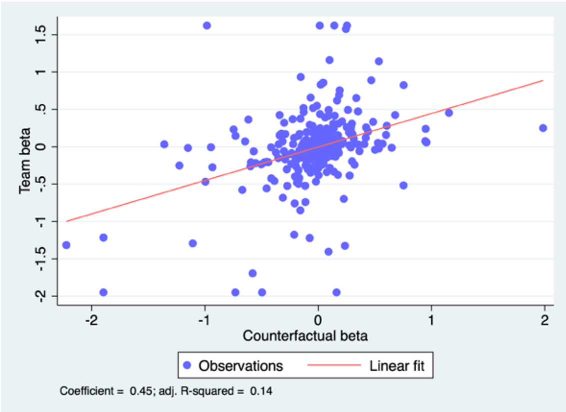


(c) Panel B1: Extrapolators and contrarians (recursive)

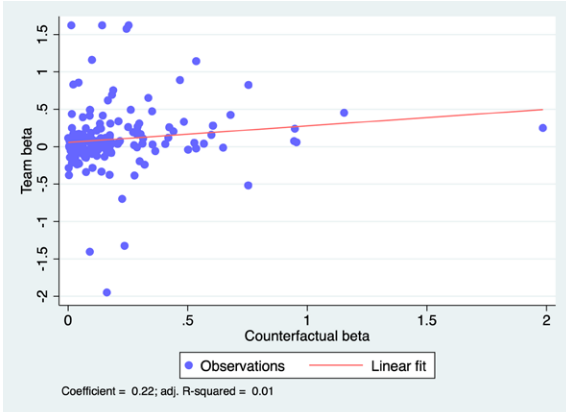


(d) Panel B2: Quintile sorts (recursive)

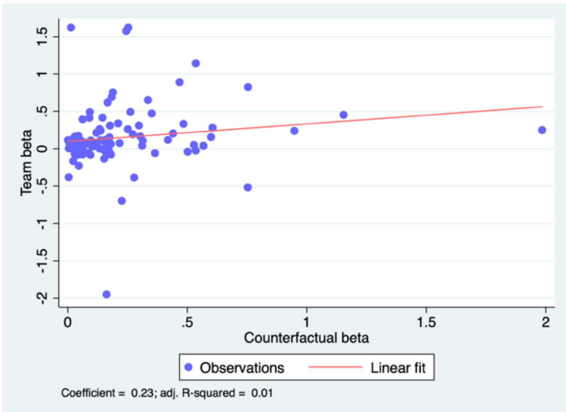
Figure 2. **Extrapolation in teams for non-learner managers**: This figure plots the team-level extrapolation metric ($\hat{\beta}_j^{TM}$) against the average extrapolation that team members display when managing a single-managed fund ($\hat{\beta}_j^{CF}$). The plot is presented for all teams (Panel 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

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 (i.e., estimated over the same sample period), 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 regressor on the right-hand side of Equation (1). The alternative extrapolation metrics are described in Section 5.1. *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 teams; *Number of Managers* is the number of managers managing a fund; *Manager Experience* is the average experience of the managers (quarters); *Number of Stocks* is the number of stocks held by a fund; *Fund Age* is the age of the fund since inception (years); *Expense Ratio* is the fund's total expense ratio (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 IA2); *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. All variables are winsorized at the 0.5th and 99.5th percentile. Panel B reports pairwise correlations of the aforementioned extrapolation metrics, and the correlation of funds' extrapolative behavior with it'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	-0.01	0.60	-0.51	0.00	0.37
Extrapolation Dummy	0.50	0.50	0	1	1
Extrapolation Beta (No Mom)	-0.01	0.46	-0.57	0.00	0.49
Extrapolation Beta (Weight Change)	-0.01	0.62	-0.50	0.00	0.38
Extrapolation Beta (Past Year Ret)	0.00	0.10	-0.09	0.00	0.07
Fund TNA	1357.18	4072.71	22.44	361.75	5435.25
Team Managed	0.68	0.36	0	1	1
Number of Managers	2.76	2.16	1	2	6
Manager Experience	32.78	15.08	13.52	30.37	61.90
Number of Stocks	96.44	159.73	26	60	253
Fund Age	14.86	11.59	4.67	11.81	38.04
Expense Ratio	1.19	0.39	0.64	1.16	1.81
Fund Turnover	0.80	0.54	0.20	0.69	1.81
β_{MOM}	0.01	0.10	-0.14	0.01	0.20
Disposition	-0.02	0.06	-0.13	-0.01	0.08
Mutual fund performance					
Fund Return	9.80	5.17	1.18	10.24	16.10
Benchmark Adj. Return	0.32	2.38	-3.48	0.29	4.11
CAPM Alpha	0.12	3.07	-4.51	0.16	4.42
FF3 Alpha	-0.35	2.35	-4.07	-0.29	3.01
FF5 Alpha	-0.12	2.54	-3.80	-0.25	3.88
Flow	3.50	23.54	-21.47	-0.82	39.67
Benchmark Adj. Volatility	1.53	1.67	0.61	1.31	2.80
FF5 Alpha Volatility	1.31	1.73	0.59	1.13	2.15

Panel B: Correlation table

Extrapolation Beta	1										
Extrapolation Beta (No Mom)	0.73	1									
Extrapolation Beta (Weight Change)	0.97	0.69	1								
Extrapolation Beta (Past Year Ret)	0.90	0.58	0.87	1							
β_{MOM}	0.15	0.16	0.13	0.10	1						
Disposition	-0.13	-0.13	-0.12	-0.09	-0.60	1					
β_{MKT}	-0.03	0.03	-0.02	-0.02	-0.11	0.01	1				
β_{SMB}	0.00	0.01	0.00	0.00	0.24	0.00	0.17	1			
β_{HML}	-0.16	-0.20	-0.15	-0.15	-0.59	0.52	-0.10	-0.08	1		
β_{RMW}	-0.08	-0.09	-0.07	-0.07	-0.27	0.26	-0.09	-0.08	0.62	1	
β_{CMA}	-0.13	-0.17	-0.13	-0.15	-0.21	0.29	-0.16	-0.19	0.44	0.46	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 IA2. 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 $(t-1)$	-0.237*	-0.355**	-0.298**	-0.331***	-0.372***	-0.405***
	[0.135]	[0.151]	[0.134]	[0.112]	[0.108]	[0.109]
Expense Ratio $(t-1)$	-0.024	0.17	-0.308	-0.051	0.332*	0.309
	[0.197]	[0.287]	[0.198]	[0.182]	[0.196]	[0.202]
Log Fund TNA $(t-1)$	-0.192	-0.670***	-0.339**	-0.235	-0.208	-0.214
	[0.174]	[0.220]	[0.161]	[0.145]	[0.151]	[0.147]
Log Fund TNA^2 $(t-1)$	0.015	0.044***	0.026**	0.018	0.023*	0.024**
	[0.013]	[0.017]	[0.012]	[0.011]	[0.012]	[0.011]
Log Fund Age $(t-1)$	-0.029	-0.036	-0.101	-0.073	-0.109	-0.128
	[0.087]	[0.118]	[0.087]	[0.074]	[0.076]	[0.079]
Fund Turnover $(t-1)$	0.106	-0.039	-0.278*	-0.340**	-0.075	-0.049
	[0.151]	[0.165]	[0.142]	[0.138]	[0.131]	[0.141]
Log N Stocks $(t-1)$	0.174**	0.092	-0.007	0.038	-0.027	-0.011
	[0.069]	[0.088]	[0.067]	[0.063]	[0.066]	[0.069]
Flow $(t-1)$	-0.005	-0.005	0.007*	0.013***	0.015***	0.016***
	[0.005]	[0.005]	[0.004]	[0.004]	[0.004]	[0.005]
Avg. Manager Exp. $(t-1)$	0.013	0.017	0.018	-0.004	-0.005	-0.008
	[0.045]	[0.054]	[0.047]	[0.041]	[0.042]	[0.042]
β_{MOM} $(t-1)$	-3.492*	-3.508*	0.665	1.106	1.068	2.444
	[2.023]	[1.806]	[1.838]	[1.555]	[1.677]	[1.739]
Disposition $(t-1)$	0.162	0.392	0.324	0.156	0.217	0.215
	[0.446]	[0.473]	[0.417]	[0.349]	[0.357]	[0.378]
Time $\times$ Style FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67634	67634	67629	67629	67629	61258
Adj. R-squared	0.8788	0.1012	0.435	0.2235	0.2255	0.2375

Table 3. **Extrapolation as a bias – additional performance metrics**: This table shows the linkage between extrapolation and future fund flows and achieving top status. 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. 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). 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 IA2). 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). Controls are the same as in Table 2. 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 $(t-1)$	-3.658***	-6.274***	-0.021***	-0.015***
	[1.271]	[1.753]	[0.007]	[0.005]
Expense Ratio $(t-1)$	-2.035*	-9.503***	0.023***	0.022***
	[1.123]	[2.382]	[0.008]	[0.006]
Log Fund TNA $(t-1)$	-9.225***	-18.678***	-0.022***	-0.014***
	[1.277]	[2.025]	[0.007]	[0.005]
Log Fund TNA^2 $(t-1)$	0.551***	1.349***	0.002***	0.001**
	[0.091]	[0.165]	[0.001]	[0.000]
Log Fund Age $(t-1)$	-3.590***	-4.745***	-0.002	-0.001
	[0.625]	[1.468]	[0.004]	[0.003]
Fund Turnover $(t-1)$	-0.877	-8.444***	0.011*	0.007*
	[0.775]	[1.423]	[0.006]	[0.004]
Log N Stocks $(t-1)$	0.701	3.157***	-0.028***	-0.019***
	[0.457]	[1.025]	[0.003]	[0.002]
Avg. Manager Exp. $(t-1)$	0.299	0.924	0.002	0.002
	[0.315]	[0.625]	[0.002]	[0.001]
β_{MOM} $(t-1)$	-7.97	7.84	-0.134***	-0.094***
	[5.764]	[8.771]	[0.043]	[0.027]
Disposition $(t-1)$	-4.499**	-19.355***	0.023*	0.018*
	[2.168]	[3.999]	[0.013]	[0.009]
CAPM Alpha $(t-1)$	1.654***	2.922***		
	[0.115]	[0.181]		
Flow $(t-1)$			0.000	0.000
			[0.000]	[0.000]
Time × Style FE	Yes	Yes	Yes	Yes
Observations	57632	57632	67634	67634
Adj. R-squared	0.0777	0.4403	0.0124	0.0148

Table 4. **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	-0.01	0.38	-0.50	0.01	0.42
Extrapolation Dummy	0.54	0.50	0	1	1
Extrapolation Beta (No Mom)	0.04	0.48	-0.47	0.01	0.64
Extrapolation Beta (Weight Change)	0.02	0.50	-0.53	0.01	0.42
Extrapolation Beta (Past Year Ret)	0.00	0.08	-0.11	0.00	0.10
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	0.02	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	-0.03	0.50	-0.56	0.00	0.52
Extrapolation Dummy	0.50	0.50	0	1	1
Extrapolation Beta (No Mom)	-0.01	0.63	-0.72	0.00	0.94
Extrapolation Beta (Weight Change)	-0.02	0.43	-0.58	0.00	0.56
Extrapolation Beta (Past Year Ret)	-0.01	0.10	-0.13	0.00	0.11
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}	0.04	0.15	-0.18	0.02	0.29
Disposition	-0.03	0.09	-0.20	-0.01	0.10

Table 5. **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	-0.01	-0.03	0.02	0.89	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	-1.66	308
β_{MOM}	0.02	0.02	0.00	-0.68	308
Disposition	-0.02	-0.02	-0.01	-1.44	308

Panel B: Contrarians

	CF	Team	CF - Team	t-stat	Obs.
Extrapolation beta	-0.22	-0.18	-0.05	-1.37	143
Fund TNA	1331.30	1126.35	204.95	1.13	143
Manager Experience	30.60	33.46	-2.86	-1.87	143
Number of Stocks	69.51	70.66	-1.15	-0.42	143
Fund Age	15.91	14.87	1.05	1.14	143
Expense Ratio	1.23	1.30	-0.07	-2.48	143
β_{MOM}	0.00	0.00	0.00	-0.15	143
Disposition	-0.02	-0.01	-0.01	-1.88	143

Panel C: Extrapolators

	CF	Team	CF - Team	t-stat	Obs.
Extrapolation beta	0.18	0.09	0.09	2.41	165
Fund TNA	1292.43	1291.89	0.54	0.00	165
Manager Experience	30.97	31.37	-0.40	-0.30	165
Number of Stocks	76.94	87.44	-10.50	-2.63	165
Fund Age	17.78	17.39	0.39	0.34	165
Expense Ratio	1.26	1.26	0.00	0.12	165
β_{MOM}	0.03	0.04	-0.01	-0.88	165
Disposition	-0.03	-0.03	0.00	-0.22	165

Table 6. **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)
$\hat{\beta}_j^{CF}$	0.6219***	0.5593***	0.9126***	0.8481***	0.9338***	0.8534***	1.1703***	1.1703***
	[0.0654]	[0.0663]	[0.1012]	[0.1030]	[0.1160]	[0.1179]	[0.1658]	[0.1682]
$\hat{\beta}_j^{CF} \times D_j^E$			-0.6737***	-0.6093***			-0.7167***	-0.6706***
			[0.1493]	[0.1475]			[0.2172]	[0.2080]
D_j^E			0.0221	-0.0036			0.022	-0.1224
			[0.0570]	[0.0573]			[0.1820]	[0.1858]
Style fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	308	307	308	307	308	307	308	307
Adj. R-squared	0.2253	0.264	0.2697	0.3002	0.0371	0.1011	0.0746	0.128
Hypothesis testing:								
p-value $H_0: \delta_0 = 1$	0.000	0.000	0.389	0.141	0.568	0.214	0.304	0.311
p-value $H_0: \delta_0 + \delta_1 = 0$			0.030	0.027			0.043	0.019
p-value $H_0: \delta_0 + \delta_1 = 1$			0.000	0.000			0.015	0.018

Table 7. **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 6 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. 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	-0.251**	-0.199***	-0.257*	-0.374***	-1.331*	-1.394***
Beta $(t-1)$	[0.106]	[0.075]	[0.148]	[0.107]	[0.684]	[0.457]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67628	67623	67634	67629	67634	67629
Adj. R-squared	0.1012	0.2252	0.101	0.2255	0.1011	0.2253

Panel B: Transmission of extrapolation from solo to team-managed funds						
	No Momentum Stocks		Weight Changes		Past 1 Year Return	
	OLS	**IV**	**OLS**	**IV**	**OLS**	**IV**
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.8793***	1.1708**	0.7657***	0.9021***	0.6469***	0.8733***
	[0.2325]	[0.5086]	[0.0958]	[0.1898]	[0.1215]	[0.3011]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.8477***	-1.0344**	-0.6501***	-0.8052***	-0.3644**	-0.7548***
	[0.2585]	[0.4283]	[0.1079]	[0.1730]	[0.1596]	[0.2827]
D_j^E	0.0728	0.5436	0.0395	0.2083	0.0051	0.0263
	[0.0887]	[0.4279]	[0.0483]	[0.1589]	[0.0165]	[0.0441]
Style fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Team controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	307	307	307	307	307	307
Adj. R-squared	0.1553	0.0734	0.3165	0.2124	0.1896	0.0895
Hypothesis testing:						
p-value $H_0 : \delta_0 = 1$	0.604	0.737	0.015	0.606	0.004	0.674
p-value $H_0 : \delta_0 + \delta_1 = 0$	0.774	0.573	0.025	0.134	0.006	0.377
p-value $H_0 : \delta_0 + \delta_1 = 1$	0.000	0.000	0.000	0.000	0.000	0.000

Table 8. **Robustness – timing solo-managed versus team-managed funds**: This table shows robustness of our main result of Table 6 with respect to the timing of solo-managed versus team-managed funds. In particular, we repeat the OLS regressions of Table 6 on how extrapolative behavior is transmitted from individual team members to teams, but we re-estimate team-level extrapolative behavior over a sample period that is constrained to be closer to the sample period over which we estimate individual behavior. In particular, Column (1) and (2) show that the result that the extrapolation bias is attenuated in teams is confirmed when we restrict the measurement of teams' extrapolative behavior to the period that at least one of the single managers is also managing a fund individually. Column (3) and (4) show that our results hold when we restrict the measurement of teams' extrapolative behavior to the period starting (ending) one year before (after) the period in which we observe the counterfactual team. Column (5) and (6) show the results hold when we begin (end) the estimation of the team-level behavior exactly when the estimation of the counterfactual team begins (ends). The regression is estimated with the same specification and the same controls as Table 6. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	Overlap Team & Single Manager		**Overlap Team & CF -1/+1 year**		**Overlap Team & CF Exact**	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.9464***	0.8937***	0.7010***	0.6345***	0.6810***	0.5936***
	[0.1253]	[0.1254]	[0.1553]	[0.1561]	[0.1529]	[0.1561]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.7040***	-0.6275***	-0.4317**	-0.3513*	-0.4657**	-0.3659*
	[0.1850]	[0.1790]	[0.2036]	[0.2023]	[0.2003]	[0.2002]
D_j^E	0.0676	0.0181	0.1242*	0.0921	0.1283*	0.1031
	[0.0720]	[0.0714]	[0.0723]	[0.0734]	[0.0716]	[0.0729]
Style fixed effects	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes
Observations	280	279	263	263	249	249
Adj. R-squared	0.2322	0.2774	0.153	0.2027	0.1497	0.1954
Hypothesis testing:						
p-value $H_0 : \delta_0 = 1$	0.669	0.397	0.055	0.038	0.020	0.010
p-value $H_0 : \delta_0 + \delta_1 = 0$	0.076	0.043	0.042	0.097	0.030	0.077
p-value $H_0 : \delta_0 + \delta_1 = 1$	0.000	0.000	0.000	0.000	0.000	0.000

Table 9. **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	0.015	-0.040	0.036	0.035	0.830	0.860	0.189	0.179	0.682	0.576
s.e.	0.037	0.042	0.014	0.015	0.029	0.029	0.031	0.032	0.032	0.038
CF - Team		0.054		0.001		-0.031		0.010		0.105
t-stat		1.232		0.127		-1.640		0.433		3.046
Obs.		128		125		124		124		119

Panel B: Contrarian teams

	Extrapolation beta		**Fixed pay**		**Performance pay**		**AUM pay**		**Ownership**	
	CF	Team	CF	Team	CF	Team	CF	Team	CF	Team
Mean	-0.216	-0.175	0.029	0.032	0.815	0.857	0.209	0.165	0.714	0.608
s.e.	0.027	0.063	0.017	0.019	0.040	0.039	0.047	0.043	0.044	0.053
CF - Team		-0.040		-0.003		-0.042		0.044		0.106
t-stat		-0.782		-0.251		-1.273		1.101		2.133
Obs.		63		63		63		63		61

Panel C: Extrapolative teams

	Extrapolation beta		**Fixed pay**		**Performance pay**		**AUM pay**		**Ownership**	
	CF	Team	CF	Team	CF	Team	CF	Team	CF	Team
Mean	0.238	0.092	0.044	0.038	0.842	0.861	0.172	0.197	0.642	0.535
s.e.	0.055	0.051	0.024	0.024	0.043	0.043	0.042	0.049	0.046	0.056
CF - Team		0.146		0.005		-0.019		-0.025		0.107
t-stat		2.112		0.293		-1.137		-1.077		2.162
Obs.		65		61		60		60		57

Table 10. **Robustness – style, workload, and fund families**: In Panel A, we show summary statistics for the style, workload, and fund family measures as specified in Sections 5.2.4 – 5.2.5. In Panels B to D, 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, $R_j = D_j^{SM}$, 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 C, $R_j = \Delta Workload_j$, 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 D, $R_j = D_j^{FS}$, 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

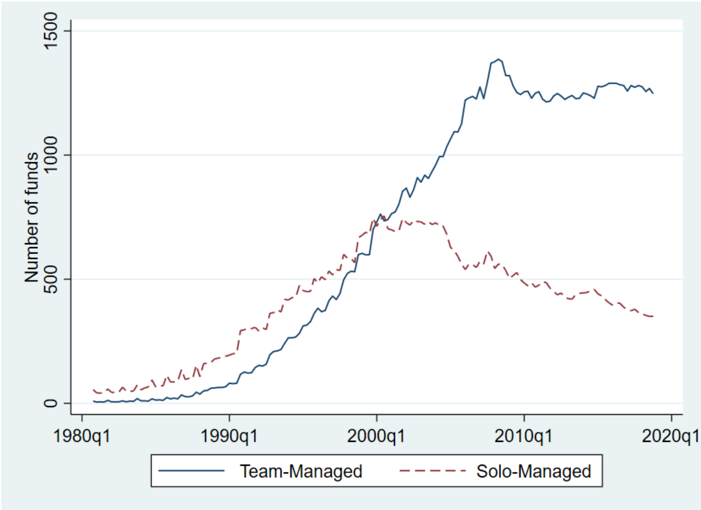
All Teams	Obs.	Mean	St. Dev.	Min	Max
D_j^{SM}	308	0.24	0.43	0	1
$\Delta Workload_j$	308	-0.71	2.28	-23	8
D_j^{FS}	308	0.40	0.49	0	1
Contrarian Teams	Obs.	Mean	St. Dev.	Min	Max
D_j^{SM}	143	0.20	0.40	0	1
$\Delta Workload_j$	143	-0.78	1.93	-12	3
D_j^{FS}	143	0.41	0.49	0	1
Extrapolative Teams	Obs.	Mean	St. Dev.	Min	Max
D_j^{SM}	165	0.27	0.44	0	1
$\Delta Workload_j$	165	-0.65	2.56	-23	8
D_j^{FS}	165	0.39	0.49	0	1

	Panel B: Style Migrations		Panel C: Workload		Panel D: Mutual Fund Family	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.9601***	0.8982***	0.8277***	0.7825***	0.8303***	0.6614***
	[0.1046]	[0.1061]	[0.1578]	[0.1576]	[0.2080]	[0.2074]
$\hat{\beta}_j^{CF} \times D_j^E \times R_j$	0.2146	0.3279	0.0557	0.0253	0.1117	-0.117
	[0.5480]	[0.5478]	[0.1308]	[0.1287]	[0.3369]	[0.3388]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.6769***	-0.6263***	-0.5684***	-0.5193***	-0.6655***	-0.4671*
	[0.1543]	[0.1530]	[0.1948]	[0.1927]	[0.2468]	[0.2481]
$\hat{\beta}_j^{CF} \times R_j$	-0.7110*	-0.7600**	0.0557	-0.065	0.12	0.2525
	[0.3673]	[0.3635]	[0.1308]	[0.1163]	[0.2389]	[0.2391]
$D_j^E \times R_j$	0.0766	0.0744	-0.0829	0.0081	-0.0825	-0.0812
	[0.1474]	[0.1455]	[0.1187]	[0.0319]	[0.1198]	[0.1169]
D_j^E	0.0261	0.0104	0.0068	-0.0058	0.0569	0.0406
	[0.0637]	[0.0642]	[0.0329]	[0.0284]	[0.0775]	[0.0765]
R_j	0.0513	-0.008	-0.01	-0.0389	0.0788	0.1129
	[0.1132]	[0.1136]	[0.0292]	[0.1480]	[0.0888]	[0.0869]
Style fixed effects	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes
Observations	308	307	308	307	308	307
Adj. R-squared	0.285	0.3099	0.2627	0.2929	0.265	0.2974

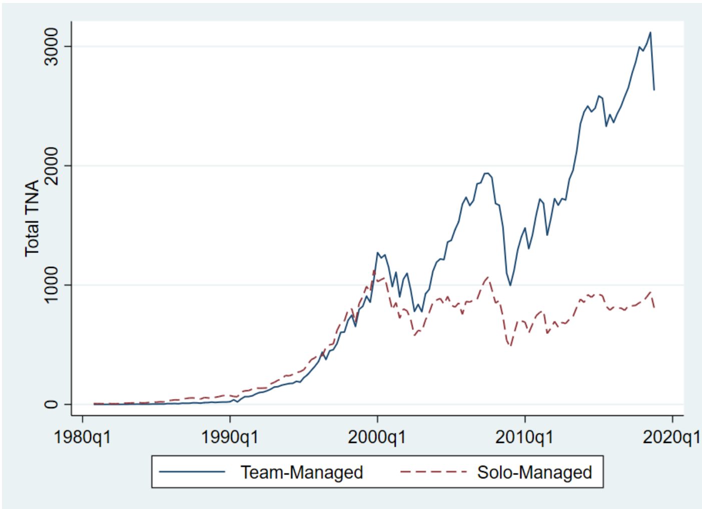
Table 11. **Mechanism – internal reflection or external screening?**: The internal reflection hypothesis and the external screening hypothesis make different predictions concerning how team composition affects the transmission of individual-level behavior to the team. To investigate the role of team 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 exposure to momentum of the team, 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)
$\hat{\beta}_j^{CF}$	0.9111***	0.8635***	0.8721***
	[0.2626]	[0.2534]	[0.2559]
$\hat{\beta}_j^{CF} \times D_j^{AE}$	-0.7060**	-0.6616**	-0.7103***
	[0.2828]	[0.2754]	[0.2732]
$\hat{\beta}_j^{CF} \times D_j^{M}$	-0.5659*	-0.4955	-0.4881*
	[0.3010]	[0.3013]	[0.2931]
D_j^{AE}	0.0648	0.0205	0.0375
	[0.0978]	[0.0986]	[0.0943]
D_j^{M}	0.0156	-0.0159	-0.0267
	[0.0737]	[0.0715]	[0.0735]
Team controls	No	Yes	Yes
Style FE	No	No	Yes
Observations	308	308	308
Adj. R-squared	0.2698	0.2849	0.2884
Hypothesis testing:			
p-value $H_0 : \delta_1 = \delta_2$	0.439	0.3655	0.1547

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

Figure A2. **Manager-level extrapolation: restricted sample versus always solo**: This figure shows the manager-level extrapolation betas (β^X) estimated over the full sample for our main metric defined in Section 2. The distribution of manager-level extrapolation betas is shown for those managers who always operate in solo-managed funds (Always Solo) versus those that are in our restricted sample (Restricted Solo), meaning that they both operate in solo-managed funds as well as team-managed funds during our sample period. The average extrapolation beta across managers that always manage solo equals -0.19 and for the managers that are in our restricted sample the average equals -0.11. A difference in means test shows that the means are statistically indistinguishable from each other (t-stat = 1.14).

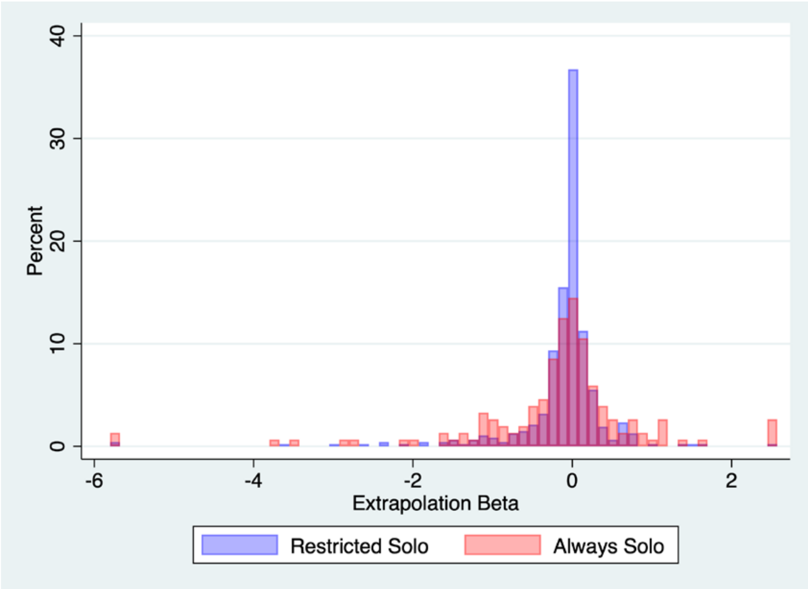


Table A1. **The explanatory power of momentum trading and the disposition effect for extrapolative behavior**: This table estimates regressions of our extrapolation metrics on a fund's momentum trading behavior and on a fund's disposition effect. We measure momentum trading as the loading of a fund's return on the momentum factor (Carhart, 1997). We measure the disposition effect following Odean (1998) and Cici (2012) (see Section IA2.1 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 Changes); 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 Year Return). More details about these metrics are in Section 2.2 and 5.1. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	Main Specification	**No Momentum**	**Weight Changes**	**Past Year Return**
	(1)	(2)	(3)	(4)
β_{MOM}	0.674***	0.583***	0.575***	0.073***
	[0.097]	[0.117]	[0.106]	[0.017]
Disposition	-0.637**	-0.394*	-0.670**	-0.083
	[0.323]	[0.213]	[0.337]	[0.053]
Constant	-0.034*	-0.026**	-0.029	-0.004
	[0.018]	[0.012]	[0.018]	[0.003]
Observations	2056	2056	2056	2056
R-squared	0.0296	0.0289	0.0235	0.0142
Adj. R-squared	0.0286	0.0279	0.0226	0.0132

Table A2. **Spanning tests of extrapolative funds**: This table shows regressions of quarterly returns of a long-short portfolio of extrapolative funds on the Carhart (1997) momentum factor, the Fama and French (2015) 5 factors, and a long-short portfolio of funds that goes long the 20% of funds with the highest momentum factor loading and short the 20% with the lowest momentum factor loading ($\beta_{FundMOM-LS}$). The long-short portfolio of extrapolative funds goes long the 20% most extrapolative funds and short the 20% least extrapolative funds. The factor exposures and the extrapolation metric are measured as in Table 2. Standard errors are in brackets. Significance: ***99%, **95%, *90%.

	Long-Short Extrapolative Funds		
	(1)	(2)	(3)
α	-0.203***	-0.137**	-0.181***
	[0.065]	[0.067]	[0.063]
β_{MOM}	0.064***		0.035**
	[0.010]		[0.015]
$\beta_{FundMOM-LS}$		0.205***	0.122**
		[0.032]	[0.052]
β_{MKT}	0.044***	0.038***	0.042***
	[0.010]	[0.009]	[0.010]
β_{SMB}	-0.061***	-0.044**	-0.049***
	[0.017]	[0.017]	[0.017]
β_{HML}	-0.061***	-0.067***	-0.060***
	[0.020]	[0.019]	[0.018]
β_{RMW}	-0.001	0.050***	0.028
	[0.018]	[0.018]	[0.020]
β_{CMA}	-0.084***	-0.049**	-0.067***
	[0.027]	[0.023]	[0.023]
Observations	114	114	114
Adj. R-squared	0.7241	0.7291	0.7473

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 6: $\hat{\beta}_j^{TM} = \alpha + \hat{\beta}_j^{CF}(\delta_0 + \delta_1 D_j^E) + \delta_2 D_j^E + \delta_3 C_j + \epsilon_j$. 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)
$\hat{\beta}_j^{CF}$	0.4941***	0.8664***	0.5109***	0.8621***	0.4879***	0.8944***
	[0.0640]	[0.1111]	[0.0639]	[0.1051]	[0.0627]	[0.1063]
$\hat{\beta}_j^{CF} \times D_j^E$		-0.6657***		-0.6382***		-0.6762***
		[0.1428]		[0.1402]		[0.1383]
D_j^E		0.0178		-0.0299		-0.0535
		[0.0562]		[0.0575]		[0.0567]
Style fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Team controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	307	307	307	307	307	307
Adj. R-squared	0.2398	0.2895	0.2491	0.2945	0.2419	0.2952
Hypothesis testing:						
p-value $H_0 : \delta_0 = 1$	0.000	0.230	0.000	0.155	0.000	0.322
p-value $H_0 : \delta_0 + \delta_1 = 0$		0.028		0.034		0.016
p-value $H_0 : \delta_0 + \delta_1 = 1$		0.000		0.000		0.000

Table A4. **Robustness – alternative assumptions extrapolation metric**: 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 $t+1$, $D^{inflows}_{j,t+1}$, 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 6 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	-0.315**	-0.278***	-0.205***	-0.183***	-0.365***	-0.358***
Beta $(t-1)$	[0.121]	[0.085]	[0.075]	[0.053]	[0.129]	[0.091]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67634	67629	67634	67629	67634	67610
Adj. R-squared	0.1013	0.2254	0.1013	0.2255	0.1013	0.2255

Panel B: Transmission of extrapolation from solo to team-managed funds						
	Horizon Universe		**No Universe**		**Separate Inflows/Outflows**	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_j^{CF}$	0.5155***	0.4850***	0.5312***	0.5099***	1.2978***	1.1819***
	[0.0770]	[0.0771]	[0.0701]	[0.0703]	[0.1496]	[0.1506]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.2793**	-0.2308**	-0.2516**	-0.2408**	-0.9663***	-0.8581***
	[0.1187]	[0.1154]	[0.1221]	[0.1179]	[0.1838]	[0.1823]
D_j^E	0.1532**	0.0823	0.1772	0.1687	-0.0596	-0.0662
	[0.0707]	[0.0708]	[0.1269]	[0.1243]	[0.0591]	[0.0590]
Style fixed effects	No	Yes	No	Yes	No	Yes
Team controls	No	Yes	No	Yes	No	Yes
Observations	308	307	308	307	307	306
Adj. R-squared	0.2141	0.2616	0.2298	0.2708	0.2608	0.2983
Hypothesis testing:						
p-value $H_0 : \delta_0 = 1$	0.000	0.000	0.000	0.000	0.047	0.228
p-value $H_0 : \delta_0 + \delta_1 = 0$	0.009	0.004	0.006	0.006	0.002	0.002
p-value $H_0 : \delta_0 + \delta_1 = 1$	0.000	0.000	0.000	0.000	0.000	0.000

Table A5. **Discussion – extrapolation and initial buys**: In this table, we estimate our extrapolation metric on initial buys only and repeat the OLS regressions of Table 6 on how extrapolative behavior is transmitted from individual team members to teams. We define initial buys as the stocks that are bought by the managers for the first time. To reduce the impact of noise in our estimates, we construct the counterfactual extrapolation metric $\hat{\beta}_j^{CF}$ by pooling all observations from the single managers that comprise the team and re-estimate our extrapolation metric, rather than taking the average of each individual manager's extrapolation metric within the team. 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 exposure to momentum of the team, the time-series average of the disposition effect of the team, and team's style dummies. Significance: ***99%, **95%, *90%.

	Initial buys			
	(1)	(2)	(3)	(4)
$\hat{\beta}_j^{CF}$	0.1108	0.1132	0.4345	0.5651*
	[0.1113]	[0.1138]	[0.2802]	[0.2882]
$\hat{\beta}_j^{CF} \times D_j^E$			-0.5955*	-0.8074**
			[0.3280]	[0.3413]
D_j^E			0.146	0.1408
			[0.2257]	[0.2342]
Style fixed effects	No	Yes	No	Yes
Team controls	No	Yes	No	Yes
Observations	307	306	307	306
Adj. R-squared	0	-0.0108	0.0087	0.0064
Hypothesis testing:				
p-value $H_0 : \delta_0 = 1$	0.000	0.000	0.045	0.132
p-value $H_0 : \delta_0 + \delta_1 = 0$			0.346	0.185
p-value $H_0 : \delta_0 + \delta_1 = 1$			0.000	0.000

Table A6. **Discussion – 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 Beta $(t-1)$	-0.027	0.005	-0.016***	-0.008***
	[0.029]	[0.027]	[0.004]	[0.003]
Controls	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes
Observations	54951	57958	67634	67634
Adj. R-squared	0.0753	0.0455	0.047	0.0346
	Panel C: Downside Performance		Panel D: NBER Recessions	
	BM Adj. Return	**FF5 Alpha**	**BM Adj. Return**	**FF5 Alpha**
	(1)	(2)	(3)	(4)
Extrapolation Beta $(t-1)$	-0.168	-0.463*	-0.474	-0.599
	[0.373]	[0.248]	[0.640]	[0.424]
Controls	Yes	Yes	Yes	Yes
Time × Style FE	Yes	Yes	Yes	Yes
Observations	11726	11723	5321	5321
Adj. R-squared	0.1242	0.256	0.166	0.3055

Table A7. **Discussion – team-based management and extrapolation for the full sample**: This table reports coefficient estimates from a regression of future changes in extrapolation from time t to $t+8$ on changes in team-based management from time $t-1$ to t at the fund level for our full sample of mutual funds described in Section 2. Since we recursively estimate fund-level extrapolation on a two-year window, the use of a 2-year prediction horizon allows to ensure that the change in extrapolative behavior on the left hand side of the regression reflects the comparison of extrapolative behavior post-managerial shift to teams relative to pre-shift. The main explanatory variable of interest takes a value of zero if there is no change in managerial structure at time t, it takes a value of 1 if there is a shift from individual to team-based management, and a value of -1 otherwise. Controls in the multivariate regression are changes in: 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. Regressions also control for style-quarter fixed effects as indicated. 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 IA2.2. Significance: ***99%, **95%, *90%.

	Δ in Extrapolation t to $t+8$	
	(1)	(2)
Δ Team Management $t-1$ to t	-0.018**	-0.021**
	[0.009]	[0.010]
Δ Expense Ratio $t-1$ to t		0.113*
		[0.063]
Δ Log Fund TNA $t-1$ to t		0.071
		[0.071]
Δ Log Fund TNA^2 $t-1$ to t		-0.003
		[0.006]
Δ Log Fund Age $t-1$ to t		0.035
		[0.042]
Δ Fund Turnover $t-1$ to t		-0.017
		[0.013]
Δ Log N Stocks $t-1$ to t		-0.006**
		[0.003]
Δ Flow $t-1$ to t		0
		[0.000]
Δ Avg. Manager Exp. $t-1$ to t		0
		[0.001]
Δ β_{MOM} $t-1$ to t		-0.213**
		[0.097]
Δ Disposition $t-1$ to t		0.014
		[0.015]
Time $\times$ Style FE	No	Yes
Observations	65277	52439
Adj. R-squared	0.000	0.007

Internet Appendix

Appendix IA1 Preparing the mutual fund dataset

In this appendix, we explain how we merged the CRSP, Morningstar, and Thomson Reuters databases, based on work by (Berk and van Binsbergen, 2015; Pástor et al., 2015; Kim, 2020). 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.

IA1.1 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.

IA1.2 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.

IA1.3 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.

IA1.4 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 IA2 Control Variable Description

IA2.1 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, Gulen, and Schill (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, Hodrick, Xing, and Zhang, 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.

IA2.2 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 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, ($TNA_{j,t-1}$) :

$$flow_{j,t} = \frac{DF_{j,t}}{TNA_{j,t-1}}. \tag{IA1}$$

When estimating how extrapolation affects mutual fund flows, we also control for the fund's CAPM alpha over the past year, because Barber, Huang, and Odean (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:

$$PRG_{j,t} = \frac{RG_{j,t}}{RG_{j,t} + UNRG_{j,t}}, \quad PRL_{j,t} = \frac{RL_{j,t}}{RL_{j,t} + UNRL_{j,t}}, \tag{IA2}$$

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.

Appendix IA3 The cost of extrapolation: Back-of-the-envelope analysis

To measure the impact that extrapolation bias has on managers' wealth accumulation, we will proceed in two steps. First, we are going to rely on insights from Ibert et al. (2018) to measure the impact that extrapolation bias has on a manager's labor income in a given year. Second, we are going to cumulate the documented impact over the career length of a manager.

Ibert et al. (2018) use a sample of Swedish mutual fund managers for whom they can observe labor income, fund revenue, and fund performance in excess of the benchmark. With this data, they estimate in Table 4 of their paper the following relation between log labor income (L), fund revenues (Rev) and fund performance (R^{abn}):

$$\begin{aligned} log(L_t) = 0.13 log(Rev_t) + 0.253 log(1 + R^{abn}_t) + 0.586 log(1 + R^{abn}_{t-1}) \\ + 0.583 log(1 + R^{abn}_{t-2}) + 0.274 log(1 + R^{abn}_{t-3}) \end{aligned} \tag{IA3}$$

For convenience, we write $Rev_t = TNA_{t-1} \times Expense\_Ratio_t$, where, following Ibert et al. (2018), we take revenue to be independent of fund returns, as these returns appear in the model by themselves. Hence, we can rewrite fund revenues as:

$$
\begin{aligned}
Rev_t &= TNA_{t-2}(1+flow_{t-1}) \times Expense\_Ratio_t \\
&= TNA_0 \prod_{k=0}^{t-2}(1+flow_{t-1-k}) \times Expense\_Ratio_t.
\end{aligned} \tag{IA4}
$$

where TNA_0 represents the size of a fund at inception that, as we show below, has no impact on our calculations. Combining Equations IA3 and IA4, we can write log labor income of a manager as:

$$
\begin{gathered}
log(L_t) = \sum_{k=0}^{3} w_k log(1+R^{abn}_{t-k}) + 0.13 \sum_{k=0}^{t-2} log(1+flow_{t-1-k}) + \\
0.13 log(Expense\_Ratio_t) + 0.13 log(TNA_0),
\end{gathered} \tag{IA5}
$$

where w_k represents the coefficient on the performance in year k on log labor estimated in Ibert et al. (2018).

To gauge the impact of extrapolation bias on fund managers' labor income, we need to express fund performance, fund flows, expense ratios, and initial TNA of a fund as a function of extrapolative behavior. Following our evidence of the role of extrapolation for returns and flows:

- We assume a linear relation between extrapolative behavior, i.e., β_X and a fund's abnormal return:

  $$R^{abn}_t = R^{abn}_0 + \beta^X R^{abnX} + \epsilon^R_t,$$

  where we take a manager's extrapolative behavior β^X as constant, we are going to treat the under-performance that results from extrapolation (R^{abnX}) also as a constant, and the error term ϵ^R_t captures all variation in performance that is unrelated to extrapolation.

- Similarly, following our empirical results on the conditioning role of extrapolation for fund flows, we model flows as a linear function of extrapolative behavior:

  $$flow_t = flow_0 + \beta^X flowX + \epsilon^f_t.$$

- We assume that expense ratios and initial assets under management (TNA_0) are independent of extrapolative behavior.

After discarding terms that are independent from extrapolative behavior, we obtain:

$$
log(L_t) = \sum_{k=0}^{3} w_k log(1+\beta^X R^{abnX}) + 0.13 \sum_{k=0}^{t-2} log(1+\beta^X flowX). \tag{IA6}
$$

Since all quantities are in logs, we can interpret the equation above as describing the percentage-change impact on labor income of a given percentage change in abnormal return and flows:

$$\Delta L_t(\%) = \sum_{k=0}^{3} w_k \Delta R^{abn}_{t-k}(\%) + 0.13 \sum_{k=0}^{t-2} \Delta flows_{t-1-k}(\%). \tag{IA7}$$

Having expressed returns and flows as a function of a manager's extrapolative behavior, we can then write changes in labor income in the t^{th} year of the manager's career as:

$$\Delta L_t(\%) = \Delta\beta^X R^{abnX(\%)} \sum_{k=0}^{3} w_k + 0.13\Delta\beta^X flowX(\%) \sum_{k=0}^{t-2} 1. \tag{IA8}$$

Using the estimates of R^{abnX} (-0.355%) and flowX(%) (-3.658%) from our performance analysis of Table 2 and 3, and the equation IA5 based on the estimates in Ibert et al. (2018), we finally obtain:

$$\Delta L_t(\%) = \Delta\beta^X(1.696(-0.355\%)) + 0.13(-3.658\%)(t-1). \tag{IA9}$$

With this equation, we can estimate the loss of income and wealth that a fund manager who extrapolates past returns faces. Table IA1 reports the results. In the table, we assess the losses that are due to extrapolation using a number of statistics. In Panel A, we report the average percentage reduction in yearly salary that is due to extrapolation. Panel B reports the cumulative reduction, expressed relative to the average yearly salary. In other words, the panel reports how many years of salary are foregone due to extrapolative behavior. Panel C and D measure the losses that are due to extrapolation by means of a present-value calculation, whereby losses are discounted to the present using either the historical risk free rate (3.24%) or the market return (9.98%), both from Ken French's website.

The main parameters of the back-of-the-envelope analysis are selected as follows:

- To select the number of years a mutual fund managers operates in the industry, we form an educated guess by estimating the difference between the average age at retirement for college graduates and the age at which college graduates enter the workforce. For the former, we use 66 years, as per statistics from Center for Retirement Research at Boston College. As for the latter, we set the age of college degree to 28. This is a conservative estimate, since bachelor and master-level degrees are often completed earlier. These statistics set at 38 years the length of the career of a mutual fund manager. Of course, a mutual fund manager may have worked in another position before becoming a mutual fund manager. Similarly, the mutual fund manager could decide to retire earlier, or he may decide to work as a manager for a longer period. We consider all these possibilities by analyzing the sensitivity of our results to a choice of a shorter work spell of 30 years (e.g., early retirement or a few years in another position) or a longer one of 45 years (late retirement).

- When selecting the relevant change in extrapolative behavior $\Delta\beta^X$, multiple choices can be contemplated. The first possible choice is to use a one-standard deviation increase in β^X. As we show in Table 1, this increase corresponds to 0.6. The next is to consider the simple difference between the average β^X among extrapolators and contrarians. The left panel of Figure 1 indicates that this difference is between 0.37 and 0.60 depending on whether we use full-sample estimates of extrapolative behavior or a recursive estimate. We choose conservatively, and pick a value of 0.4. Similarly, one can choose the difference between the average β^X in the top and the bottom quintile of β^X. The right panel of Figure 1 indicates that this difference is between 0.78 and 1.23, depending on whether we use full-sample or recursive estimates of β^X. Once again, we make a conservative selection, and pick 0.8. Finally, one can also be interested in a more modest change in extrapolation, and for completeness we include in our analysis also a smaller reduction in β^X of only 0.1.

Other parameters that enter our estimations are:

- The yearly salary of a representative manager who does not extrapolate; we set this to one hundred thousand dollars, and results are fairly insensitive to this dollar amount, since our calculations are based on Ibert et al. (2018), and these in turn concern the growth in salary, rather than the level of the salary itself.[44]

- The discount rate, which affects Panels C and D only. We have experimented with both a risk-free discount rate and the historical market return. Results are reported for both cases.

The results indicate that extrapolation can have a sizable impact on managers' labour income. For brevity we concentrate on the results for a 1-standard deviation increase in β^X. We also focus on a 38-year career length, but results with other values of the career span are similar. We find that extrapolation leads to a 5% reduction in yearly salary on average during the work life of a manager. Cumulated over the entire work life, such a loss equates to more than 2 yearly salaries, relative to what the manager would have earned on average, had he not extrapolated. Such a reduction in yearly labor income corresponds to a 2.7% to 3.9% reduction in the present value of all manager's income, depending on the selected discount rate.

Of course, the opportunity cost of the loss that is due to extrapolation extends beyond the sheer loss of salary. This is because the manager who receives labor income may smooth consumption

[44] We have also experimented with an alternative scenario where we use Ibert et al. (2018) estimates, jointly with average fund returns, average flows, and average salaries increases over the career of a reference manager reflecting nationwide statistics about salary increases with seniority. Results remain qualitatively similar to the ones presented in Table IA1.

by investing a portion of his labor income in financial markets. So, when a manager's income is reduced, he does not only miss the income, but he also gives up the profits of the foregone investment. Figure IA1 quantifies such a foregone wealth in three possible scenarios. In Panel A, we assume a manager would reinvest all his foregone salary into the risk-free security. In Panel B, we assume full reinvestment in the market. Finally, in Panel C we assume reinvestment in the market portfolio, but we cap the reinvestment rate at 50%.

The figure reinforces the message that the losses that are due to extrapolation can be substantial. For brevity we focus on the intermediate case, a career length of 38 years, one standard deviation $\beta^X = 0.60$, and a market reinvestment of 50%. The figure shows that extrapolation causes an asset manager to forego wealth that is close to ten times the yearly manager's income.

Overall, this back-of-the-envelope analysis indicates that extrapolation can have a large impact on an asset manager's financial outcomes.

Appendix IA4 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.

IA4.1 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:

$$r_{s,t} = \gamma_s r_{m,t} + \varepsilon_{s,t}, \tag{IA10}$$

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:

$$trade_{s,i,t+1} = \beta_i^{SM} r_{s,t-4\to t} + e_{s,i,t+1}, \tag{IA11}$$

where β_i^{SM} equals the true extrapolation beta of manager $i = 1, \ldots, N$, $r_{s,t-4\to t}$ the annual past return 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:

$$trade_{s,j,t+1} = \beta_j^{TM} r_{s,t-4\to t} + e_{s,j,t+1}, \tag{IA12}$$

where

$$\beta_j^{TM} = \alpha + \kappa_0 \bar{\beta}_j^{SM} + \upsilon_j, \tag{IA13}$$

$$\bar{\beta}_j^{SM} \equiv \frac{\sum_{i=1|j}^{2} \beta_{i|j}^{SM}}{2}, \tag{IA14}$$

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 (IA13) 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:

$$\beta_j^{TM} = \alpha + \kappa_0 \bar{\beta}_j^{SM} + \kappa_1 \bar{\beta}_j^{SM} \times D_j + \kappa_2 D_j + \upsilon_j, \tag{IA15}$$

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 ($\kappa_0 + \kappa_1$).

IA4.2 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:

$$\hat{\beta}_j^{TM} = c + \delta_0 \hat{\bar{\beta}}_j^{SM} + \epsilon_j. \tag{IA16}$$

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

$$\hat{\beta}_j^{TM} = c + \delta_0 \hat{\bar{\beta}}_j^{SM} + \delta_1 \hat{\bar{\beta}}_j^{SM} \times \hat{D}_j + \delta_2 \hat{D}_j + \epsilon_j. \tag{IA17}$$

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).

IA4.3 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 (IA11), 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, $\bar{\hat{\beta}}_j^{SM,1}$ and $\bar{\hat{\beta}}_j^{SM,2}$. Because we estimate the extrapolation betas on disjoint samples, their measurement errors are uncorrelated. As a result, we can use the extrapolation beta of sample one (two) as instrument for sample two (one). Formally, the first and second stage of the IV method are as follows:

$$\begin{aligned} &\text{1st stage: } \bar{\hat{\beta}}_j^{SM,1} = \alpha + \lambda_0 \bar{\hat{\beta}}_j^{SM,2} + u_j, \\ &\text{2nd stage: } \hat{\beta}_j^{TM} = c + \delta_0 \bar{\hat{\beta}}_j^{SM,1,pred} + \epsilon_j, \end{aligned} \tag{IA18}$$

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 (IA18) 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:

$$\begin{aligned} &\text{1st stage: } \begin{cases} \bar{\hat{\beta}}_j^{SM,1} & = \alpha_1 + \lambda_{1,0} \bar{\hat{\beta}}_j^{SM,2} + \lambda_{1,1} \bar{\hat{\beta}}_j^{SM,2} \times \hat{D}_j^2 + \lambda_{1,2} \hat{D}_j^2 + u_{1,j} \\ \bar{\hat{\beta}}_j^{SM,1} \times \hat{D}_j^1 & = \alpha_2 + \lambda_{2,0} \bar{\hat{\beta}}_j^{SM,2} + \lambda_{2,1} \bar{\hat{\beta}}_j^{SM,2} \times \hat{D}_j^2 + \lambda_{2,2} \hat{D}_j^2 + u_{2,j} \\ \hat{D}_j^1 & = \alpha_3 + \lambda_{3,0} \bar{\hat{\beta}}_j^{SM,2} + \lambda_{3,1} \bar{\hat{\beta}}_j^{SM,2} \times \hat{D}_j^2 + \lambda_{3,2} \hat{D}_j^2 + u_{3,j} \end{cases} \\ &\text{2nd stage: } \hat{\beta}_j^{TM} = c + \delta_0 \bar{\hat{\beta}}_j^{SM,1,pred} + \delta_1 \bar{\hat{\beta}}_j^{SM,1,pred} \times \hat{D}_j^{1,pred} + \delta_2 \hat{D}_j^{1,pred} + \epsilon_j. \end{aligned} \tag{IA19}$$

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.

IA4.4 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 (IA13). The results are depicted in Panel A of Table IA2 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 IA2, 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.[45] When simulating the transmission coefficient under the null $\kappa_1 = 0$, Panel B of Table IA2 shows that we do not falsely reject the null using both the OLS and IV estimators.

[Place Appendix Table IA2 about here]

IA4.5 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 2,000 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 6.

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

In Figure IA2, we show the distribution of the coefficient on $\hat{\beta}_j^{CF}$ for the 2,000 draws where we exclude draws that produce weak instruments.[46] 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 0.80 and a standard deviation of 0.28.

[Place Appendix Figure IA2-IA3 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 6 for multiple draws. In Figure IA3, we find that the distribution of the transmission coefficients 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 1.01 with a standard deviation of 0.47. 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 0.48 with a standard deviation of 0.25. 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.

Appendix IA5 Entering as a solo versus team-based asset manager

We argue that the learning argument described in Section 5.2.1 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 solo precedes team management for all members of the team, whereas 54 belong to the case in which team precedes solo management.[47] 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:

[46] 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.

[47] For the remaining teams, some members first operate solo and then move on to teams, whereas the opposite occurs for the other members.

$$
\begin{aligned}
\hat{\beta}_j^{TM} &= \alpha + \delta_0\hat{\beta}_j^{CF} + \delta_1\hat{\beta}_j^{CF} \times D_j^E \times D_j^l + \delta_2\hat{\beta}_j^{CF} \times D_j^E + \delta_3\hat{\beta}_j^{CF} \times D_j^l \\
&+ \delta_4 D_j^E \times D_j^l + \delta_5 D_j^E + \delta_6 D_j^l + \delta_7 C_j + \epsilon_j, \qquad l = ES, ET. \qquad \text{(IA20)}
\end{aligned}
$$

The coefficient δ_1 on the double interaction term is the main coefficient of interest. When $l = ES$, 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 when $l = ET$ 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 a fund manager learns that extrapolation is harmful while working in a team, and later moves 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 B of Table IA5 reports the results of the estimation when $l = ES$ and Panel C when $l = ET$. 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, whereas learning implies a negative δ_1 in Panel C. Overall, these results do not support learning as a plausible explanation for our finding.

Appendix IA6 Alternative Proxy Workload

As an alternative proxy for workload, we use 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.[48] 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

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

in that stock 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:

$$Workload_{i,t} = \sum_{j \in S_{i,t}} U_{j,t} + \sum_{j \in C_{i,t}} \max \left[\frac{U_{j,t}}{NumManagers_{j,t}} - Overlap_{i,j,t}, 0 \right], \tag{IA21}$$

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 $\Delta Workload_j$.

Table IA6 summarizes the results and shows that δ_1 is statistically indistinguishable from zero. Hence, also based on this alternative measure to proxy for workload we conclude that a change in workload in team-managed versus solo-managed funds cannot rationalize our findings.

Figure IA1. **Extrapolation and Foregone Wealth Accumulation with Reinvestment**: This figure shows the foregone wealth accumulation due to extrapolation, after accounting for the missed reinvestment opportunities. In every year during the work life of a manager, we measure the manager's salary loss due to extrapolation. We then assume the manager would have reinvested this portion of salary either in full (Panel A and C), or only in part (Panel B, 50% reinvestment rate). We contemplate either the reinvestment at the risk-free rate (Panel A), or the reinvestment in the market portfolio (Panel B and C). Each panel reports the cumulative end-of-work-life value of the investment losses that are due to extrapolation, in multiples of average yearly salary.

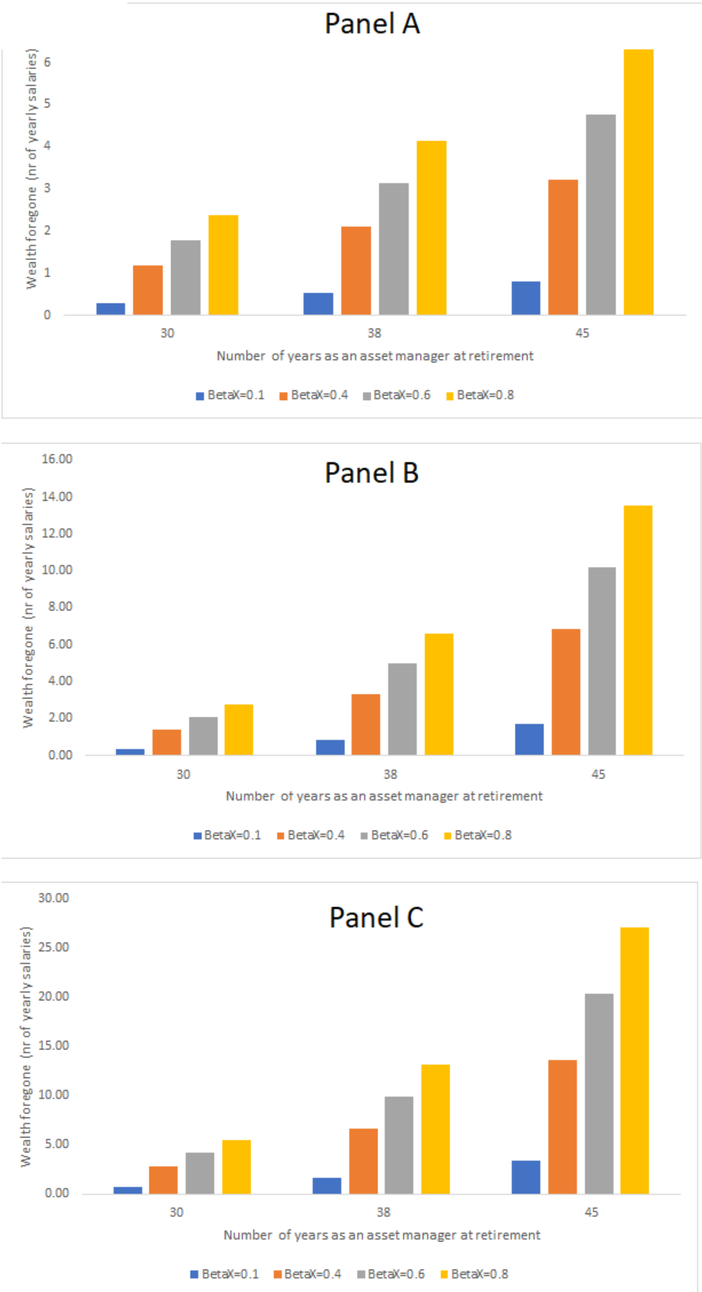


Figure IA2. **IV — empirical simulations**: This figure shows the distribution of transmission coefficients (δ_0) that we obtain for different random samples using our IV methodology based on Equation (6), excluding the interaction term. The mean of the transmission coefficient equals 0.80 with a standard deviation of 0.28. The transmission coefficient estimated in the main specification in Column 6 of Table 6 equals 0.85.

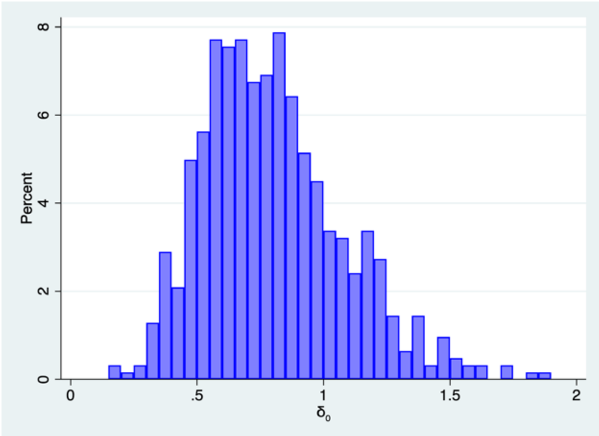
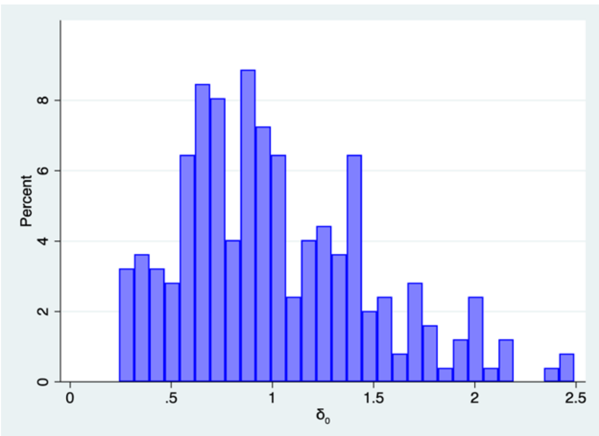
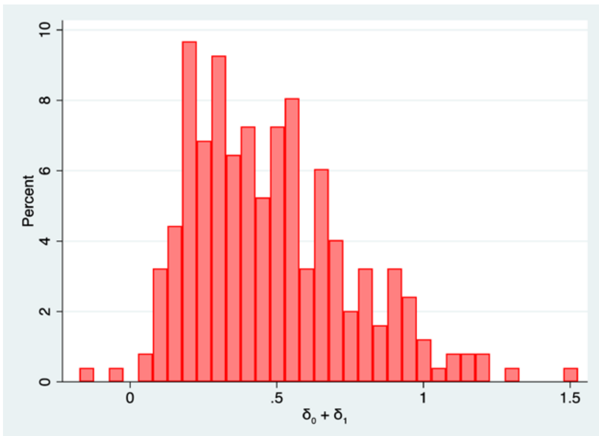


Figure IA3. **IV empirical simulations — contrarians versus extrapolators**: This figure shows the distribution of transmission coefficients for contrarians (δ_0) and extrapolators ($\delta_0 + \delta_1$) that we obtain for different random samples using our IV methodology based on Equation (6). The average transmission coefficient for contrarians equals 1.01 with a standard deviation of 0.47 and for extrapolators the average transmission coefficient equals 0.48 with a standard deviation of 0.25. The transmission coefficient estimated in the main specification in Column 8 of Table 6 equals 1.17 for contrarians and 0.50 for extrapolators.



(a) Contrarians



(b) Extrapolators

Table IA1. **Back of the Envelope**: This table shows back-of-the-envelope statistics concerning the impact of extrapolation on fund returns and flows. More details are in Section IA3.

Panel A: Average yearly reduction in labour income

	Increase in Extrapolation			
Years of work	0.1	0.4	0.6	0.8
30	0.72%	2.86%	4.26%	5.65%
38	0.90%	3.57%	5.31%	7.02%
45	1.06%	4.19%	6.21%	8.20%

Panel B: Cumulative reduction in labour income relative to yearly salary

Years of work	Increase in Extrapolation			
	0.1	0.4	0.6	0.8
30	0.22	0.86	1.32	1.78
38	0.34	1.37	2.09	2.80
45	0.48	1.90	2.89	3.88

Panel C: PV cumulative reduction in labour income as a fraction of PV total income (risk-free rate)

Years of work	Increase in Extrapolation			
	0.1	0.4	0.6	0.8
30	0.49%	1.96%	2.93%	3.88%
38	0.67%	2.67%	3.97%	5.26%
45	0.82%	3.25%	4.84%	6.40%

Panel D: PV cumulative reduction in labour income as a fraction of PV total income (market rate)

Years of work	Increase in Extrapolation			
	0.1	0.4	0.6	0.8
30	0.41%	1.62%	2.43%	3.22%
38	0.46%	1.81%	2.71%	3.60%
45	0.48%	1.91%	2.86%	3.79%

Table IA2. **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

Table IA3. **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	0.32	1																					
Extrapolation Beta (No Mom)	0.73	0.42	1																				
Extrapolation Beta (Weight Change)	0.97	0.30	0.69	1																			
Extrapolation Beta (Past Year Ret)	0.90	0.31	0.58	0.87	1																		
Fund TNA	-0.02	-0.06	-0.02	-0.03	-0.03	1																	
Team Managed	-0.03	0.06	-0.02	-0.03	-0.02	-0.03	1																
Number of Managers	-0.02	-0.03	-0.02	-0.02	-0.02	0.08	0.51	1															
Manager Experience	-0.03	-0.08	-0.05	-0.03	-0.03	0.09	-0.05	-0.01	1														
Number of Stocks	0.01	0.03	0.02	0.01	0.01	0.08	0.08	0.22	-0.09	1													
Fund Age	-0.01	-0.06	-0.03	-0.01	-0.02	0.29	-0.11	-0.05	0.02	-0.08	1												
Expense Ratio	0.00	-0.02	0.01	0.00	0.01	-0.22	-0.08	-0.13	-0.04	-0.25	-0.11	1											
Fund Turnover	0.08	0.17	0.05	0.08	0.06	-0.14	-0.08	-0.04	-0.18	-0.07	-0.12	0.25	1										
β_{MOM}	0.15	0.35	0.16	0.13	0.10	-0.05	-0.02	-0.03	-0.07	0.02	-0.07	0.05	0.42	1									
Disposition	-0.13	-0.32	-0.13	-0.12	-0.09	0.05	0.13	0.11	0.06	0.02	-0.02	-0.09	-0.40	-0.60	1								
Fund Return	-0.03	-0.01	-0.01	-0.03	-0.01	0.08	0.00	-0.02	0.18	0.08	0.08	-0.07	-0.08	-0.06	0.07	1							
Benchmark Adj. Return	-0.06	-0.06	-0.05	-0.07	-0.05	0.13	-0.06	-0.03	0.07	0.00	0.06	-0.01	-0.08	0.01	0.05	0.35	1						
CAPM Alpha	0.00	-0.02	0.00	-0.01	0.00	0.11	-0.05	-0.06	0.01	-0.06	0.08	-0.02	-0.06	0.09	-0.02	0.30	0.64	1					
FF3 Alpha	0.01	0.02	0.00	0.00	0.01	0.12	-0.05	-0.05	0.08	-0.02	0.07	-0.07	-0.13	0.08	-0.01	0.32	0.64	0.66	1				
FF5 Alpha	0.01	0.06	0.02	0.01	0.02	0.10	-0.07	-0.05	0.06	-0.04	0.06	0.02	0.00	0.14	-0.10	0.29	0.57	0.56	0.89	1			
Flow	-0.03	0.01	-0.03	-0.04	-0.01	0.04	0.01	-0.04	0.01	0.02	-0.09	-0.01	-0.02	-0.05	0.04	0.11	0.18	0.13	0.21	0.18	1		
Benchmark Adj. Volatility	0.00	0.00	-0.02	0.00	0.02	-0.03	-0.05	-0.08	0.02	-0.11	0.02	0.19	0.10	0.10	-0.02	0.05	0.14	0.23	0.02	0.15	-0.03	1	
FF5 Alpha Volatility	0.00	0.01	-0.02	-0.01	0.01	-0.03	-0.03	-0.06	0.03	-0.10	0.02	0.13	0.08	0.12	-0.02	0.08	0.14	0.24	0.03	0.13	-0.04	0.9452	1

Table IA4. **Detailed summary statistics extrapolation bias in solo-managed funds versus team-managed funds**: This table compares the extrapolation beta of the team, $\hat{\beta}_j^{TM}$, with its statistical counterfactual $\hat{\beta}_j^{CF}$, i.e., the average level of extrapolation observed among team members when they manage a fund alone. We report the results for all teams combined (Panel A), for contrarian teams (Panel B), and for extrapolative teams (Panel C). A team consists mainly of contrarians if $\hat{\beta}_j^{CF} \leq 0$ and of extrapolators if $\hat{\beta}_j^{CF} > 0$. We report the mean, standard error of the mean (s.e.), the corresponding t-stat, the total number of teams that belong to each group (N), and the median number of observations that are used to estimate the respective extrapolation beta (N^X) of each group.

Panel A: All Teams

	Mean	s.e.	t-stat	N	N^X
$\hat{\beta}_j^{CF}$	-0.011	0.022	-0.489	308	7773
$\hat{\beta}_j^{TM}$	-0.034	0.028	-1.200	308	3898
Difference $\hat{\beta}_j^{CF}$ - $\hat{\beta}_j^{TM}$	0.023	0.026	0.891		

Panel B: Contrarian Teams

	Mean	s.e.	t-stat	N	N^X
$\hat{\beta}_j^{CF}$	-0.224	0.029	-7.593	143	7506
$\hat{\beta}_j^{TM}$	-0.178	0.043	-4.127	143	3825
Difference $\hat{\beta}_j^{CF}$ - $\hat{\beta}_j^{TM}$	-0.046	0.034	-1.374		

Panel C: Extrapolative Teams

	Mean	s.e.	t-stat	N	N^X
$\hat{\beta}_j^{CF}$	0.184	0.024	7.815	165	7916
$\hat{\beta}_j^{TM}$	0.090	0.035	2.589	165	4008
Difference $\hat{\beta}_j^{CF}$ - $\hat{\beta}_j^{TM}$	0.094	0.039	2.414		

Table IA5. **Robustness – entering as a solo versus team-based asset manager**: In Panel A, we show summary statistics for the entry measures as specified in Section IA5. In Panels B and C, 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, $R_j = D_j^{ES}$, 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 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
D_j^{ES}	308	0.21	0.41	0	1
D_j^{ET}	308	0.18	0.38	0	1
Contrarian Teams	Obs.	Mean	St. Dev.	Min	Max
D_j^{ES}	143	0.21	0.41	0	1
D_j^{ET}	143	0.14	0.35	0	1
Extrapolative Teams	Obs.	Mean	St. Dev.	Min	Max
D_j^{ES}	165	0.21	0.41	0	1
D_j^{ET}	165	0.21	0.41	0	1

	Panel B: Enter Single		Panel C: Enter Team	
	(1)	(2)	(3)	(4)
$\hat{\beta}_j^{CF}$	0.9742***	0.9181***	0.9405***	0.8578***
	[0.1053]	[0.1068]	[0.1159]	[0.1166]
$\hat{\beta}_j^{CF} \times D_j^E \times R_j$	1.0816*	0.7937	0.3102	0.1992
	[0.6227]	[0.6180]	[0.3569]	[0.3518]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.7489***	-0.6709***	-0.7379***	-0.6446***
	[0.1538]	[0.1529]	[0.1717]	[0.1705]
$\hat{\beta}_j^{CF} \times R_j$	-0.8292**	-0.9372***	-0.1319	-0.0591
	[0.3553]	[0.3490]	[0.2434]	[0.2360]
$D_j^E \times R_j$	-0.0467	0.1042	-0.0115	-0.0158
	[0.1488]	[0.1483]	[0.1572]	[0.1552]
D_j^E	0.0463	0.0036	0.0257	0.0015
	[0.0633]	[0.0644]	[0.0626]	[0.0619]
R_j	0.0066	-0.0683	-0.07	-0.0705
	[0.1095]	[0.1085]	[0.1212]	[0.1197]
Style fixed effects	No	Yes	No	Yes
Team controls	No	Yes	No	Yes
Observations	308	307	308	307
Adj. R-squared	0.282	0.312	0.2628	0.2931

Table IA6. **Robustness – Stocks overseen as proxy for workload**: In this table we perform the same analysis as in Table 10 for our workload measure $Workload_j$: $\hat{\beta}_j^{TM} = \alpha + \delta_0 \hat{\beta}_j^{CF} + \delta_1 \hat{\beta}_j^{CF} \times D_j^E \times Workload_j + (...) + \delta_7 C_j + \epsilon_j$, where we measure the change in workload as the difference in workload faced by managers of team j when managing as part of that team, and the workload these same managers face when managing a fund alone. Workload is proxied by the total number of stocks that are overseen by the managers when they work in teams versus when they work individually, see Internet Appendix IA6 for details. The main coefficient of interest is δ_1, which measures if the attenuation of extrapolation bias is stronger or weaker for teams whose members experience a larger difference in workload when they manage as part of a team versus in their solo-managed funds. 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%.

	Workload	
	(1)	(2)
$\hat{\beta}_j^{CF}$	0.9576***	0.8955***
	[0.1000]	[0.1024]
$\hat{\beta}_j^{CF} \times D_j^E \times Workload_j$	0.0089**	0.0069
	[0.0038]	[0.0047]
$\hat{\beta}_j^{CF} \times D_j^E$	-0.7848***	-0.7242***
	[0.1625]	[0.1619]
$\hat{\beta}_j^{CF} \times Workload_j$	-0.0113***	-0.0092***
	[0.0029]	[0.0029]
$D_j^E \times Workload_j$	0.0006	0.0003
	[0.0006]	[0.0006]
D_j^E	-0.0138	-0.0333
	[0.0602]	[0.0608]
$Workload_j$	-0.0005	-0.0003
	[0.0005]	[0.0005]
Style fixed effects	No	Yes
Team controls	No	Yes
Observations	308	307
Adj. R-squared	0.2627	0.2929

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