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Can Mutual Fund "Stars" Really Pick Stocks? New Evidence from a Bootstrap Analysis

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ABSTRACT

We apply a new bootstrap statistical technique to examine the performance of the U.S. open-end, domestic equity mutual fund industry over the 1975 to 2002 period. A bootstrap approach is necessary because the cross section of mutual fund alphas has a complex nonnormal distribution due to heterogeneous risk-taking by funds as well as nonnormalities in individual fund alpha distributions. Our bootstrap approach uncovers findings that differ from many past studies. Specifically, we find that a sizable minority of managers pick stocks well enough to more than cover their costs. Moreover, the superior alphas of these managers *persist*.

WAS PETER LYNCH, FORMER MANAGER of the Fidelity Magellan fund, a "star" stock-picker, or was he simply endowed with stellar luck? The popular press seems to assume that Mr. Lynch's fund performed well due to his unusual acumen in identifying underpriced stocks. In addition, Marcus (1990) concludes that the prolonged superior performance of the Magellan fund is difficult to explain as a purely random outcome, that is, where Mr. Lynch and the other Magellan managers have no true stockpicking skills and are merely the luckiest of a large group of fund managers. More recently, the Schroder Ultra Fund topped the field of 6,000 funds (across all investment objective categories) with a return of 107% per year over the 3 years ending in 2001. This fund closed to new investors in 1998 due to overwhelming demand, presumably because investors credited the fund manager with having extraordinary skills.

Recent research documents that subgroups of fund managers have superior stock-picking skills, even though most prior studies conclude that the average

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mutual fund underperforms its benchmarks, net of costs.[1] For example, Chen, Jegadeesh, and Wermers (2000) examine the stockholdings and active trades of mutual funds and find that growth-oriented funds have unique skills in identifying underpriced large-capitalization growth stocks. Furthermore, Wermers (2000) finds that high-turnover mutual funds hold stocks that substantially beat the Standard and Poor's 500 index over the 1975 to 1994 period.

The apparent superior performance of a small group of funds such as Magellan or the Schroder Ultra Fund raises the question of whether such performance is credible evidence of genuine stock-picking skills, or whether it simply reflects the extraordinary luck of a few individual fund managers. Given that hundreds of new funds are launched every year, and by January 2005, over 4,500 equity mutual funds existed in the United States (holding assets valued at almost $4.4 trillion), it is natural to expect that some funds will outperform market indexes by a large amount simply by chance. However, past studies of mutual fund performance do not explicitly recognize and model the role of luck in performance outcomes. Indeed, to a large extent, the literature on performance persistence focuses on measuring out-of-sample performance to control for luck. These models discount the possibility that luck can also persist.

This paper conducts the first comprehensive examination of mutual fund performance ("alpha") that explicitly controls for luck without imposing an ex ante parametric distribution from which fund returns are assumed to be drawn. Specifically, we apply several different bootstrap approaches to analyze the significance of the alphas of extreme funds, that is, funds with large, positive estimated alphas. In employing the bootstrap to analyze the significance of alpha estimates, we explicitly model and control for the expected idiosyncratic variation in mutual fund returns. As Horowitz (2003) emphasizes, in Monte Carlo experiments the bootstrap can "spectacularly" reduce the difference between true and nominal probabilities of correctly rejecting a given null hypothesis (in our context, that no superior fund managers exist). Furthermore, given the intractability of parametrically modeling the joint distribution of mutual fund alphas across several hundred funds, most of which are very sparsely overlapping, the bootstrap is a very attractive approach to use in analyzing a cross section of ranked mutual funds.

The questions this paper asks can be stated very simply: With mutual fund alphas that deviate significantly from normality, how many funds from a large group would we expect to exhibit high alphas simply due to luck, and how does this figure compare to the number we actually observe? To address these questions, we apply our bootstrap technique to the monthly net returns of the universe of U.S. open-end, domestic equity funds during the 1975 to 2002 period—one of the largest panels of fund returns ever analyzed. Across a wide array of performance measurement models, our bootstrap tests consistently indicate that the large positive alphas of the top 10% of funds, net of costs, are extremely unlikely to arise solely due to sampling variability (luck). This result obtains

[1] For evidence of the underperformance of the average mutual fund on a style-adjusted basis, see, for example, Carhart (1997).

when we apply the bootstrap to the cross-sectional distribution of fund alphas, when we apply it to the cross-sectional distribution of the *t-statistics* of fund alphas, as well as when we apply it under several extensions that we employ.[2] Our tests also show strong evidence of mutual funds with negative and significant alphas, controlling for luck.

To illustrate the focus of our paper, suppose that we are told that a particular fund has an alpha of 10% per year over a 5-year period. *Prima facie*, this is an extremely impressive performance record. However, if this fund is the best performer among a group of 1,000 funds, then its alpha may not appear to be so impressive. Furthermore, when outlier funds are selected from such an ex post ranking of a large cross section, the separation of luck from skill becomes extremely sensitive to the assumed joint distribution from which the fund alphas are drawn. Any such analysis must account for nonnormality in this distribution, which, as we will show, can result from (1) heterogenous risk-taking across funds and (2) nonnormally distributed individual fund alphas.

To address the aforementioned questions, we apply the bootstrap to the monthly returns of the 1,788 mutual funds in our sample that exist for at least 5 years. The results show that, by luck alone, nine funds would be expected to exhibit an estimated alpha greater than 10% per year (net of costs) over (at least) a 5-year period. In reality, 29 funds exceed this level of alpha. As our analysis will show, this is sufficient, statistically, to provide overwhelming evidence that some fund managers have superior talent in picking stocks. Overall, our results provide compelling evidence that, net of all expenses and costs (except load charges and taxes), the superior alphas of star mutual fund managers survive and are not an artifact of luck. The key to our study is the bootstrap analysis that allows us to precisely separate luck from skill in the complicated nonnormal cross section of ranked mutual fund alphas.

The above-mentioned bootstrap results reflect net-of-cost performance. When we repeat our tests at the pre-cost level, ranking domestic equity mutual funds with the stockholdings-level performance measure of Daniel et al. (1997), we find that highly ranked funds exhibit levels of pre-cost performance that are similar, but slightly higher than their net-of-cost performance; however, almost all low-ranked funds exhibit insignificant pre-cost performance. Thus, much of the aforementioned variation in the cross section of highly ranked fund net-of-cost performance is due to differences in skill in choosing stocks, not to differences in expenses or trade costs. However, variation across low-ranked funds is mainly due to differences in costs, as these managers exhibit no skills. These findings are noteworthy in that they suggest that active management skills generate not only superior cost efficiencies but also superior fund performance. That is, while most funds cannot compensate for their expenses and trade costs, a subgroup of funds exhibits stock-picking skills that more than compensate for such costs.

[2] Specifically, these extensions show that our results are not sensitive to a potential omitted factor from our models, or to possible cross-sectional correlations in idiosyncratic returns among mutual funds (since they may hold similar stocks due, perhaps, to herding behavior).

Further bootstrap results indicate that superior after-cost performance holds mainly for growth-oriented funds. This result supports prior evidence at the stockholdings level that indicates that the average manager of a growth-oriented fund can pick stocks that beat his benchmarks, while the average manager of an income-oriented fund cannot (Chen, Jegadeesh, and Wermers (2000)). Our findings indicate that even seemingly well-performing income fund managers are merely lucky. In addition, we find stronger evidence of superior fund management during the first half of our sample period; after 1990, we must look further to the extreme right tail of the alpha distribution to find superior managers (i.e., managers who are not simply lucky). Thus, the huge growth in new funds over the past decade has apparently been driven by a growth in the number of active managers without talent, who often appear in the right tail by chance alone.[3]

One may wonder whether our results are of economic significance, since our main evidence of superior performance occurs in the top 10% of alpha-ranked funds. For example, if the above bootstrap-based evidence of skill corresponds largely to small mutual funds, then our results may not have significant implications for the average investor in actively managed mutual funds. To address this issue, we measure economic impact by examining the difference in value-added between all funds that exhibit a certain alpha (including lucky and skilled funds) and those funds that exhibit the same alpha by luck alone; this difference measures skill-based value-added. We estimate that active management by funds that exceed an alpha of 4% per year (through skill alone) generates about $1.2 billion per year in wealth, that is, in excess of benchmark returns, expenses, and trading costs. Thus, subgroups of funds that possess skill add significantly to the wealth of fund shareholders. Note that a similar computation shows that underperforming mutual funds destroy at least $1.5 billion per year in investor wealth.

Perhaps the strongest motivation for employing the bootstrap is illustrated by our tests of performance persistence, since most investors look at past performance to infer the future. Here, we focus on reconstructing tests of persistence similar to those used by Carhart (1997), while applying the bootstrap instead of the standard parametric t-tests used by Carhart and others. Notably, our findings overturn some important and widely cited results from Carhart's paper. Specifically, we find significant persistence in net return alphas (using bootstrapped p-values) for the top decile (and, sometimes, top two deciles) of managers, using several different past alpha ranking periods.

[3] Our bootstrap results also provide useful guidelines for investors. For example, the bootstrap indicates that, among the subgroup of fund managers that have an estimated alpha exceeding 7% per year over a 5-year (or longer) period, half have stock-picking talent, while the other half are simply lucky. This information is also useful as inputs to Bayesian models of performance evaluation. For example, Pastor and Stambaugh (2002) and Baks, Metrick, and Wachter (2001) show how prior views about manager skill combine with sample information in the investment decision. Our results provide a reasonable starting point in forming prior beliefs about manager skills for future tests of U.S. domestic equity mutual fund performance.

Why does the cross-sectional bootstrap yield a substantially different inference regarding high- and low-alpha funds, relative to the often-used p-values of individual fund alphas in these regions? First, our bootstrap formally models the cross-sectional nature of ex post sorts of funds by building the empirical joint distribution of their alphas. This helps in capturing unknown forms of heteroskedasticity and cross-fund correlations in returns. Further, and as might be expected, higher moments play an important role in the explanation. Indeed, we reject normality for about half of our individual fund alphas, and this translates to nonnormalities in the cross section of fund alphas. However, and perhaps more surprisingly, we also find that clustering in idiosyncratic risk-taking among funds also induces important nonnormalities in the cross section of alphas. Specifically, we find large cross-sectional variations in the idiosyncratic risks funds take, with clusters of high- and low-risk funds, perhaps due to the risk-shifting of Chevalier and Ellison (1997). In cases in which risk-taking varies substantially, such as among the high-alpha growth funds or the top Carhart-ranked funds mentioned above, the empirical distribution (bootstrap) uncovers thinner tails in the cross section of fund alphas than that imposed by assuming a parametric normal distribution because of the resulting mixture of individual fund alpha distributions with heterogeneous variances. That is, the presence of clusters of high-risk and low-risk mutual funds (even when individual fund alphas are normally distributed) can create a cross-sectional distribution of alphas that has thin tails relative to a normal distribution, leading to underrejection of the null of no performance in the absence of the bootstrap. In other cases, such as the high-alpha income-oriented funds also noted above, less clustered (more uniformly distributed) risk-taking across funds obtains. The resulting cross section of alphas exhibits thick tails, leading to overrejection of the null (without the bootstrap). Thus, heterogeneous risk-taking among funds, as well as higher moments in individual fund alphas, may induce thin- or thick-tailed cross-sectional alpha distributions, or even tails that are thinner at some percentile points, and thicker at others.

It is important to note that ranking funds by their alpha t-statistic controls for differences in risk-taking across funds. That is, the cross section of fund t-statistics remains normally distributed in the presence of funds with normally distributed alphas, but with different levels of idiosyncratic risk. Nevertheless, the higher moments that we find in individual fund alphas (i.e., skewness and kurtosis) result in a cross section of t-statistics that remains distinctly nonnormal. Indeed, we find that the bootstrap remains crucial in inference tests when analyzing the cross section of t-statistics. Thus, our bootstrap provides improved inference in identifying funds with significant skills because of (1) differential risk-taking among funds and (2) nonnormalities in individual fund alphas.

In summary, when we account for the complex distribution of cross-sectional alphas, we confirm that superior managers with persistent talent do exist among growth-oriented funds, and that the bootstrap is crucial to uncovering the significance of such talent. Our results are also interesting in light of the model of Berk and Green (2004), who predict that net return persistence is

competed away by fund inflows (since managers likely have decreasing returns-to-scale in their talents). Our evidence of persistence in the top decile of funds indicates that such a reversion to the mean in performance is somewhat slow to occur.

Our paper proceeds as follows. Section I describes our bootstrapping procedure, while Section II describes the mutual fund database that we use in our study. Section III provides empirical results, the robustness of which we explore further in Section IV. Section V examines performance persistence using the bootstrap, and Section VI concludes.

I. Bootstrap Evaluation of Fund Alphas

A. *Rationale for the Bootstrap Approach*

We apply a bootstrap procedure to evaluate the performance of open-end domestic equity mutual funds in the United States. In this setting, there are many reasons why the bootstrap is necessary for proper inference. These include the propensity of individual funds to exhibit nonnormally distributed returns, as well as the cross section of funds representing a complex mixture of these individual fund distributions. We begin by discussing individual funds. We then progress to the central focus of this paper, that is, evaluating the cross-sectional distribution of ranked mutual fund alphas, which involves evaluating a complex mixture of individual fund alpha distributions.

A.1. Individual Mutual Fund Alphas

As we describe in Section III.A of this paper, roughly half of our funds have alphas that are drawn from a distinctly nonnormal distribution. These nonnormalities arise for several reasons. First, individual stocks within the typical mutual fund portfolio realize returns with nonnegligible higher moments. Thus, while the central limit theorem implies that an equal-weighted portfolio of such nonnormally distributed stocks will approach normality, managers often hold heavy positions in relatively few stocks or industries. Second, market benchmark returns may be nonnormal, and co-skewness in benchmark and individual stock returns may obtain. In addition, individual stocks exhibit varying levels of time-series autocorrelations in returns. Finally, funds may implement dynamic strategies that involve changing their levels of risk-taking when the risk of the overall market portfolio changes, or in response to their performance ranking relative to similar funds. Thus, because each of these regularities can contribute to nonnormally distributed mutual fund alphas, normality may be a poor approximation in practice, even for a fairly large mutual fund portfolio.

The bootstrap can substantially improve on this approximation, as Bickel and Freedman (1984) and Hall (1986) show. For example, by recognizing the presence of thick tails in individual fund returns, the bootstrap often rejects abnormal performance for fewer mutual funds; we return to this finding in Section III.B.

A.2. The Cross Section of Mutual Fund Alphas

While the intuition about non-normalities in individual fund alphas above is helpful, such intuition does not necessarily carry over to the cross section of mutual funds. Specifically, the cross-sectional distribution of alphas also carries the effect of variation in risk-taking (as well as sample size) across funds.[4] Furthermore, cross-sectional correlations in residual (fund-specific) risk, although very close to zero on average, may be nonzero in the tails if some funds load on similar nonpriced factors. These effects tend to be important because high-risk funds often hold concentrated portfolios that load on similar industries or individual stocks.

That is, while fund-level nonnormalities in alphas imply nonnormalities in the cross-sectional distribution of alphas, the reverse need not be true. Even funds that have normally distributed residuals can create nonnormalities in the cross section of ranked alphas. To illustrate, consider 1,000 mutual funds, each existing over 336 months (the time span of our sample period). Suppose each fund has independently and identically distributed (IID) standard normal model residuals, and that the true model intercept (alpha) equals zero for each fund. Thus, measured fund alphas are simply the average realized residual over the 336 months. In this simple case, the cross-sectional distribution of fund alphas would be normally distributed.

However, consider the same 1,000 funds with heterogeneous levels of risk such that, across funds, residual variances range uniformly between 0.5 and 1.5 (i.e., the average variance is unity). In this case, the tails of the cross-sectional distribution of alphas are now fatter than those of a normal distribution. The intuition here is clear: As we move further to the right in the right tail, the probability of these extreme outcomes does not fall very quickly, as high-risk funds more than compensate for the large drop in such extreme outcomes from low-risk funds. Conversely, consider a case in which the distribution of risk levels is less evenly spread out (i.e., more clustered), with 1% of the funds having a residual standard deviation of four, and the remaining 99% having a standard deviation of 0.92 (the average risk across funds remains equal to one). Here, tails of the cross section of alphas are now thinner than those of a normal. The intuition for these unexpected results is quite simple: The presence of many funds with low risk levels means that these funds' realized residuals have a low

[4] It is noteworthy that, even if all funds had normally distributed returns with identical levels of risk, it would still be infeasible to apply standard statistical methods to assess the significance of extreme alphas drawn from a large universe of ranked funds. In this case, the best alpha is modeled as the maximum value drawn from a multivariate normal distribution whose dimension depends on the number of funds in existence. Modeling this joint normal distribution depends on estimating the entire covariance matrix across all individual mutual funds, which is generally impossible to estimate with precision. Specifically, the difficulty in estimating the covariance matrix across several hundreds or thousands of funds is compounded by the entry and exit of funds, which implies that many funds do not have overlapping return records with which to estimate their covariances. Although one might use long-history market indices to improve the covariance matrix estimation as in Pastor and Stambaugh (2002), this method is not likely to improve the covariance estimation between funds that take extreme positions away from the indices.

probability of lying out in the far tails of the cross-sectional distribution of alpha estimates. At some point in the right tail, this probability decreases faster than can be compensated by the presence of a small group of high-risk funds.

Finally, consider a larger proportion of high-risk funds than the prior case—suppose 10% of the 1,000 funds have a residual standard deviation of two, while the remaining 90% have a standard deviation of 0.82 (again, the risk continues to be equal to one across all funds). In this case, the cross section of alphas has five- and three-percentile points that are thinner, but a one-percentile point that is thicker, than that of a normal. Thus, the cross section of alphas can have thick or thin tails relative to a normal distribution, regardless of the distribution of individual fund returns, as long as risk-taking is heterogeneous across funds.

In unreported tests, we measure the heterogeneity in risk-taking among all U.S. domestic equity mutual funds between 1975 and 2002. We find a heavily skewed distribution of risk-taking among funds; most funds cluster together with similar levels of risk, while a significant minority of funds exhibit much higher levels of risk. In further unreported tests, we bootstrap the cross section of fund alphas, where each fund is assumed to have residuals drawn from a normal distribution that has the same moments as those present in the actual (nonnormal) fund residuals (using a four-factor model to estimate residuals).[5] The results show that the cross section of bootstrapped alphas (assuming individual fund alpha normality) has thinner tails relative to a normal distribution, except in the extreme regions of the tails, which are thicker. Therefore, the heterogeneity in risk-taking that we observe in our fund sample generates many unusual nonnormalities in the cross section of alphas, even before considering any nonnormalities in individual fund alphas.

It is important to note that similar cross-sectional effects will not result when we assess the distribution of the t-statistic of the fund alphas. Since the t-statistic normalizes by standard deviation, heterogeneity in risk-taking across funds, by itself, will not bring about nonnormalities in the cross section.[6] However, nonnormalities in individual fund residuals—which, as we discuss in a later section, we find for about half of our funds—still imply nonnormalities in the cross section of t-statistics.[7]

[5] During each bootstrap iteration, 336 draws (with replacement) are made from a normal distribution with the same mean and variance as each fund's actual four-factor model residuals, and each fund's alpha for that iteration is measured as the average of these residuals. This generates one cross-sectional alpha outcome. We repeat this process 1,000 times.

[6] In fact, this points to the superior properties of the t-statistic relative to the alpha itself; because of these superior properties, we use this measure extensively in our bootstrap tests in later sections.

[7] For example, suppose that funds have IID thick-tailed residuals that (for each fund) are drawn from a mixture of two normal distributions. The first distribution represents a "high volatility" state, with a standard deviation of three and a probability of 10%, and the second represents a "normal" state, with a standard deviation of 1/3 and a probability of 90% (i.e., the average residual variance remains at unity for each fund). In this case (details available upon request), the cross-sectional distribution of t-statistics is thin-tailed relative to a normal, even though the individual fund residuals are fat-tailed. More complex effects, such as small positive correlations in the tails of the residual distributions (across funds) and small negative correlations in the central part of the residual distributions (to yield an overall correlation of zero between funds) can also change the cross-sectional tail probabilities in unexpected ways.

Thus, many factors, including cross-sectional differences in sample size (fund lives) and risk-taking, as well as fat tails and skews in the individual fund residuals, influence the shape of the distribution of alphas across funds. Given the possible interactions among these effects and the added complexity arising from parameter estimation errors, it is very difficult, using an ex ante imposed distribution, to credibly evaluate the significance of the observed alphas of funds since the quantiles of the standard normal distribution and those of the bootstrap need not be the same in the center, shoulders, tails, and extreme tails. Instead, the bootstrap is required for proper inference involving the cross-sectional distribution of fund performance outcomes.

To summarize, it is only in the very special case in which (1) the residuals of fund returns are drawn from a multivariate normal distribution, (2) correlations in these residuals are zero, (3) funds have identical risk levels, and (4) there is no parameter estimation error that guarantees that the standard critical values of the normal distribution are appropriate in the cross section. In all other cases, the cross section will be a complicated mixture of individual fund return distributions and must be evaluated with the bootstrap.[8]

B. Implementation

In our implementation, we consider two test statistics, namely, the estimated alpha, $\hat{\alpha}$, and the estimated t-statistic of $\hat{\alpha}$, $\hat{t}_{\hat{\alpha}}$. Note that $\hat{\alpha}$ measures the economic size of abnormal performance, but suffers from a potential lack of precision in the construction of confidence intervals, whereas $\hat{t}_{\hat{\alpha}}$ is a pivotal statistic with better sampling properties.[9] In addition, $\hat{t}_{\hat{\alpha}}$ has another very attractive statistical property. Specifically, a fund that has a short life or engages in high risk-taking will have a high variance-estimated alpha distribution, and thus alphas for these funds will tend to be spurious outliers in the cross section. In addition, these funds tend to be smaller funds that are more likely to be subject to survival bias, raising the concern that the extreme right tail of the cross section of fund alphas is inflated. The t-statistic provides a correction for these spurious outliers by normalizing the estimated alpha by the estimated variance of the alpha estimate. Furthermore, the cross-sectional distribution of standard deviation has better properties than the cross section of alphas, in the presence of heterogenous fund volatilities due to differing fund risk levels or lifespans. For these reasons, we propose an alternate bootstrap that is conducted using $\hat{t}_{\hat{\alpha}}$, rather than $\hat{\alpha}$. Indeed, the bulk of our tests in this paper apply to the t-statistic.

We apply our bootstrap procedure to monthly mutual fund returns using several models of performance proposed by the past literature. These include the simple one-factor model of Jensen (1968), the three-factor model of Fama

[8] In addition, refinements of the bootstrap (which we implement below) provide a general approach for dealing with unknown time-series dependencies that are due, for example, to heteroskedasticity or serial correlation in the residuals from performance regressions. These bootstrap refinements also address the estimation of cross-sectional correlations in regression residuals, thus avoiding the estimation of a very large covariance matrix for these residuals.

[9] A pivotal statistic is one that is not a function of nuisance parameters, such as Var (ε_{it}).

and French (1993), the timing models of Treynor and Mazuy (1966) and Merton and Henriksson (1981), and several models that include conditional factors based on the papers of Ferson and Schadt (1996) and Christopherson, Ferson, and Glassman (1998). We present results for two representative models in this paper; however results for all other models are consistent with those presented and are available upon request from the authors.[10] The first model, the main model that we present in this paper, is the Carhart (1997) four-factor regression

$$r_{i,t} = \alpha_i + \beta_i \cdot RMRF_t + s_i \cdot SMB_t + h_i \cdot HML_t + p_i \cdot PR1YR_t + \varepsilon_{i,t} \ , \qquad (1)$$

where $r_{i,t}$ is the month t excess return on managed portfolio i (net return minus T-bill return), $RMRF_t$ is the month t excess return on a value-weighted aggregate market proxy portfolio, and SMB_t, HML_t, and $PR1YR_t$ are the month t returns on value-weighted, zero-investment factor-mimicking portfolios for size, book-to-market equity, and 1-year momentum in stock returns, respectively.

The second representative model is a conditional version of the four-factor model that controls for time-varying $RMRF_t$ loadings by a mutual fund, using the technique of Ferson and Schadt (1996). Hence, the second model extends equation (1) as follows:

$$r_{i,t} = \alpha_i + \beta_i \cdot RMRF_t + s_i \cdot SMB_t + h_i \cdot HML_t + p_i \cdot PR1YR_t + \sum_{j=1}^{K} B_{i,j}[z_{j,t-1} \cdot RMRF_t] + \varepsilon_{i,t}, \qquad (2)$$

where $z_{j,t-1} = Z_{j,t-1} - E(Z_j)$, the end of month $t-1$ deviation of public information variable j from its time series mean, and $B_{i,j}$ is the fund's "beta response" to the predictive value of $z_{j,t-1}$ in forecasting the following month's excess market return, $RMRF_t$. This model computes the alpha of a managed portfolio, controlling for strategies that dynamically tilt the portfolio's beta in response to the predictable component of market returns.[11]

[10] We consider 15 different models. In all cases, the results are similar to those we present in the paper. The two representative models that we present are the "best fit" models, according to standard model selection criteria, such as the Schwarz Information Criterion.

[11] We also use model selection criteria to choose the conditioning variables for our representative conditional model. The result, according to this criterion, is that the dividend yield (alone) is the conditioning variable for the preferred model; therefore, the results that we present use $K = 1$. However, we find that results similar to those presented hold for other versions of the conditional model, including a model that uses four conditioning variables as potentially holding predictive value for each of the four factors of the model. These results are available upon request from the authors. Specifically, we consider the following public information variables: (1) the short interest rate, measured as the lagged level of the 1-month Treasury bill yield; (2) the dividend yield, measured as the lagged total cash dividends on the value-weighted CRSP index over the previous 12 months divided by the lagged level of the index; (3) the term spread, measured as the lagged yield on a constant-maturity 10-year Treasury bond less the lagged yield on 3-month Treasury bills; and (4) the default spread, measured as the lagged yield difference between bonds rated BAA by Moody's and bonds rated AAA. These variables have been shown to be useful for predicting stock returns and risks over time (see, for example, Pesaran and Timmermann (1995)).

We now illustrate the bootstrap implementation with the Carhart (1997) four-factor model of equation (1). The application of the bootstrap procedure to other models used in our paper is very similar, with the only modification of the following steps being the substitution of the appropriate benchmark model of performance.

To prepare for our bootstrap procedure, we use the Carhart model to compute ordinary least squares (OLS)-estimated alphas, factor loadings, and residuals using the time series of monthly net returns (minus the T-bill rate) for fund i (r_{it}),

$$r_{it} = \hat{\alpha}_i + \hat{\beta}_i RMRF_t + \hat{s}_i SMB_t + \hat{h}_i HML_t + \hat{p}_i PR1YR_t + \hat{\epsilon}_{i,t} \ . \tag{3}$$

For fund i, the coefficient estimates, $\{\hat{\alpha}_i, \hat{\beta}_i, \hat{s}_i, \hat{h}_i, \hat{p}_i\}$, as well as the time series of estimated residuals, $\{\hat{\epsilon}_{i,t}, t = T_{i0}, \ldots, T_{i1}\}$, and the t-statistic of alpha, $\hat{t}_{\hat{\alpha}_i}$, are saved, where T_{i0} and T_{i1} are the dates of the first and last monthly returns available for fund i, respectively.

B.1. The Baseline Bootstrap Procedure: Residual Resampling

Using our baseline bootstrap, for each fund i, we draw a sample with replacement from the fund residuals that are saved in the first step above, creating a pseudo–time series of resampled residuals, $\{\hat{\epsilon}^b_{i,t_\varepsilon}, t_\varepsilon = s^b_{T_{i0}}, \ldots, s^b_{T_{i1}}\}$, where b is an index for the bootstrap number (so $b = 1$ for bootstrap resample number 1), and where each of the time indices $s^b_{T_{i0}}, \ldots, s^b_{T_{i1}}$ are drawn randomly from $[T_{i0},\ldots,T_{i1}]$ in such a way that reorders the original sample of $T_{i1} - T_{i0} + 1$ residuals for fund i. Conversely, the original chronological ordering of the factor returns is unaltered; we relax this restriction in a different version of our bootstrap below.

Next, we construct a time series of pseudo–monthly excess returns for this fund, imposing the null hypothesis of zero true performance ($\alpha_i = 0$, or, equivalently, $\hat{t}_{\hat{\alpha}_i} = 0$),

$$\left\{r^b_{i,t} = \hat{\beta}_i RMRF_t + \hat{s}_i SMB_t + \hat{h}_i HML_t + \hat{p}_i PR1YR_t + \hat{\epsilon}^{\,b}_{i,t_\varepsilon}\right\}, \tag{4}$$

for $t = T_{i0}, \ldots, T_{i1}$ and $t_\varepsilon = s^b_{T_{i0}}, \ldots, s^b_{T_{i1}}$. As equation (4) indicates, this sequence of artificial returns has a true alpha (and t-statistic of alpha) that is zero by construction. However, when we next regress the returns for a given bootstrap sample, b, on the Carhart factors, a positive estimated alpha (and t-statistic) may result, since that bootstrap may have drawn an abnormally high number of positive residuals, or, conversely, a negative alpha (and t-statistic) may result if an abnormally high number of negative residuals are drawn.

Repeating the above steps across all funds $i = 1,\ldots,N$, we arrive at a draw from the cross section of bootstrapped alphas. Repeating this for all bootstrap iterations, $b = 1,\ldots,1{,}000$, we then build the distribution of these cross-sectional draws of alphas, $\{\hat{\alpha}^b_i, i = 1, \ldots, N\}$, or their t-statistics,

$\{\hat{t}^b_{\hat{\alpha}_i}, i = 1, \ldots, N\}$, that result purely from sampling variation while imposing the null of a true alpha that is equal to zero. For example, the distribution of alphas (or t-statistics) for the top fund is constructed as the distribution of the maximum alpha (or, maximum t-statistic) generated across all bootstraps.[12] As we note in Section I.A, this cross-sectional distribution can be nonnormal, even if individual fund alphas are normally distributed. If we find that our bootstrap iterations generate far fewer extreme positive values of $\hat{\alpha}$ (or $\hat{t}_{\hat{\alpha}}$) compared to those observed in the actual data, then we conclude that sampling variation (luck) is not the sole source of high alphas, but rather that genuine stock-picking skills actually exist.

B.2. Bootstrap Extensions

We implement some other straightforward extensions of this bootstrap for our universe of funds as well. These extensions, which we describe in more detail in Section IV, include simultaneous residual and factor resampling, as well as a procedure that demonstrates that our results are robust to the presence of a potential omitted factor in our models. We also allow for the possibility of cross-sectional dependence among fund residuals that may be due, for example, to funds holding similar (or the same) stocks at the same time. That is, funds with very high measured alphas might have similar holdings. In addition, we implement a procedure that allows for the possibility that the residuals are correlated over time for a given fund, perhaps due to time-series patterns in stock returns that are not properly specified by our performance models.

II. Data

We examine monthly returns from the Center for Research in Security Prices (CRSP) mutual fund files. The CRSP database contains monthly data on net returns for each shareclass of every open-end mutual fund since January 1, 1962, with no minimum survival requirement for funds to be included in the database. Further details on this mutual fund database are available from CRSP.

Although some investment objective information is available from the CRSP database, we supplement these data with investment objective and other fund information from the CDA-Spectrum mutual fund files obtained from Thomson Financial, Inc., of Rockville, Maryland.[13] We use Thomson data since these in-

[12] Of course, this maximum alpha can potentially be associated with a different fund during each bootstrap iteration, depending on the outcome of the draw from each fund's residuals.

[13] The Thomson database, and the technique for matching it with the CRSP database, are described in Wermers (1999, 2000). These links are available from Wharton Research Data Services (WRDS).

vestment objectives are more complete and consistent than the corresponding information from CRSP.[14] For each open-end U.S. domestic equity fund, we compute monthly fund-level net returns by weighting shareclass-level returns by the proportion of fund total net assets represented by each shareclass at the beginning of each month. All shareclasses that exist at the beginning of a given month are included in this computation for that month; specifically, no-load, load, and institutional classes. Thus, our computed fund-level returns represent the experience of the average dollar invested in that fund (across all shareclasses), although most shareclass-level returns are not substantially different (ignoring loads) from fund-level returns. Since both the CRSP and CDA databases contain essentially all mutual funds that exist during our sample period (with the exception of some very small funds), our merged database is essentially free of survival bias.[15]

Our final database contains fund-level monthly net returns data on 2,118 U.S. open-end domestic equity funds that exist for at least a portion of the period from January 31, 1975 to December 31, 2002. We study the performance of the full sample of funds, as well as funds in each investment objective category. Namely, our sample consists of aggressive growth funds (285), growth funds (1,227), growth-and-income funds (396), and balanced or income funds (210).[16] Since balanced funds and income funds allocate a significant fraction of assets to nonequity investments, we require that such funds hold at least 50% domestic equities during the majority of their existence to be included in our tests.

Table I shows counts of funds and their average returns during 5-year subintervals. Panel A presents counts for the entire fund data set, while Panels B through E present counts segregated by investment objective. For example, Panel A shows that there were 322 funds (both surviving and nonsurviving)

[14] CRSP investment objective data are often missing for at least some years (and sometimes all years) before 1992. In addition, CRSP reports investment objective information, when available, from four different sources. As these sources classify funds in different ways, it is often difficult to determine the precise investment objective of a fund. The Thomson files report investment objectives in a more consistent manner across funds and over time. In any case, all investment objective information (CRSP and Thomson) is considered, as well as the name of the fund, when we classify it into a domestic equity category. Further, we use the first available investment objective for that fund to classify a fund throughout its life. Only 16% of funds change objectives, making this a reasonable approximation.

[15] A small number of very small funds could not be matched between the CRSP and CDA files, that is, they were usually present in the CRSP database, but not in the CDA database. Wermers (2000) discusses this limitation of the matching procedure; however, we note that these funds are generally very small funds with a short life during our sample period. Since we require a minimum return history for a fund to be included in our regression tests, the majority of these unmatched funds would be excluded from our tests in any case.

[16] Our study combines income funds and balanced funds, as the number in each category is relatively small (and because funds in these two categories make similar investments). Descriptions of the types of investments made by funds in each category are available in Grinblatt, Titman, and Wermers (1995).

during 1971 to 1975; this figure grows to 1,824 during 1998 to 2002. This rapid growth in numbers is mainly driven by a large expansion in the number of growth funds (Panel C), although other types also exhibit substantial increases. In order to verify whether our fund universe is representative of all U.S. open-end domestic equity funds that exist at each point in time, we compare our counts with those obtained from the Investment Company Institute (ICI,

Table I
Summary Statistics for Mutual Fund Database

This table reports the number, returns, and performance of U.S. open-end equity funds in existence during the 1975 to 2002 period. Panel A reports results for all investment objectives, Panel B for aggressive growth funds, Panel C for growth funds, Panel D for growth and income funds, and Panel E for balanced or income funds (these two categories are combined). The first column in each panel of this table shows the number of funds in existence in each subperiod. The second column of each panel reports the number of funds in existence with at least 60 monthly net return observations during the subperiod. The row "1971–1975," for example, shows that during the 1971 to 1975 subperiod, there existed 322 funds, but only 231 of them had 60 monthly observations during the 5-year subperiod. The third column reports the excess return in percent per year (12 times the average monthly return) of an equally weighted portfolio of funds during the subperiod. The fourth column reports the excess return in percent per year for an equally weighted portfolio of funds with at least 60 monthly observations during the subperiod. Column five reports the unconditional four-factor model alpha in percent per year for the equally weighted portfolio of funds during the subperiod. Column six reports the four-factor alpha in percent per year for funds that had at least 60 monthly observations during the subperiod.

	Number of Funds		Excess Return of Equally Weighted Portfolio (Pct/Year)		4-Factor-Alpha of Equally Weighted Portfolio (Pct/Year)	
Time Periods	≥1 Obs.	≥60 Obs.	≥1 Obs.	≥60 Obs.	≥1 Obs.	≥60 Obs.
Panel A: All Investment Objectives						
1971–1975	322	231	−9.8	−9.5	−0.1	0.1
1976–1980	342	282	11.9	12.1	−1.0	−0.7
1981–1985	459	300	4.1	4.0	0.2	0.1
1986–1990	796	421	9.5	9.6	0.2	0.0
1991–1995	1428	681	4.7	4.9	0.0	0.2
1996–2000	1929	1115	15.1	15.0	−1.8	−1.8
1998–2002	1824	1304	5.9	6.1	−1.0	−0.6
1975–2002	2118	1788	6.8	7.0	−0.5	−0.4
Panel B: Aggressive Growth Funds						
1971–1975	84	65	−12.6	−12.0	−0.4	0.0
1976–1980	88	76	15.8	16.0	−1.6	−1.3
1981–1985	132	79	3.8	3.5	−0.1	−0.7
1986–1990	208	122	9.8	9.8	0.5	0.1
1991–1995	240	177	7.1	7.4	1.0	1.2
1996–2000	242	191	17.4	17.3	−2.4	−2.3
1998–2002	232	200	6.6	7.0	−1.1	−0.8
1975–2002	285	264	8.0	8.0	−0.4	−0.4

(*continued*)

Table I—*Continued*

Time Periods	Number of Funds		Excess Return of Equally Weighted Portfolio (Pct/Year)		4-Factor-Alpha of Equally Weighted Portfolio (Pct/Year)	
	≥1 Obs.	≥60 Obs.	≥1 Obs.	≥60 Obs.	≥1 Obs.	≥60 Obs.
Panel C: Growth Funds						
1971–1975	99	65	−11.2	−10.8	0.2	0.8
1976–1980	108	84	12.6	13.1	−1.2	−0.8
1981–1985	158	91	4.8	4.7	1.2	1.2
1986–1990	292	142	10.0	9.8	0.5	0.1
1991–1995	692	251	4.2	4.4	−0.5	−0.2
1996–2000	1145	527	16.1	16.1	−1.6	−1.7
1998–2002	1079	697	6.5	7.0	−0.7	−0.1
1975–2002	1227	985	7.2	7.4	−0.4	−0.1
Panel D: Growth and Income Funds						
1971–1975	89	64	−8.2	−7.8	−0.1	−0.1
1976–1980	93	76	10.3	10.7	−0.2	−0.1
1981–1985	112	82	4.0	3.8	0.0	−0.1
1986–1990	195	105	9.6	9.7	−0.4	−0.4
1991–1995	330	169	3.7	4.0	−0.2	0.0
1996–2000	355	265	13.6	13.8	−1.8	−1.6
1998–2002	335	270	4.6	4.7	−1.4	−1.2
1975–2002	396	353	6.2	6.2	−1.1	−1.0
Panel E: Balanced or Income Funds						
1971–1975	50	37	−5.6	−5.9	−0.6	−0.7
1976–1980	53	46	6.8	6.7	−0.8	−1.0
1981–1985	57	48	3.8	3.7	−0.4	−0.6
1986–1990	101	52	7.9	7.8	0.5	0.0
1991–1995	166	84	3.4	3.4	−0.2	−0.2
1996–2000	187	132	10.1	10.2	−1.9	−1.9
1998–2002	178	137	3.2	3.5	−1.1	−0.8
1975–2002	210	186	4.9	4.9	−0.7	−0.6

2004). In general, we find that, allowing for a lag in the inclusion of new funds by Thomson (as noted in Wermers (2000)), our counts track the counts of the ICI quite closely.[17]

[17] Specifically, our data set has 312, 306, 401, 745, 1,175, 1,704, and 1,558 funds at the end of 1975, 1980, 1985, 1990, 1995, 2000, and 2002, respectively (note that these counts differ from those shown in the first column of Panel A, since Panel A includes funds that did not survive until the end of the period shown). Our count drops after 1999 because we do not add funds that started up after that year (since these funds would not have sufficient returns by 2002 for our baseline regression tests). In order to check our counts of funds against the ICI totals, we first adjust our counts to exclude balanced funds and income funds to render our counts comparable to those provided in Table 6 of ICI (2004). For example, we subtract the 54 balanced and income funds from our total of 401 funds to arrive at 347 funds at the beginning of 1985; in comparison, the ICI counts 430 funds (in the categories of "capital appreciation" and "total return") at the same time. The lower number of funds in our database relative to the ICI is due to our counts lagging those of the ICI by 1 to 3 years. For example, our January 1995 count of 1,045 (excluding balanced/income) roughly matches the ICI count of 1,086 at January 1993; our January 1990 count of 651 roughly matches the ICI count of 621 at the beginning of 1987.

We include only funds that have a minimum of 60 monthly net return observations in our baseline bootstrap tests; we relax this in later tests.[18] Panel A of Table I compares, for each 5-year subperiod, counts of all existing funds against counts of funds that have all 60 monthly returns. In addition, the panel presents average monthly excess returns and four-factor model alphas (both annualized to percent per year) for equal-weighted portfolios of funds in these two groups. Although our count of funds is substantially lower when we require 60 monthly returns, excess returns and four-factor alphas are only slightly higher than those for the full sample. Specifically, excess returns and alphas of funds surviving for the full 60 months are roughly 20 basis points per year higher during each subperiod than those for all existing funds (see Panel A). Slightly higher (but still small) differences exist for growth-oriented funds, which generally undertake riskier strategies (see Panels B and C). Overall, our results show that short-lived funds do not have substantially different average returns from longer-lived funds (consistent with the evidence of Carhart et al. (2002)). Nevertheless, as a robustness check, we apply the bootstrap (using some of our simpler models) with a minimum history requirement of 18, 30, 90, and 120 months in extensions of our tests. The results (which will be presented in Section IV.E) generally show that survival bias has almost no impact on our bootstrap results.

III. Empirical Results

A. *The Normality of Individual Fund Alphas*

Before progressing to our bootstrap tests, we analyze (in unreported tests) the distribution of individual fund residuals generated by the models of equations (1) and (2), as well as residuals generated by many other commonly used performance models. We find that normality is rejected for 48% of funds when using either the unconditional or conditional four-factor model; similar results obtain with all other models that we test. Moreover, we also find that the rejections tend to be very large for many of the funds, especially funds with extreme estimated alphas (either positive or negative). This strong finding of nonnormal residuals challenges the validity of earlier research that relies on the normality assumption: In turn, this challenge to standard t- and F-tests of the significance of fund alphas strongly indicates the need to bootstrap, especially in the tails, to determine whether significant estimated alphas are due, at least in part, to manager skills, or to luck alone. As we apply our bootstrap in the following sections, we will highlight the significant changes in inference that result, relative to the normality assumption.

[18] This minimum data requirement is necessary to generate more precise regression parameter estimates for our more complex models of performance. These monthly returns need not be contiguous, but any gap in returns results in the next nonmissing return observation being discarded since this return is cumulated (by CRSP) after the last nonmissing return observation (and, thus, cannot be used in our regressions).

B. Bootstrap Analysis of the Significance of Alpha Outliers

We first apply our baseline residual resampling method, described in Section I.B.1, to analyze the significance of mutual fund alphas. In these tests, we rank all mutual funds that have at least 60 months of return observations during the 1975 to 2002 period on their model alphas. It is important to note that, in all of our bootstrap results to come, for each ranked fund we compare p-values generated from our cross-sectional bootstrap with standard p-values that correspond to the t-statistics of these individual ranked funds—these individual fund t-tests, of course, do not consider the joint nature of the ex post sorting that we implement. As we discuss in Section I.A, the cross-sectional nature of our bootstrap, along with its ability to model nonnormalities in fund alphas, provides benefits over the casual use of standard t-tests applied to individual funds. Since investors and researchers usually examine funds without considering the joint nature of ex post sorts, we use this approach to inference as a benchmark against which to compare the bootstrap. As we will see, in many cases the bootstrap provides substantially different conclusions about the significance of individual ranked-fund performance.

B.1. Baseline Bootstrap Tests: Residual Resampling

Panel A of Table II shows several points in the resulting cross section of ranked alphas (using the unconditional and conditional four-factor models of equations (1) and (2)), and presents bootstrapped p-values ("Cross-sectionally bootstrapped p-value"), as well as standard p-values that correspond to the t-statistic of the individual fund at each percentile point of the distribution ("Parametric (standard) p-value"). For example, consistent with the results of Carhart (1997), the median fund in our sample has an unconditional four-factor alpha of -0.1% per month (-1.2%, annualized), while the bottom and top funds have alpha estimates of -3.6% and 4.2% per month, respectively.[19] Also, as further examples, the fifth-ranked fund and the fund at the one-percentile point in our sample have alphas of 1.3% and 1% per month, respectively.[20] Ranking funds by their *conditional* four-factor alphas results in alphas and p-values (both cross-sectionally bootstrapped and parametric normal) that are remarkably similar to those from the unconditional four-factor alpha sort. This finding indicates that mutual funds do not substantially time the overall market factor

[19] This top-ranked fund is the Schroeder Ultra Fund, which the media prominently featured as a fund with extraordinary performance. Although the fund eventually closed to new investments, there were many cases of investors wishing to purchase some shares from current shareholders at exorbitant prices in order to be allowed to add further to those holdings, which is allowed by this fund.

[20] As an example, the cross-sectionally bootstrapped p-value of 0.02 is the probability that the fifth-ranked fund (from repeated ex post alpha sorts) generates an estimated alpha of at least 1.3% per month, purely by sampling variation (i.e., with a true alpha of zero). In contrast, the parametric (standard) p-value of 0.01 for this fund is obtained from a simple t-test for its estimated alpha, without regard to the rank of this fund in the cross section.

Table II

The Cross Section of Mutual Fund Alphas

In Panel A of this table, all U.S. open-end domestic equity funds that have at least 60 monthly net return observations during the 1975 to 2002 period are ranked on their four-factor alpha. The first to third rows report results when funds are ranked on their unconditional four-factor alphas. The fourth to sixth rows report results when funds are ranked on their conditional four-factor alphas. In Panel A, the first and second rows report the OLS estimate of alphas (in percent per month) and the cross-sectionally bootstrapped p-values of the alphas. For comparison, the third row reports the p-values of the t-statistic of alphas based on standard critical values of the t-statistic. The fourth and fifth rows of Panel A report conditional alphas (in percent per month) and the cross-sectionally bootstrapped p-values of conditional alphas. The sixth row reports the p-values of the t-statistic of alphas, based on standard critical values of the t-statistic. In Panel B, all U.S. open-end domestic equity funds that have at least 60 monthly net return observations during the 1975 to 2002 period are ranked on the t-statistics of their four-factor alpha (first row). The second row of Panel B shows the cross-sectionally bootstrapped p-values of the t-statistics. For comparison, the third row shows the p-values of the t-statistic based on standard critical values. Rows four to six show results when funds are ranked based on the t-statistic of their conditional four-factor model alpha. Rows four and five report the t-statistics of the conditional alpha and the cross-sectionally bootstrapped p-value of the t-statistic. Row six shows the p-values of the t-statistic of the conditional alphas, based on standard critical values. In each panel, the first columns on the left (right) report results for funds with the five lowest (highest) alphas or t-statistics, followed by results for marginal funds at different percentiles in the left (right) tail of the distribution. The cross-sectionally bootstrapped p-value is based on the distribution of the best (worst) funds in 1,000 bootstrap resamples. The t-statistics of alpha are based on heteroskedasticity- and autocorrelation-consistent standard errors.

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	30%	40%	Median	40%	30%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel A: Funds Ranked on Four-Factor Model Alphas																									
Unconditional Alpha (Pct/Month)	−3.6	−2.7	−2.0	−1.7	−1.5	−0.8	−0.6	−0.5	−0.4	−0.2	−0.2	−0.1	−0.1	0.0	0.1	0.1	0.3	0.4	0.6	1.0	1.3	1.4	1.5	1.6	4.2
Cross-sectionally Bootstrapped p-Value	0.04	<0.01	<0.01	<0.01	<0.01	0.02	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01		1.00	1.00	0.99	0.02	<0.01	<0.01	<0.01	0.02	0.06	0.08	0.19	0.02
Parametric (Standard) p-Value	0.04	<0.01	<0.01	<0.01	<0.01	<0.01	0.06	0.01	0.09	<0.01	0.40	0.36		0.50	0.31	0.29	0.12	0.03	0.03	0.01	0.01	<0.01	0.12	0.01	<0.01
Conditional Alpha (Pct/Month)	−4.8	−2.2	−2.0	−1.9	−1.8	−1.0	−0.6	−0.5	−0.4	−0.3	−0.2	−0.1	−0.1	0.0	0.0	0.1	0.3	0.4	0.6	1.1	1.4	1.5	1.6	1.8	4.2
Cross-sectionally Bootstrapped p-Value	<0.01	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01		1	1	0.88	<0.01	<0.01	<0.01	<0.01	0.01	0.02	0.05	0.09	0.01
Parametric (Standard) p-Value	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.05	0.03	0.02	0.12	0.37		0.58	0.40	0.13	0.25	0.09	0.02	0.02	0.15	<0.01	0.01	0.01	<0.01
Panel B: Funds Ranked on the t-Statistics of Four-Factor Model Alphas																									
t-Unconditional Alpha	−7.9	−6.1	−6.0	−4.8	−4.5	−3.6	−3.0	−2.4	−1.9	−1.4	−0.9	−0.6	−0.4	0.0	0.3	0.8	1.4	2.0	2.3	2.8	3.5	3.5	4.1	4.2	6.6
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01		1.00	1.00	1.00	0.25	<0.01	<0.01	<0.01	0.04	0.08	0.01	0.03	<0.01
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.09	0.17	0.26		0.49	0.37	0.23	0.09	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
t-Conditional Alpha	−8.5	−5.8	−5.8	−5.7	−5.2	−3.8	−3.2	−2.7	−2.1	−1.6	−1.1	−0.7	−0.4	−0.1	0.3	0.8	1.4	2.0	2.3	2.9	3.5	3.7	3.9	4.6	5.8
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01		1.00	1.00	1.00	0.04	<0.01	<0.01	<0.01	0.01	0.02	0.02	0.01	0.01
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.02	0.06	0.13	0.23		0.55	0.40	0.22	0.08	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01

according to the level of the lagged dividend yield on the market portfolio. Therefore, for the remainder of this paper, we present results only for the unconditional four-factor model; however, in all cases, the conditional four-factor model exhibits similar results, which are available upon request from the authors.

Overall, the results in Panel A show that funds with alphas ranked in the top decile (10^{th} percentile and above) generally exhibit significant bootstrapped p-values, whether we use the unconditional or conditional four-factor model. However, this is not always the case. For example, the second-ranked fund under the unconditional model displays a large but insignificant alpha; this alpha simply is insufficiently large to reject (based on the empirical distribution of alphas) the hypothesis that the manager achieved it through luck alone. Thus, our bootstrap highlights that extreme alphas are not always significant, and that the bootstrap is important in testing for significance in the tails, which can have quite complex distributional properties.

No funds between (and including) the 20^{th} percentile and the median exhibit alphas sufficient to beat their benchmarks, net of costs, using either the unconditional or conditional versions of the four-factor model. When we examine funds below the median, using a null hypothesis that these funds do not underperform their benchmarks (net of costs), we find that all bootstrapped p-values strongly reject this null. This finding of significantly negative alphas for below-median funds indicates that these funds may very well be inferior to low-cost index funds. In unreported results available from the authors upon request, we arrive at the same conclusions with all other performance models, including complex models with both market timing measures and multiple risk factors.

As we discuss in Section I.B, we also rank, according to a second measure of fund performance, the t-statistic for the estimated alpha.[21] Again, the t-statistic has some advantageous statistical properties when constructing bootstrapped cross-sectional distributions, since it scales alpha by its standard error (which tends to be larger for shorter-lived funds and for funds that take higher levels of risk). In addition, it is related to the Treynor and Black (1973) appraisal ratio, which is prescribed by Brown et al. (1992) for helping to mitigate survival bias problems. Thus, the distribution of bootstrapped t-statistics in the tails is likely to exhibit better properties (fewer problems with high variance or survival bias) than the distribution of bootstrapped alpha estimates in that region.

Panel B presents results for funds ranked by their t-statistics. In general, right-tail funds continue to exhibit significant performance under a t-statistic ranking, as they do in the alpha ranking of Panel A. Most importantly, note that our inference about fund manager talent is somewhat different with the cross-sectional bootstrap than with the standard parametric normal assumption applied to individual fund alpha distributions ("Parametric (standard) p-value"

[21] Reported t-statistics use Newey–West (1987) heteroskedasticity- and autocorrelation-consistent standard errors.

in Panel B).[22] Namely, most of the top five funds have bootstrapped p-values that are higher than their parametric p-values, for both unconditional and conditional model alpha t-statistics. In this extreme right tail of the cross section, the bootstrap uncovers more probability mass (a fatter extreme right tail) than expected under a parametric normal assumption as a result of the complex interaction between nonnormal individual fund alphas as well as the complexity of the mixture of these distributions imposed by the cross-sectional draws. The same applies to funds closer to the median. For example, the standard parametric p-value for the t-statistic (the one-tailed p-value for $t = 1.4$ is roughly 9%) indicates that the fund at the 10^{th} percentile exhibits a significant t-statistic, under the unconditional four-factor model. However, the bootstrap does not find this t-statistic to be significant, and does not reject the null of no manager talent at the 10^{th} percentile (this p-value equals 25%).

To explore further, Figure 1 presents distributions of unconditional four-factor alphas for funds at various points in the cross section. For example, Panel A1 shows the bootstrapped distribution of the alpha of the bottom-ranked fund across all bootstrap iterations. While the mode of this distribution lies at roughly -1.7% per month, bootstrapped alphas vary from about -1% per month to (in rare cases) less than -6% per month. It is easy to see that the actual bottom-fund (estimated) alpha of -3.6% per month (the dashed line in Panel A1) lies well within the left-tail rejection region of the distribution; this rejection is so strong that a standard t-test also rejects. However, Panel B4 gives a case in which the bootstrap rejects the null, while the simple t-test does not. In general, as we proceed to the center of the cross-sectional distribution, (Panels A1 to A4 and Panels B1 to B4), alpha distributions become more symmetric, but remain markedly nonnormal.

As we note earlier, the extreme deviation from normality that we observe in the extreme top and bottom funds, as ranked by alpha (Panels A1 and B1), is due to those positions generally being occupied by funds that have very risky strategies. This motivates our t-statistic ranking procedure shown in Panel B of Table II. However, we find that bootstrapped t-statistics for funds at various points in the cross-sectional distribution also deviate substantially from normality, as Figure 2 illustrates.

Panel A of Figure 2 compares the cross-sectional distribution of actual fund t-statistic estimates with the distribution generated by the bootstrap.[23] The two

[22] Again, in addition to their normality assumption, one should note that these individual fund parametric p-values do not account for the ex post, cross-sectional nature of our ranking of funds. For instance, one would not conclude that the 3-percentile fund shown in Panel A has a significant alpha simply because its p-value is 3%—this would be expected in the absence of any true alpha under an assumption of IID multivariate normal fund returns. However, since ex post individual fund p-values are often used to infer talent (rather than a full cross-sectional test statistic across funds), we compare the inference under the cross-sectional bootstrap with this often-used approach.

[23] The distributions are smoothed with a kernel density estimator. This estimator replaces the "boxes" in a histogram by "bumps" that are smooth, and the kernel function is a weighting function that determines the shape of the bumps. We generate the plot using a Gaussian kernel function. The optimal bandwidth controls the smoothness of the density estimate, and is calculated according to Silverman (1986).

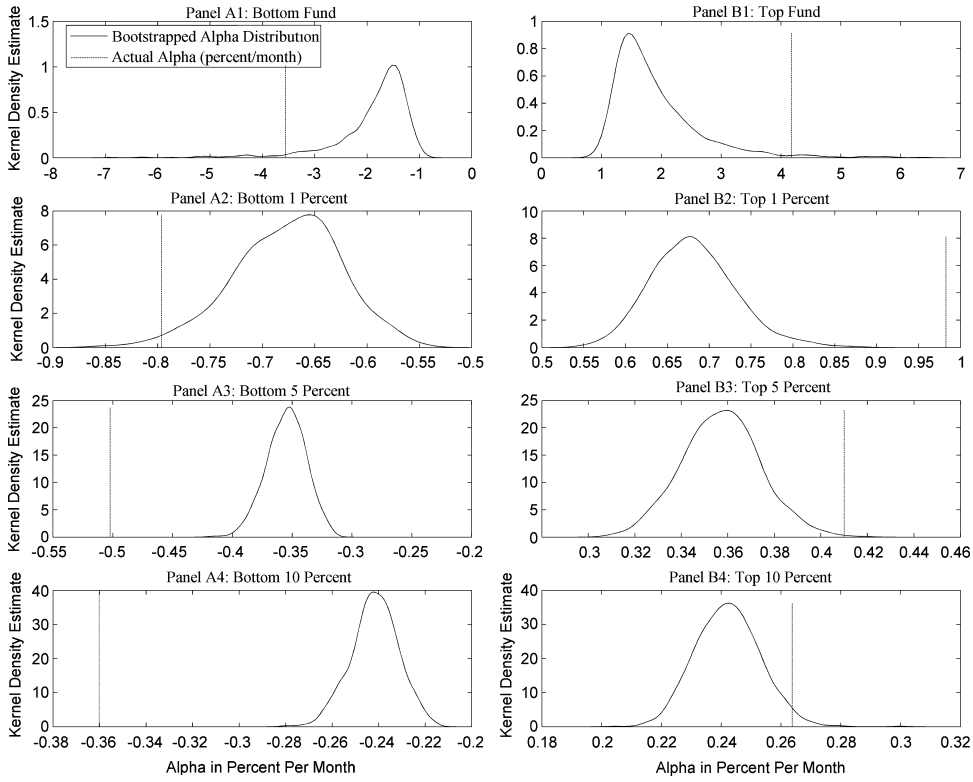

Figure 1. Estimated alphas vs. bootstrapped alpha distributions for individual funds at various percentile points in the cross section. This figure plots kernel density estimates of the bootstrapped unconditional four-factor model alpha distribution (solid line) for all U.S. equity funds with at least 60 monthly net return observations during the 1975 to 2002 period. The *x*-axis shows the alpha performance measure in percent per month, and the *y*-axis shows the kernel density estimate. The dashed vertical line represents the actual (estimated) fund alpha. Panels A1–A4 show marginal funds in the left tail of the distribution. Panels B1–B4 show marginal funds in the right tail of the distribution. For example, "Top 1 Percent" in Panel B2 refers to the marginal alpha at the top one percentile of the distribution.

densities in Panel A have quite different shapes. In particular, the distribution of actual *t*-statistics has more probability mass in the left and right tails, and far less mass in the center than the bootstrapped distribution. However, this is not the whole story—the distribution of actual *t*-statistics also exhibits several complex features such as "shoulders" in the tail regions. Thus, our bootstrap inference is different from inference based on the normality assumption not simply because the bootstrap more adequately measures fat or thin tails of the actual distribution but also because the bootstrap more adequately captures the complex shape of the entire cross-sectional distribution of *t*-statistics (and, especially that of the tails) under the null. The 95% standard error bands around the bootstrapped distribution confirm that the differences between the two distributions are statistically significant.

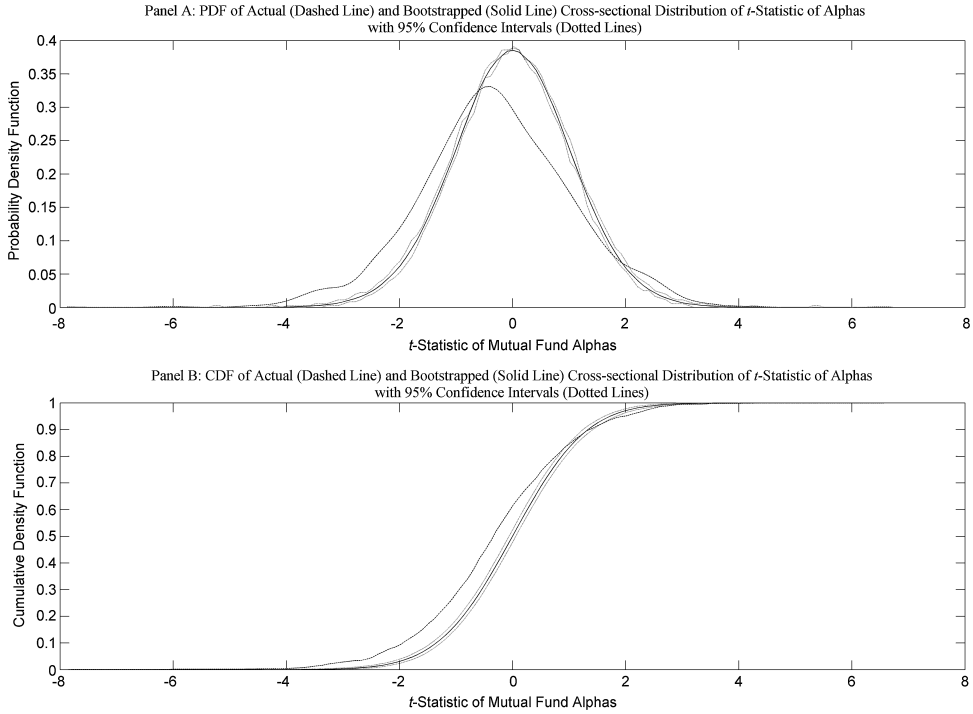

Figure 2. Estimated vs. bootstrapped cross section of alpha *t*-statistics. This figure plots kernel density estimates of the actual (dashed line) and bootstrapped (solid line) cross-sectional distributions of the *t*-statistic of mutual fund alphas. Panel A shows the kernel density estimate of the probability density function (PDF) of the distributions, and Panel B the kernel density estimate of the cumulative density function (CDF) of the distributions. The alpha estimates are based on the unconditional four-factor model applied to all U.S. equity funds with at least 60 monthly net return observations during the 1975 to 2002 period. The dotted lines give 95% confidence intervals of the bootstrapped distribution.

Overall, this figure illustrates that our sample of funds generates actual *t*-statistics that have a very nonnormal cross-sectional distribution, and that the tails of this actual distribution are not well explained by random sampling error (which is represented by the bootstrapped distribution). These observations reinforce our prior evidence that many superior and inferior funds exist in our sample. Since our interest is the actual number of funds that exceed a certain level of alpha compared to the bootstrapped distribution, we plot the cumulative density function in Panel B. The results confirm our observations from Panel A that in the far right tail, the actual probability distribution has more weight than the bootstrapped distribution. In addition, as Panel B of Table II indicates, *t*-statistics above 1.96 are generally significant, which results in the actual cumulative density function lying below the bootstrapped cumulative density function in that region.

We can also use the bootstrapped distribution of alphas to calculate how many funds (out of the total set of funds with a track record of at least 5 years)

would be expected, by chance alone, to exceed a given level of performance. This number can be compared to the number of funds that actually exceed this level of performance in our sample. Panel A of Figure 3 plots the cumulative number of funds from the original and from the bootstrapped distribution (the average number across all bootstrap iterations) that perform above each level of alpha, while Panel B plots the cumulative numbers that perform below each level. For example, Panel A indicates that nine funds should have an alpha estimate higher than 10% per year by chance, whereas in reality, 29 funds achieve this alpha, and Panel B indicates that 128 funds exhibit an alpha estimate less than −5% per year, compared to an expected number of 63 funds by random chance.

Overall, the results in this section provide strong evidence that many of the extreme funds in our sample exhibit significant positive (or negative) alphas and alpha *t*-statistics. For example, Panel A of Figure 3 indicates that, among the subgroup of fund managers that have an alpha greater than 7% per year over a 5-year (or longer) period, about half have stockpicking talent sufficient to exceed their costs, while the other half are simply lucky.

To evaluate the overall potential economic impact of our findings, we approximate the value added of skilled managers. This is important as, for example, our bootstrap-based evidence of skill might occur primarily among small mutual funds, which might tend to lie further in the tails of the alpha distribution. If so, then our results may not have significant implications for the average investor in actively managed mutual funds. To address this issue, we measure economic impact by examining the difference in the value added between all funds that have a certain level of performance, including lucky and skilled funds, and those funds that achieve the same level of performance due to luck alone (estimated by the bootstrap); this difference estimates skill-based added value.[24] Panels A and B of Figure 3 show the cumulative value added (value destroyed) above (below) each point in the alpha distribution. As the figure depicts, we estimate that about $1.2 billion per year in wealth is generated (see "Mean"), in excess of expenses and trading costs, through true active management skills by funds in the right tail of the cross section of alphas over the 1975 to 2002 period. By contrast, truly underperforming left-tail funds destroy a total of $1.5 billion per year by their inability to compensate for fees and trading costs. It should be noted that wealth created in a typical year exceeds wealth destroyed by a greater ratio than these figures, as the average outperforming fund in our

[24] For example, as we mention, there are 29 funds with alpha estimates greater than or equal to 10% per year, while only 9 funds are expected to exhibit such alphas by chance. To approximate the value-added of the additional 20 funds (since we do not know which 20 funds out of the 29 are skilled), we estimate value-added for a given 1% alpha interval as that level of alpha (e.g., 10% per year) multiplied by the average size of all funds lying in the same 1% per year alpha interval, multiplied by the difference between the total number of funds in that interval and the number of "lucky" funds (according to the bootstrap). These interval value-added estimates are cumulated for all alpha intervals above and including a given interval (e.g., the 10% interval), and these cumulative value-added estimates are presented in the graph. As an upper bound, we repeat this computation using the average size of the largest funds in each alpha interval in case the 20 skilled funds are generally the largest funds (labeled "Max").

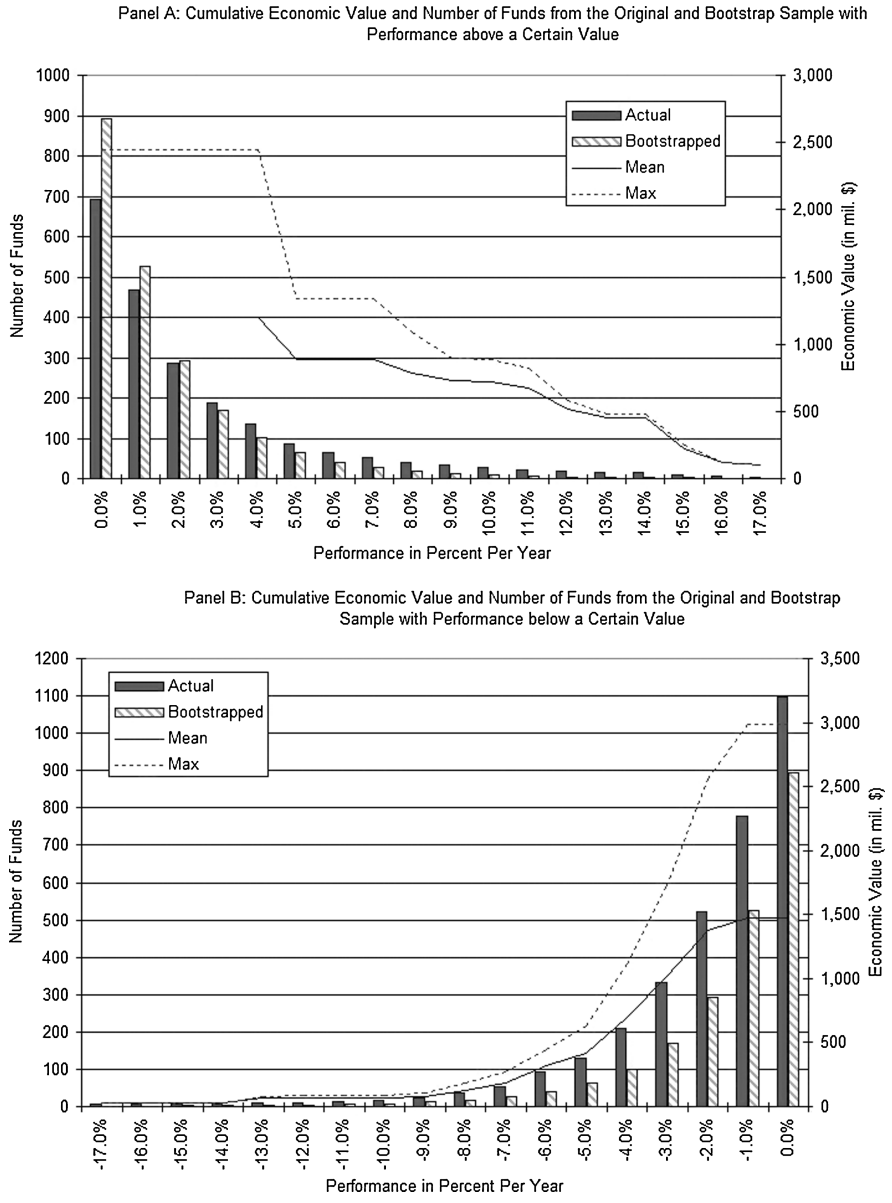

Figure 3. Cumulative economic value-added by funds above (or below) various alpha levels. This figure presents the number of funds from the original and the bootstrapped cross-sectional distributions (as vertical bars) that surpass (Panel A) or lie below (Panel B) various unconditional four-factor alpha levels. In Panel A, the solid and dashed lines show the cumulative economic value that a hypothetical investor could potentially gain by investing in the difference between the actual and the bootstrapped number of funds in all higher performance brackets. The solid (dashed) line is based on the average (average of a subgroup of the largest funds') total net assets in each performance bracket. In Panel B, the solid and dashed lines show the cumulative value that is potentially lost by the statistically significant underperformance of some funds. The results are based on all U.S. equity funds in our sample between 1975 and 2002 with a minimum of 60 monthly net return observations.

sample tends to be longer-lived (and our estimate assumes that all funds in our sample exist in a "typical" year).

One should note, however, that many issues could complicate the interpretation of our baseline bootstrap results above. For example, funds may have cross-sectionally correlated residuals. If so, this could bias our bootstrap results, which (so far) have rested on the assumption of independent residuals. We explore these and other concerns in Section IV of this paper.

B.2. Baseline Bootstrap Tests for Subperiods

To examine whether the cross-sectional distribution of mutual fund performance changes over our sample period, we examine two subperiods of roughly equal lengths, namely, 1975 to 1989 and 1990 to 2002. Table III reports the results. According to our bootstrap, outperforming fund managers have become more scarce since 1990. Either markets have become more efficient, or competition among the large number of new funds has reduced the gains from trading (or perhaps these two are related). Nevertheless, we do find substantial performance in the top 5% of funds in the post-1990 period. Note, also, that inference based on the bootstrap differs from that of the standard parametric normal at many more points in the cross-sectional distribution for subperiods, relative to the whole sample of Table II. In fact, as we will see in the next section, the bootstrap becomes more important for correct inference when we move to smaller numbers of funds, or to funds that have shorter lives.

B.3. Baseline Bootstrap Tests for Investment Objective Subgroups

Prior research indicates that managers of growth-oriented funds have better stock-picking talent than managers of income-oriented funds. For example, Chen, Jegadeesh, and Wermers (2000) find that the average growth fund buys stocks with abnormal returns that are 2% per year higher than stocks the fund sells. By contrast, the average income-oriented fund does not exhibit any stock-picking talent. However, it is not clear whether stock-picking talent translates into superior net return performance. Accordingly, we also divide our sample of funds by investment objective to verify whether the investment style of a fund affects the tails of the alpha and t-statistic distributions.

Table IV reports bootstrap results for each investment objective subgroup ranked with the unconditional four-factor alpha, as well as the corresponding t-statistic of this alpha. We focus on discussing the t-statistic rankings, although the alpha rankings show similar results. For example, the second half of Panel A of Table IV shows the results of our t-statistic ranking and associated bootstrapped p-values for "growth" funds. For this subgroup of funds, our bootstrapped p-values indicate that all funds above (and including) the 5-percentile point have managers with stock-picking skills that outweigh their expenses and trading costs. However, the results also lead us to conclude that the inferior performance of funds in the left tail is significant; all funds below (and including) the 20^{th} percentile in the left tail have managers who

Table III

The Cross Section of Mutual Fund Alphas, for Subperiods

This table reports subperiod results for the cross section of funds' performance measures. In Panel A, all U.S. open-end domestic equity funds that have at least 60 monthly net return observations during the 1975 to 1989 period are ranked on their unconditional four-factor model alphas. The first and second rows report the OLS estimate of alphas (in percent per month) and the cross-sectionally bootstrapped p-values of alpha. For comparison, the third row reports the p-values of the t-statistic of alphas, based on standard critical values of the t-statistic. In Panel B, all U.S. open-end domestic equity funds that have at least 60 monthly net returns during the 1975 to 1989 period are ranked on the t-statistics of their unconditional four-factor model alpha. The first row shows the t-statistics of the unconditonal alpha. The second row reports the cross-sectionally bootstrapped p-values of the t-statistic of alphas. For comparison, the third row shows the p-values of the t-statistic, based on standard critical values. Panels C and D report the same measures as Panels A and B, but for the subperiod 1990 to 2002. In each panel, the first columns on the left (right) report results for funds with the five lowest (highest) alphas or t-statistics, followed by marginal funds at different percentiles in the left (right) tail of the distribution. The cross-sectionally bootstrapped p-value is based on the distribution of the best (worst) funds in 1,000 bootstrap resamples. The t-statistics of alpha are based on heteroskedasticity- and autocorrelation-consistent standard errors.

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	30%	40%	Median	40%	30%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel A: Funds Ranked on Four-Factor Model Alphas (1975–1989)																									
Unconditional Alpha (Pct/Month)	−2.7	−1.4	−1.2	−1.1	−1.0	−1.0	−0.6	−0.5	−0.3	−0.2	−0.1	−0.1	0.0	0.1	0.1	0.2	0.3	0.5	0.6	0.7	0.7	0.8	0.9	1.0	1.0
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.03	0.10		<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.01	0.01	0.02	0.05	0.36
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.10	0.02	0.01	0.28	0.16	0.36		0.33	0.35	0.08	0.01	0.09	0.03	<0.01	<0.01	<0.01	0.01	<0.01	0.01
Panel B: Funds Ranked on t-Statistics of Four-Factor Model Alphas (1975–1989)																									
t-Unconditional Alpha	−6.1	−5.2	−4.7	−4.6	−4.0	−4.0	−2.9	−2.3	−1.8	−1.0	−0.6	−0.4	0.0	0.4	0.8	1.2	1.9	2.5	2.8	3.4	3.4	3.5	3.5	4.4	4.6
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.03	0.14	0.03		0.03	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	<0.01	0.02
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.16	0.27	0.35		0.36	0.21	0.11	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	30%	40%	Median	40%	30%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel C: Funds Ranked on Four-Factor Model Alphas (1990–2002)																									
Unconditional Alpha (Pct/Month)	−3.8	−3.6	−2.7	−1.5	−1.4	−0.9	−0.6	−0.5	−0.4	−0.3	−0.2	−0.1	−0.1	0.0	0.1	0.1	0.3	0.4	0.6	1.2	1.4	1.5	1.6	1.6	4.2
Cross-sectionally Bootstrapped *p*-Value	0.04	<0.01	<0.01	0.02	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01		1.00	1.00	0.99	0.23	<0.01	<0.01	<0.01	0.02	0.01	0.07	0.19	0.02
Parametric (Standard) *p*-Value	<0.01	0.04	<0.01	<0.01	<0.01	0.01	0.01	0.04	0.05	0.07	0.24	0.25		0.49	0.44	0.07	<0.01	0.03	0.16	<0.01	<0.01	0.12	<0.01	0.01	<0.01
Panel D: Funds Ranked on *t*-Statistics of Four-Factor Model Alphas (1990–2002)																									
t-Unconditional Alpha	−7.3	−6.6	−6.0	−5.0	−4.7	−3.6	−2.9	−2.5	−1.9	−1.4	−1.0	−0.6	−0.4	−0.1	0.3	0.7	1.3	1.9	2.2	2.8	3.4	3.5	4.1	4.2	6.6
Cross-sectionally Bootstrapped *p*-Value	0.02	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01		1.00	1.00	1.00	0.50	<0.01	<0.01	0.01	0.04	0.12	<0.01	0.03	<0.01
Parametric (Standard) *p*-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.09	0.17	0.27		0.48	0.38	0.23	0.09	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01

Table IV

The Cross Section of Mutual Fund Alphas, by Investment Objective

This table reports the cross section of alphas by investment objective. In Panel A all growth funds that have at least 60 monthly net return observations during the 1975 to 2002 period are ranked on their unconditional four-factor model alphas. The first and second rows report the OLS estimate of alphas (in percent per month) and the cross-sectionally bootstrapped p-value of the unconditional four-factor alpha. For comparison, the third row reports the p-values of the t-statistic of alphas, based on standard critical values of the t-statistic. In rows four to six of Panel A all growth funds that have at least 60 monthly net return observations during the 1975 to 2002 period are ranked on the t-statistics of their unconditional four-factor model alphas. The fourth row shows the t-statistics of the alpha. The fifth row reports the cross-sectionally bootstrapped p-values of the t-statistic of alphas. For comparison, the sixth row shows the p-values of the t-statistic, based on standard critical values. Panels B, C, and D report the same measures as Panel A, but for the investment objectives of aggressive growth, growth and income, and balanced or income. In each panel, the first columns on the left (right) report results for funds with the five lowest (highest) alphas or t-statistics, followed by results for marginal funds at different percentiles in the left (right) tail of the distribution. The cross-sectionally bootstrapped p-value is based on the distribution of the best (worst) funds in 1,000 bootstrap resamples. The t-statistics of alpha are based on heteroskedasticity- and autocorrelation-consistent standard errors.

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel A: Growth Funds Ranked on Four-Factor Model Alphas																				
Unconditional Alpha (Pct/Month)	−3.6	−2.7	−1.7	−1.5	−1.2	−0.8	−0.6	−0.5	−0.4	−0.3	0.1	0.3	0.4	0.6	1.0	1.3	1.4	1.5	1.6	4.2
Cross-sectionally Bootstrapped p-Value	0.05	<0.01	0.01	0.01	0.07	0.25	0.02	<0.01	<0.01	<0.01	0.99	0.02	<0.01	<0.01	<0.01	0.02	0.06	0.08	0.19	0.02
Parametric (Standard) p-Value	0.04	<0.01	<0.01	<0.01	<0.01	0.06	<0.01	0.18	0.08	0.31	0.29	0.12	0.03	0.03	0.01	0.01	0.00	0.12	0.01	<0.01
Growth Funds Ranked on t-Statistics of Four-Factor Model Alphas																				
t-Unconditional Alpha	−7.9	−4.8	−4.2	−4.0	−3.9	−3.6	−2.9	−2.4	−1.8	−1.3	0.8	1.4	2.0	2.3	2.8	3.5	3.5	4.1	4.2	6.6
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	1.00	0.25	<0.01	<0.01	<0.01	0.04	0.08	0.01	0.03	<0.01
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.04	0.10	0.23	0.09	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Panel B: Aggressive Growth Funds Ranked on Four-Factor Model Alphas																				
Unconditional Alpha (Pct/Month)	−2.0	−1.2	−1.1	−1.1	−0.9	−1.1	−0.7	−0.6	−0.4	−0.3	0.2	0.3	0.6	0.7	0.9	0.9	0.9	0.9	0.9	1.1
Cross-sectionally Bootstrapped p-Value	<0.01	0.02	0.01	<0.01	<0.01	0.01	<0.01	<0.01	<0.01	<0.01	0.33	<0.01	<0.01	<0.01	0.05	<0.01	0.02	0.05	0.20	0.37
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.09	0.20	0.07	0.01	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Aggressive Growth Funds Ranked on t-Statistics of Four-Factor Model Alphas																				
t-Unconditional Alpha	−6.1	−3.8	−1.5	−2.1	−2.4	−1.5	−4.5	−1.7	−1.5	−1.2	0.7	2.4	2.6	2.9	2.2	3.1	1.2	2.2	2.3	3.5
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.63	0.13	<0.01	<0.01	0.04	0.02	0.06	0.04	0.12	0.19
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.09	0.20	0.07	0.01	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel C: Growth and Income Funds Ranked on Four-Factor Model Alphas																				
Unconditional Alpha (Pct/Month)	−0.9	−0.8	−0.8	−0.7	−0.6	−0.7	−0.5	−0.4	−0.3	−0.2	0.1	0.1	0.3	0.3	0.5	0.4	0.5	0.5	0.6	0.8
Cross-sectionally Bootstrapped *p*-Value	0.11	0.03	0.01	0.01	<0.01	0.01	<0.01	<0.01	<0.01	<0.01	0.97	1.00	0.22	0.35	0.27	0.35	0.27	0.20	0.39	0.26
Parametric (Standard) *p*-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.03	0.06	0.15	0.24	0.31	0.17	0.01	0.09	0.02	0.03	0.02	0.09	0.03	0.05
Growth and Income Funds Ranked on *t*-Statistics of Four-Factor Model Alphas																				
t-Unconditional Alpha	−4.4	−4.0	−3.8	−3.6	−3.6	−3.6	−3.0	−2.5	−2.1	−1.4	0.7	1.3	1.7	1.9	2.5	2.4	2.5	2.6	2.7	3.5
Cross-sectionally Bootstrapped *p*-Value	0.03	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.99	0.68	0.59	0.69	0.46	0.35	0.46	0.49	0.63	0.27
Parametric (Standard) *p*-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.02	0.08	0.25	0.10	0.05	0.03	0.01	0.01	0.01	0.01	<0.01	<0.01
Panel D: Balanced or Income Funds Ranked on Four-Factor Model Alphas																				
Unconditional Alpha (Pct/Month)	−0.6	−0.6	−0.5	−0.4	−0.4	−0.6	−0.3	−0.3	−0.3	−0.2	0.1	0.1	0.2	0.3	0.4	0.3	0.3	0.4	0.4	0.5
Cross-sectionally Bootstrapped *p*-Value	0.37	0.05	0.01	0.06	0.03	0.05	0.01	<0.01	<0.01	<0.01	0.95	0.40	0.46	0.22	0.37	0.17	0.35	0.17	0.37	0.47
Parametric (Standard) *p*-Value	0.01	0.01	0.02	0.15	0.02	0.01	0.04	0.05	<0.01	0.04	0.17	0.21	0.05	0.11	<0.01	0.10	0.09	0.01	<0.01	0.05
Balanced or Income Funds Ranked on *t*-Statistics of Four-Factor Model Alphas																				
t-Unconditional Alpha	−6.0	−3.6	−3.3	−3.3	−3.2	−3.6	−3.1	−2.6	−2.3	−1.6	0.7	1.2	1.7	2.0	2.4	2.1	2.1	2.2	2.4	2.8
Cross-sectionally Bootstrapped *p*-Value	<0.01	0.01	<0.01	<0.01	<0.01	0.01	<0.01	<0.01	<0.01	<0.01	0.98	0.81	0.65	0.51	0.71	0.50	0.69	0.74	0.71	0.64
Parametric (Standard) *p*-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.01	0.05	0.26	0.11	0.05	0.03	0.01	0.02	0.02	0.02	0.01	<0.01

cannot recover costs through superior stock-picking talents. Note that inference based on a standard *t*-test follows our cross-sectional bootstrap-based inferences fairly closely, with the exception of the 10-percentile point for this subgroup of funds. Thus, other than the important result that the cross-sectional bootstrap finds outperformance in only half of the right tail, compared to standard individual fund *t*-tests, differences between the two different test results are not large.

Panel B reports results for aggressive growth funds (see the *t*-statistic ranking). Here, right-tail bootstrapped *p*-values above (and including) the 5-percentile fund are statistically significant, with the exception of the best and second-best funds. Note that using standard *t*-tests, we conclude that these two funds, and the 10-percentile fund, exhibit significant performance. Again, as with growth funds, standard *t*-tests would lead us to believe that superior performance is about twice as prevalent among funds as it actually proves to be. Even more compelling evidence on the importance of the bootstrap is evident in Panels C and D, which examine funds that have an investment objective of growth and income and funds that have an investment objective of either balanced or income, respectively. While the large bootstrapped *p*-values lead us to conclude that high-alpha funds in either of these groups are simply lucky, standard *t*-tests would lead us to conclude otherwise for funds above (and including) the 10-percentile fund.

Thus, among smaller samples of funds, our bootstrap departs most strongly from the standard normality assumption when we examine the riskiest funds (extreme aggressive growth funds) as well as fund groups for which the alpha of top funds is not very large (income-oriented funds). Figure 4 further explores the shape of the bootstrapped distributions of *t*-statistics in the right tail of each ranked investment objective category. These figures clearly show the complex nonnormal shapes of these distributions, which explains why we arrive at different conclusions about manager talent with the bootstrap. Note that with more extreme actual measured *t*-statistics (i.e., Panels A1 through D1), bootstrapped distributions become much more nonnormal, exhibiting substantial "shoulders" and skewness. In addition, note that the outperformance for growth-oriented funds is much more dramatic than for income-oriented funds, making the bootstrap less crucial for arriving at appropriate inferences for growth funds.

For the sake of brevity, we do not include results for the conditional four-factor model segregated by investment objective categories, but these results are very similar to the unconditional model results above and are available from the authors upon request On the whole, we find that inference using the bootstrap is substantially different from that of the normality assumption of past performance studies. Specifically, in some very important cases, we arrive at exactly the opposite conclusion about manager talent, especially in examining the right tails of funds with higher-risk strategies (aggressive growth funds) or funds with lower levels of right-tail performance (income-oriented funds).

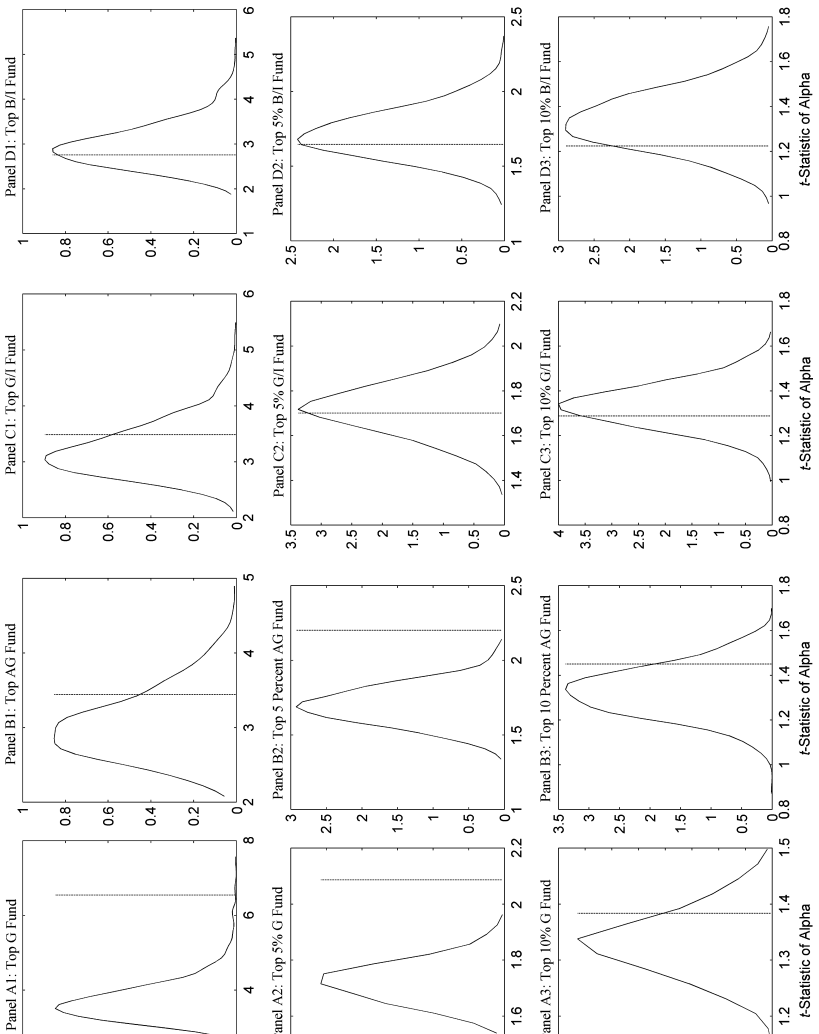

Figure 4. Estimated alpha *t*-statistics vs. bootstrapped alpha *t*-statistic distributions for individual funds at various percentile points in the cross section, by investment objective. This figure plots kernel density estimates of the bootstrapped distribution of the *t*-statistic of alpha (solid line) for U.S. equity funds with at least 60 monthly net return observations during the 1975 to 2002 period. Panels A1–A3 show results for growth funds (G), Panels B1–B3 for aggressive growth funds (AG), Panels C1–C3 for growth and income funds (G/I), and Panels D1–D3 for balanced or income funds (B/I). The funds are marginal funds in the right tail of the distribution. The *x*-axis shows the *t*-statistic, and the *y*-axis shows the kernel density. The dashed vertical line represents the actual *t*-statistic of alpha. The results are based on the unconditional four-factor model.

IV. Sensitivity Analysis

In the last section, we conduct an extensive set of resampling tests to determine the significance of alpha and t-statistic outliers. In this section, we test whether our results are sensitive to changes in the nature of the bootstrap procedure, to the assumed return generating process, or to the set of mutual funds included in the bootstrap tests. In general, we show that our main findings in this paper are robust to changes in these parameters.

A. Time-Series Dependence

Our bootstrap results assume that, conditional on the factor realizations, the residuals are independently and identically distributed. While this may seem to be a strong assumption, it allows for conditional dependence in returns through the time-series behavior of the factors. In addition, this simple bootstrap has some robustness properties that apply if the IID assumption is violated (see, for example, Hall (1992)).

Nevertheless, as a robustness check, we explicitly allow for dependence in return residuals over time by adopting the stationary bootstrap suggested by Politis and Romano (1994), which resamples data blocks (returns) of random length. Specifically, the Politis and Romano approach draws a sequence of IID variables from a geometric distribution to determine the length of the blocks and draws a sequence from a uniform distribution to arrange the blocks to yield a stationary pseudo-time series.

To explore the sensitivity of our results, we compare the bootstrap results under a block length of one monthly return (which is the same as our previous independent resampling) and for larger block lengths, up to a maximum block length of 10 monthly returns. In unreported results, we find that, with all block lengths greater than one, estimated alphas, t-statistics, and bootstrapped p-values are almost identical to the results from our baseline block length of one (see Section III).

B. Residual and Factor Resampling

Next, we implement a bootstrap with independent resampling of regression residuals and factor returns. This approach investigates whether randomizing factor returns changes our results, due, perhaps, to breaking a persistence (autocorrelation) in these factor returns. In a later section we consider the effect on our bootstrap if a potential missing factor from our model has persistent returns.

When resampling factor returns, we use the same draw across all funds (to preserve the correlation effect of factor returns on all funds), giving (for bootstrap iteration b and fund i) independently resampled factors and residuals

$$\left\{RMRF_t^b, SMB_t^b, HML_t^b, PR1YR_t^b,\ t = \tau_{T_{i0}}^b, \ldots, \tau_{T_{i1}}^b\right\} \text{ and } \left\{\hat{\epsilon}_{i,t}^b,\ t = s_{T_{i0}}^b, \ldots, s_{T_{i1}}^b\right\}. \tag{5}$$

Next, for each bootstrap iteration b, we construct a time series of (bootstrapped) monthly net returns for fund i, again imposing the null hypothesis of zero true performance ($\alpha_i = 0$),

$$\{r^b_{i,t} = \hat{\beta}_i RMRF^b_{t_F} + \hat{s}_i SMB^b_{t_F} + \hat{h}_i HML^b_{t_F} + \hat{p}_i PR1YR^b_{t_F} + \hat{\epsilon}^b_{i,t_\epsilon}\}, \tag{6}$$

for $t_F = \tau^b_{T_{i0}}, \ldots, \tau^b_{T_{i1}}$ and $t_\varepsilon = s^b_{T_{i0}}, \ldots, s^b_{T_{i1}}$, the independent time reorderings imposed by resampling the factor returns and residuals, respectively, in bootstrap iteration b. Again, in unreported results, we find that this approach exhibits almost identical results (for both left-tail and right-tail funds) to those of our residual-only approach in Section III.

C. Cross-sectional Bootstrap

In empirical tests, we find that the cross-sectional correlation between fund residuals is very low; the average residual correlation is 0.09 for the four-factor Carhart model. Nevertheless, we refine our bootstrap procedure to capture any potential cross-sectional correlation in residuals by implementing an extension that draws residuals, across all funds, during identical time periods. For example, funds may herd into, or otherwise hold the same stocks at the same time, inducing correlations in their residuals. This herding may be especially important in the tails of the cross section of alphas.

In this procedure, rather than drawing sequences of time periods t_i that are unique to each fund i, we draw T time periods from the set $\{t = 1, \ldots, T\}$ and then resample residuals for this reindexed time sequence across *all* funds, thereby preserving any cross-sectional correlation in the residuals. Since, as a result, some funds may be allocated bootstrap index entries from periods when they did not exist or otherwise have a return observation, we drop a fund if it does not have at least 60 observations during the reindexed time sequence. Again, unreported tests show that these results are almost identical to our baseline results of Section III.

D. Portfolios of Funds

In order to determine whether our analysis of individual fund alphas in the cross section, or t-statistics of individual fund alphas, substantially affects our inference tests, we consider the corresponding average statistics for portfolios of funds in each tail of the alpha (or t-statistic) distribution. This cross-sectional smoothing provides a robustness check of our results. For example, in the left tail of the alpha distribution, our first portfolio consists of all funds that have an alpha (or, alternatively, t-statistic) that lies in the lowest one percentile of the alpha (t-statistic) distribution of all funds. Our second portfolio consists of all funds that have an alpha that lies in the lowest two percentiles of the alpha (t-statistic) distribution, etc. Analogous portfolios are formed for the right tail of the alpha (t -statistic), distributions. We calculate the average alpha (t -statistic) for funds in each of these portfolios, as well as the associated bootstrapped

p-value for each portfolio. For these portfolio tests, the p-value tells us the probability of observing the average t-statistic that we actually do observe under the imposed assumption of a zero true t-statistic. This test is similar to bootstrapping an F-test to determine whether we can jointly reject the null that all funds in a certain portion of the tail do not have a performance measure that deviates from zero. Again, in unreported tests, we find strong evidence that both the left-tail and right-tail alpha t-statistics observed for the portfolios of funds cannot be attributed simply to luck.

E. Length of Data Records

Short-lived funds tend to generate higher dispersion and, therefore, more extreme alpha estimates, than long-lived funds. This leads to nontrivial heteroskedasticity in the cross section of alpha estimates. In an attempt to correct for this effect, the baseline bootstrap results impose a minimum of 60 observations in order to exclude funds that are very short-lived; in addition, we base most tests on the t-statistic, which is less sensitive to these variance outliers. However, it is possible that this minimum return requirement may impose a survival bias on our results; bootstrapping may be less (or more) necessary for proper inference when we do not impose such a requirement.

To explore this concern, we vary the return requirement to include funds that have at least 18, 30, 90, and 120 months of observations, respectively. The results, shown in Table V, strongly indicate that our inference about the tails of the performance distribution remains qualitatively similar as we move from a requirement of 18 monthly returns to a requirement of 120 returns. Note that a requirement of 120 months eliminates the extreme left and right tails, but the bootstrap responds by moving deeper into the distribution to identify outperforming funds. Thus, the bootstrap performs consistently across different types of data inclusion rules.

F. Persistent Omitted Factor in Fund Residuals

Our return-generating models assume that there is no persistent source of variation in fund residuals. If a persistent nonpriced factor (e.g., an oil shock factor) is missing from our model, then perhaps we might erroneously conclude that the alpha of a fund with a loading on this factor (e.g., an energy fund) is extreme due to manager talent. Thus, it is important to test for the possibility of a persistent missing factor in our models.

We find two pieces of evidence against such a missing nonpriced factor. First, the argument of a missing factor is only plausible for a relatively short-lived fund that exists only during the period in which this factor affects fund returns. Our results in the prior section show that our broad conclusions hold, whether we require a fund to exist for 18 or 120 months.

Second, we apply a variation of the bootstrap that uses a Monte Carlo simulation of a hypothetical omitted factor in the residuals. To capture the persistent nature of the omitted factor, we model it as a slowly mean-reverting AR(1) process and choose parameter values that are consistent with the observed fund

Table V

Sensitivity Analysis of the Cross Section of Mutual Fund Alphas to Minimum Number of Observations

In Panel A, all U.S. open-end domestic equity funds that have at least 18 monthly net return observations during the 1975 to 2002 period are ranked on the t-statistics of their unconditional four-factor model alphas. The first row shows the t-statistics of the unconditional alphas. The second row reports the cross-sectionally bootstrapped p-values of the t-statistics. For comparison, the third row shows the p-value of the t-statistic, based on standard critical values. Panels B, C, D, and E report the same measures as Panel A, but for funds with a minimum number of observations of 30, 60, 90, and 120 monthly net return observations, respectively. In each panel, the first columns on the left (right) report results for funds with the five lowest (highest) t-statistics, followed by results for marginal funds at different percentiles in the left (right) tail of the distribution. The cross-sectionally bootstrapped p-value is based on the distribution of the best (worst) funds in 1,000 bootstrap resamples. The t-statistics of alpha are based on heteroskedasticity- and autocorrelation-consistent standard errors.

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel A: Minimum of 18 Observations Per Fund																				
t-Unconditional Alpha	−7.9	−6.1	−6.0	−4.8	−4.5	−3.8	−3.1	−2.5	−2.1	−1.4	0.7	1.3	1.9	2.2	2.7	3.5	3.5	4.1	4.2	6.6
Cross-sectionally Bootstrapped p-Value	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	1.00	0.93	0.01	<0.01	0.04	0.20	0.34	0.06	0.15	0.03
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.02	0.08	0.25	0.10	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Panel B: Minimum of 30 Observations Per Fund																				
t-Unconditional Alpha	−7.9	−6.1	−6.0	−4.8	−4.5	−3.7	−3.1	−2.5	−2.1	−1.4	0.7	1.3	1.9	2.3	2.7	3.5	3.5	4.1	4.2	6.6
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	1.00	0.83	<0.01	<0.01	0.02	0.11	0.20	0.03	0.09	0.01
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.02	0.08	0.24	0.10	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Panel C: Minimum of 60 Observations Per Fund																				
t-Unconditional Alpha	−7.9	−6.1	−6.0	−4.8	−4.5	−3.6	−3.0	−2.4	−1.9	−1.4	0.8	1.4	2.0	2.3	2.8	3.5	3.5	4.1	4.2	6.6
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	1.00	0.25	<0.01	<0.01	<0.01	0.04	0.08	0.01	0.03	<0.01
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.09	0.23	0.09	0.03	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Panel D: Minimum of 90 Observations Per Fund																				
t-Unconditional Alpha	−7.9	−6.1	−6.0	−4.8	−4.5	−3.6	−2.6	−2.4	−1.9	−1.4	0.8	1.4	1.9	2.2	2.6	3.1	3.2	3.4	3.5	3.5
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.97	0.06	0.01	<0.01	0.13	0.13	0.16	0.11	0.30	0.64
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.09	0.22	0.08	0.03	0.01	0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Panel E: Minimum of 120 Observations Per Fund																				
t-Unconditional Alpha	−7.9	−6.1	−4.8	−4.5	−3.8	−3.6	−2.6	−2.4	−2.0	−1.3	0.9	1.5	2.0	2.3	2.7	3.1	3.2	3.4	3.5	3.5
Cross-sectionally Bootstrapped p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.46	0.01	<0.01	<0.01	0.01	0.02	0.02	0.02	0.12	0.40
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.03	0.10	0.19	0.07	0.02	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01

data. Details of the Monte Carlo simulation are available from the authors upon request. These results are broadly consistent with our baseline results, thus it is very unlikely that an omitted factor is driving our main performance results.

G. Bootstrap Tests for Stockholdings-Based Alphas

To further examine the robustness of our results, we examine the importance of the bootstrap for evaluating the stockholdings-based performance measure of Daniel et al. (1997; DGTW). This "Characteristic Selectivity Measure," computed by matching each stock held by a fund with a value-weighted portfolio of stocks that have similar size, book-to-market, and momentum characteristics, is given by

$$CS_t = \sum_{j=1}^{N} \tilde{w}_{j,t-1}\left(\tilde{R}_{j,t} - \tilde{R}_t^{b_{j,t-1}}\right), \tag{7}$$

where $\tilde{w}_{j,t-1}$ is the portfolio weight on stock j at the end of month $t-1$, $\tilde{R}_{j,t}$ is the month t buy-and-hold return of stock j, and $\tilde{R}_t^{b_{j,t-1}}$ is the month t buy-and-hold return of the value-weighted matching benchmark portfolio. The construction of the benchmarks follows the procedure in Daniel et al. (1997).

This measure provides not only an analysis of fund returns before all costs, but also an alternative benchmarking approach. Specifically, if portfolios of certain types of stocks, such as small-capitalization value stocks, are able to outperform the Carhart four-factor model, we should observe funds that hold predominantly these stocks in the right tail of our cross section of alphas. If this outperformance is due to nonlinear factor return premia, then the DGTW matching procedure should provide an improved benchmark for such stocks.

Analogous to our prior application of the bootstrap, for each fund we bootstrap the characteristic selectivity (CS) performance measure by subtracting the time-series average CS measure from each month's measure to arrive at a demeaned CS residual. Bootstrapping the fund return is then simple—we resample these demeaned residuals to generate a bootstrapped sequence of monthly residuals, and we compute the bootstrapped fund performance as the average residual and the t-statistic of the average residual. This procedure is repeated for 1,000 bootstrapped iterations for each fund, and the cross-sectional distribution of CS measures is constructed from these bootstrap outcomes. We repeat this process 1,000 times to construct the cross-sectional empirical distribution of the time-series t-statistics of the CS measures.

Table VI reports bootstrap results for the CS performance measures. Again, we focus on a discussion of the distribution of t-statistics.

Although the distribution of CS measures is shifted slightly to the right, as we would expect from its pre-cost nature (compared to the unconditional four-factor model of Table II), the right-tail bootstrap results are very consistent with our previous after-cost results. Specifically, according to the bootstrapped t-statistic, significant performance now extends to all funds at or above the 20-percentile point (see Panel A), as opposed to the 5-percentile cut-off for our after-cost results of Table II. Thus, funds between the 5$^{\text{th}}$ and 20$^{\text{th}}$ percentiles are

Table VI

The Cross Section of Stockholdings-Based Performance Measures

In Panel A, all U.S. open-end domestic equity funds that have holdings available for at least 5 years during the 1975 to 2002 period are ranked on the CS measure introduced by Daniel et al. (1997). The first and second rows of Panel A report the CS measures (in percent per month) and the cross-sectionally bootstrapped p-values of the CS measures. For comparison, the third row reports the p-values of the t-statistic of the CS measures based on standard critical values of the t-statistic. In rows four to six funds are ranked on the t-statistics of their CS measures. The fourth row shows the t-statistics. The fifth row reports the cross-sectionally bootstrapped p-values of the t-statistic. For comparison, the sixth row shows the p-values of the t-statistic, based on standard critical values. Panels B, C, D, and E report the same measures as Panel A but for growth, aggressive growth, growth and income, and balanced or income funds, respectively. In each panel, the first columns on the left (right) report results for funds with the five lowest (highest) CS measures or t-statistics, followed by results for marginal funds at different percentiles in the left (right) tail of the distribution. The cross-sectionally bootstrapped p-value is based on the distribution of the best (worst) funds in 1,000 bootstrap resamples. The t-statistics of the CS measure are based on heteroskedasticity- and autocorrelation-consistent standard errors.

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel A: All Investment Objectives – Funds Ranked on CS Measure																				
CS (Pct/Month)	−2.4	−2.0	−1.6	−1.5	−1.5	−0.7	−0.4	−0.3	−0.2	−0.1	0.2	0.4	0.6	0.7	0.9	1.2	1.3	1.4	1.6	1.7
Cross-sectionally Bootstrapped p-Value	0.29	0.14	0.16	0.09	0.05	0.76	1.00	1.00	1.00	1.00	<0.01	<0.01	<0.01	<0.01	0.19	0.29	0.35	0.40	0.51	0.74
Parametric (Standard) p-Value	0.03	<0.01	0.16	<0.01	0.09	0.07	0.06	0.19	0.12	0.14	0.17	0.08	0.01	0.03	<0.01	<0.01	0.21	0.05	0.05	0.07
Funds Ranked on t-Statistics of CS Measure																				
t-Unconditional CS	−4.3	−3.2	−3.1	−3.0	−3.0	−2.1	−1.6	−1.4	−1.0	−0.5	1.2	1.7	2.0	2.3	2.9	3.1	3.2	3.3	3.5	3.7
Cross-sectionally Bootstrapped p-Value	0.13	0.76	0.60	0.49	0.49	1.00	1.00	1.00	1.00	1.00	<0.01	<0.01	<0.01	<0.01	<0.01	0.07	0.09	0.18	0.21	0.34
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	0.02	0.06	0.09	0.16	0.32	0.12	0.05	0.02	0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Panel B: Growth Funds Ranked on CS Measure																				
CS (Pct/Month)	−2.4	−2.0	−1.5	−1.0	−1.0	−0.8	−0.5	−0.4	−0.2	−0.1	0.3	0.4	0.6	0.7	0.9	1.1	1.2	1.4	1.6	1.7
Cross-sectionally Bootstrapped p-Value	0.14	0.05	0.11	0.78	0.65	0.90	0.94	1.00	1.00	1.00	<0.01	<0.01	<0.01	<0.01	0.25	0.25	0.26	0.21	0.31	0.57
Parametric (Standard) p-Value	0.03	<0.01	<0.01	0.18	0.16	0.30	0.03	0.20	0.17	0.22	0.28	0.18	0.03	0.04	0.03	0.01	<0.01	0.05	0.05	0.07
Growth Funds Ranked on t-Statistics of CS Measure																				
t-Unconditional CS	−4.3	−3.2	−3.0	−3.0	−2.8	−2.4	−1.7	−1.4	−1.0	−0.5	1.3	1.7	2.0	2.2	2.9	3.1	3.1	3.2	3.3	3.5
Cross-sectionally Bootstrapped p-Value	0.11	0.51	0.40	0.31	0.29	0.78	1.00	1.00	1.00	1.00	<0.01	<0.01	<0.01	0.01	<0.01	0.01	0.05	0.11	0.25	0.42
Parametric (Standard) p-Value	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	0.05	0.08	0.17	0.31	0.10	0.05	0.02	0.02	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01

(*continued*)

Table VI—*Continued*

	Bottom	2.	3.	4.	5.	1%	3%	5%	10%	20%	20%	10%	5%	3%	1%	5.	4.	3.	2.	Top
Panel C: Aggressive Growth Funds Ranked on CS Measure																				
CS (Pct/Month)	−1.5	−0.9	−0.7	−0.6	−0.4	−0.7	−0.4	−0.3	−0.2	−0.1	0.2	0.4	0.5	0.7	0.9	0.8	0.9	0.9	0.9	1.0
Cross-sectionally Bootstrapped p-Value	0.2	0.32	0.53	0.82	0.99	0.53	0.97	1.00	1.00	1.00	<0.01	<0.01	<0.01	0.01	0.17	0.06	0.05	0.17	0.39	0.68
Parametric (Standard) p-Value	0.09	0.05	0.04	0.20	0.08	0.04	0.06	0.10	0.29	0.40	0.01	0.02	0.09	0.07	0.07	0.01	<0.01	0.07	<0.01	0.07
Aggressive Growth Funds Ranked on t-Statistics of CS Measure																				
t-Unconditional CS	−1.8	−1.6	−1.6	−1.6	−1.4	−1.6	−1.3	−1.2	−0.8	−0.4	1.2	1.7	2.1	2.3	2.8	2.5	2.7	2.8	2.9	3.7
Cross-sectionally Bootstrapped p-Value	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	<0.01	<0.01	<0.01	0.02	0.04	0.07	0.04	0.04	0.13	0.05
Parametric (Standard) p-Value	0.04	0.05	0.05	0.06	0.08	0.05	0.09	0.12	0.21	0.34	0.12	0.04	0.02	0.01	<0.01	0.01	<0.01	<0.01	<0.01	<0.01
Panel D: Growth and Income Funds Ranked on CS Measure																				
CS (Pct/Month)	−1.6	−0.9	−0.4	−0.4	−0.3	−0.4	−0.3	−0.2	−0.2	−0.1	0.1	0.2	0.4	0.5	0.5	0.5	0.5	0.5	0.5	0.7
Cross-sectionally Bootstrapped p-Value	0.15	0.07	0.92	0.92	0.92	0.92	0.98	0.91	0.89	1.00	0.08	0.18	<0.01	0.01	0.40	0.12	0.26	0.40	0.65	0.74
Parametric (Standard) p-Value	0.16	<0.01	<0.01	0.17	0.08	<0.01	0.10	0.03	0.22	0.16	0.15	0.06	0.08	0.01	0.06	0.01	0.06	0.06	0.01	0.04
Growth and Income Funds Ranked on t-Statistics of CS Measure																				
t-Unconditional CS	−3.1	−2.6	−2.6	−2.0	−2.0	−2.6	−1.6	−1.5	−1.1	−0.6	1.0	1.6	2.0	2.4	2.7	2.4	2.4	2.7	2.9	3.0
Cross-sectionally Bootstrapped p-Value	0.42	0.55	0.37	0.94	0.86	0.37	0.99	0.97	0.96	1.00	0.02	0.02	0.03	0.02	0.13	0.13	0.26	0.13	0.20	0.49
Parametric (Standard) p-Value	<0.01	<0.01	0.01	0.02	0.02	0.01	0.06	0.07	0.13	0.28	0.15	0.06	0.02	0.01	<0.01	0.01	0.01	<0.01	<0.01	<0.01
Panel E: Balanced or Income Funds Ranked on CS Measure																				
CS (Pct/Month)	−0.4	−0.3	−0.3	−0.2	−0.2	−0.4	−0.4	−0.3	−0.3	−0.1	0.2	0.3	0.4	1.3	1.3	0.2	0.2	0.3	0.4	1.3
Cross-sectionally Bootstrapped p-Value	0.69	0.42	0.34	0.49	0.40	0.69	0.69	0.42	0.34	0.79	0.13	0.27	0.39	0.22	0.22	0.13	0.25	0.27	0.39	0.22
Parametric (Standard) p-Value	0.23	0.10	0.01	0.17	0.21	0.23	0.23	0.10	0.01	0.24	0.05	0.10	0.03	0.21	0.21	0.17	0.05	0.10	0.03	0.21
Balanced and Income Funds Ranked on t-Statistics of CS Measure																				
t-Unconditional CS	−2.4	−1.3	−1.0	−0.8	−0.7	−2.4	−2.4	−1.3	−1.0	−0.7	1.1	1.7	1.9	2.4	2.4	1.3	1.7	1.7	1.9	2.44
Cross-sectionally Bootstrapped p-Value	0.32	0.81	0.88	0.86	0.79	0.32	0.32	0.81	0.88	0.69	0.17	0.18	0.23	0.23	0.23	0.16	0.05	0.18	0.23	0.23
Parametric (Standard) p-Value	0.01	0.1	0.17	0.21	0.23	0.01	0.01	0.10	0.17	0.24	0.14	0.05	0.03	0.01	0.01	0.10	0.05	0.05	0.03	0.01

skilled, but cannot generate performance sufficient to overcome their expenses and trading costs. It is important to note that the t-statistic bootstrap finds no evidence of underperforming mutual funds, gross of expenses and trading costs. It is easy to understand this outcome, as one cannot imagine a fund manager who perversely attempts to underperform her benchmarks. Thus, the significant underperformance documented in Table II is entirely due to funds that cannot pick stocks well enough to cover their costs, and not to funds that somehow consistently choose underperforming stocks. Again, outperformance is much more prevalent among growth-oriented than income-oriented funds. In addition, inference based on the bootstrap deviates substantially in both the left and right tails of the distributions.

V. Performance Persistence

Our analysis in Sections III and IV demonstrates that the performance of the top aggressive growth and growth managers is not an artifact of luck. This finding implies that some level of persistence in performance is present as well, although the extent and duration of such persistence is not yet known. Persistence is also an interesting issue in light of Lynch and Musto (2003), who predict persistence among winning funds, but not among losers, and Berk and Green (2004), who predict negligible persistence among winning funds. Following these papers, we measure persistence in fund performance, net of trading costs and fees.

Perhaps the most influential paper on performance persistence is Carhart (1997), which tests whether the alpha from the unconditional four-factor model persists over 1- to 3-year periods. Carhart's general results are that persistence in superior fund performance is very weak to nonexistent.[25] To test the robustness of Carhart's results, we implement a bootstrap analysis of the Carhart sorting procedure (rather than Carhart's standard t-tests) to evaluate the significance of the future alphas of past winning and losing funds. The application of the bootstrap will sharpen the estimates of p-values, but will not change the alpha point estimates from those of Carhart.[26]

[25] In general, other studies find similar results: Gruber (1996) finds weak persistence among superior funds; Bollen and Busse (2005) find evidence of very short-term persistence (at the quarterly frequency); Teo and Woo (2001) find that losing funds strongly persist for up to 6 years; and Wermers (2005) finds strong evidence of multiyear persistence in superior growth funds, but at the stockholdings level (pre-expenses and trading costs).

[26] However, two further differences in our study are also important to note, and they affect the point estimates. First, our data set covers the 1975 to 2002 (inclusive) period, while Carhart's covers the 1962 to 1993 (inclusive) period. Second, and more importantly, we combine share classes into portfolios before ranking funds on net returns at the portfolio level, while Carhart ranks share classes directly. Our approach therefore reduces the influence of small share classes, especially during the latter years of our sample period. In addition, share classes of a single portfolio, by construction, have almost perfectly correlated net returns, the only difference being due to uneven changes in expense ratios across the share classes during the time period under consideration. This invalidates the assumption of independent residuals in cross-sectional regressions. This consideration is only important during the post-1990 period, when multiple share classes became significant in the U.S. fund industry.

In our baseline persistence tests, we rank funds using the alpha (intercept) of the unconditional four-factor model measured over the 3 years prior to a given year-end. For example, funds are first ranked on January 1, 1978 by their four-factor alphas over the period 1975 to 1977, and the excess returns of funds are measured over the following year (1978, in this case).[27] We repeat this process through our last ranking date, January 1, 2002. Four-factor alphas are then computed for equal-weighted ranked portfolios in the cross section, which consists of different funds over time. That is, to remain consistent with Carhart, we form equal-weighted portfolios of funds rather than examine individual funds (as we do in prior sections of this paper), with the exception of the very top and bottom funds.[28]

Panel A of Table VII shows that the top-ranked fund (the identity of which changes over the years), ranked by its lagged 3-year alpha, generates a test-year alpha of 0.48% per month, which is significant using either the cross-sectionally bootstrapped right-tail p-value, or the right-tail p-value that corresponds to a simple t-test (labeled as "one-tailed parametric p-value of alpha). In fact, the standard t-test provides an inference similar to that of the bootstrap for many ranked-fund fractiles, except that the bootstrap provides the very important insight that the top decile consists of skilled funds. Consistent with Carhart, our standard t-test does not reject the null of no performance for these funds; however, the bootstrap strongly rejects the null.[29]

Some statistics that describe these fractile portfolio distributions are helpful in understanding why the bootstrap differs from the standard t-test. Panel A provides a normality test (Jarque–Bera) as well as a measure of standard deviation, skew, and kurtosis for each portfolio. Note that the standard deviations indicate heterogeneity in risk-taking in the cross section, and the skew and kurtosis measures indicate some important nonnormalities in portfolio returns. Either of these factors can explain why the cross-sectional bootstrap differs from the standard t-tests implemented by Carhart, as we discuss above in Section I.A.

[27] Funds are required to have 36 monthly return observations during the 4 years prior to the ranking date, but need not have complete return information during the test year (to minimize survival bias). Weights of portfolios are readjusted whenever a fund disappears during the test year.

[28] To generate bootstrapped p-values of the t-statistic, we follow a procedure analogous to the bootstrap algorithm described in Section I.B. Specifically, we bootstrap fund excess returns during the test year using factor loadings estimated during the prior 3-year period under the null of a zero true alpha during the test year for each ranked fund or portfolio of funds. This process is repeated for each test year to build a full time series of test-year bootstrapped excess fund (or portfolio of fund) returns over all test years for each ranking point in the alpha distribution. We next estimate the alpha and t-statistic of alpha for each fractile portfolio (or individual fund), then repeat the above for all remaining bootstrap iterations. Thus, funds are ranked on their 3-year lagged alphas, but inferences are made based on the bootstrapped distribution of their test-year t-statistics.

[29] In general, inferences using standard t-tests agree with those of the bootstrap in the left tail, mainly because their underperformance is so large. These past losing funds exhibit even higher levels of skewness and kurtosis than past winning funds. In addition, we find positive and significant alphas for the spread portfolios (e.g., the top minus bottom 10%, labeled "sprd 10%"), which is not surprising based on the strong results for high- and low-ranked funds.

Table VII

Bootstrap Performance Persistence Tests—All Investment Objectives

In Panel A, mutual funds are sorted on January 1 each year (from 1978 until 2002) into decile portolios based on their unconditional four-factor model alphas estimated over the prior 3 years. We require a minimum of 36 monthly net return observations for this estimate. For funds that have missing observations during these prior 3 years, observations from the 12 months preceding the 3-year window are added to obtain 36 observations. This assures that funds with missing observations are not excluded. The portfolios are equally weighted monthly, so the weights are readjusted whenever a fund disappears. Funds with the highest past 3-year return comprise decile 1, and funds with the lowest comprise decile 10. The "5%ile" portfolio is an equally weighted portfolio of the top5% funds. The last four rows represent the difference in returns between the top and bottom deciles (5 percentiles, 1 percentiles), as well as between the ninth and tenth deciles. In Panel B, the portfolios are formed based on past 1-year alphas, and funds are held for 1 year. Column five reports the one-tailed parametric p-value of alpha. Columns six and seven report the cross-sectionally bootstrapped p-values for the t-statistic of alpha. Column six reports the probability that the bootstrapped t-statistic of alpha is lower than $(-|t(\text{alpha})|)$, i.e., the left tail of the bootstrapped distribution. Column seven reports the probability that the bootstrapped t-statistic of alpha is higher than $(+|t(\text{alpha})|)$, i.e., the right tail of the bootstrapped distribution. Columns 12 and 13 report the adjusted R-squared and the annual expense ratio. The expense ratio is calculated as the time-series average of the cross-sectional average of the expense ratios of the funds in the portfolios. The last three columns report the skewness, kurtosis, and the p-value of the Jarque-Bera nonnormality statistics. RMRF, SMB, and HML are Fama and French's (1993) market proxy and factor-mimicking portfolios for size and book-to-market equity. PR1YR is a factor-mimicking portfolio for 1-year return momentum. Alpha (in percent per month) is the intercept of the model.

Panel A: 3-Year Ranking Periods, 1-Year Holding Period

Fractile	Excess Ret. (Pct/ Month)	Std. Dev.	Alpha (Pct/ Month)	t-Stat of Alpha	One-Tailed Parametric p-Value of Alpha	Bootstr. p-Value of t(Alpha) (Left Tail)	Bootstr. p-Value t(Alpha) (Right Tail)	RMRF	SMB	HML	PR1YR	Adj. R^2	Exp. Ratio	Skew.	Kur.	p-Value (JB-Test)
Top	1.04	7.64	0.48	1.5	0.06	0.05	0.04	0.91	0.46	−0.56	0.19	0.65	1.04	0.2	5.2	<0.01
1%ile	0.54	6.79	0.11	0.7	0.24	0.38	0.20	1.03	0.53	−0.45	−0.06	0.89	0.98	−0.2	4.6	<0.01
5%ile	0.58	5.65	0.12	1.3	0.09	0.13	0.03	0.97	0.40	−0.27	−0.04	0.95	1.01	−0.1	5.0	<0.01
1.Dec	0.57	5.17	0.08	1.1	0.13	0.23	0.05	0.95	0.33	−0.15	−0.04	0.96	0.97	−0.2	4.7	<0.01
2.Dec	0.53	4.42	0.02	0.3	0.39	0.60	0.23	0.90	0.15	−0.02	−0.01	0.97	0.87	0.3	4.6	<0.01
3.Dec	0.53	4.21	−0.02	−0.4	0.35	0.45	0.27	0.90	0.10	0.06	0.01	0.98	0.86	0.3	4.7	<0.01
4.Dec	0.50	4.06	−0.02	−0.3	0.38	0.57	0.15	0.88	0.08	0.06	−0.01	0.98	0.84	0.1	4.4	<0.01
5.Dec	0.49	4.13	−0.04	−1.0	0.16	0.32	0.04	0.90	0.05	0.08	−0.01	0.98	0.86	0.3	5.3	<0.01
6.Dec	0.46	4.04	−0.08	−2.3	0.01	0.03	<0.01	0.89	0.03	0.08	0.01	0.98	0.85	0.4	5.8	<0.01
7.Dec	0.48	4.12	−0.09	−1.9	0.03	0.03	<0.01	0.90	0.05	0.10	0.02	0.97	0.88	0.2	4.8	<0.01
8.Dec	0.52	4.08	−0.07	−1.7	0.05	0.07	0.01	0.89	0.10	0.11	0.04	0.97	0.94	0.3	4.7	<0.01
9.Dec	0.50	4.25	−0.09	−1.6	0.06	0.10	0.03	0.90	0.14	0.08	0.03	0.97	1.03	0.5	6.2	<0.01
10.Dec	0.33	4.54	−0.29	−4.2	<0.01	<0.01	<0.01	0.93	0.26	0.08	0.04	0.96	1.30	0.7	5.5	<0.01
95%ile	0.15	4.76	−0.49	−6.0	<0.01	<0.01	<0.01	0.95	0.31	0.06	0.04	0.95	1.50	0.3	4.3	<0.01
99%ile	−0.22	5.66	−0.89	−5.2	<0.01	<0.01	<0.01	1.04	0.40	0.09	0.00	0.81	2.47	1.0	8.2	<0.01
Bottom	−1.02	11.69	−1.38	−2.6	<0.01	<0.01	<0.01	0.87	0.98	−0.42	−0.11	0.32	5.26	2.4	20.5	<0.01
Sprd10%	0.24	1.57	0.37	3.8	<0.01	<0.01	<0.01	0.02	0.08	−0.23	−0.07	0.36	−0.32	−0.3	4.7	<0.01
Sprd5%	0.42	2.10	0.61	4.7	<0.01	<0.01	<0.01	0.01	0.09	−0.33	−0.08	0.36	−0.49	−0.4	4.4	<0.01
Sprd1%	0.76	3.89	1.00	4.0	<0.01	<0.01	<0.01	−0.01	0.14	−0.55	−0.06	0.25	−1.49	−0.8	5.7	<0.01
Dec9-10	0.17	0.85	0.20	4.5	<0.01	<0.01	<0.01	−0.03	−0.12	0.01	0.00	0.27	−0.27	0.0	6.5	<0.01

(*continued*)

Table VII—*Continued*

Panel B: 1-Year Ranking Periods, 1-Year Holding Period

Fractile	Excess Ret. (Pct/ Month)	Std. Dev.	Alpha (Pct/ Month)	t-Stat of Alpha	One-Tailed Parametric p-Value of Alpha	Bootstr. p-Value of t(Alpha) (Left Tail)	Bootstr. p-Value t(Alpha) (Right Tail)	RMRF	SMB	HML	PR1YR	Adj. R^2	Exp. Ratio	Skew.	Kur.	p-Value (JB-Test)
Top	0.84	9.14	0.14	0.4	0.36	0.39	0.31	1.10	0.85	−0.27	<0.01	0.55	1.06	0.3	8.7	<0.01
1%ile	0.79	6.50	0.14	0.9	0.19	0.13	0.24	1.04	0.61	−0.26	0.04	0.87	1.26	0.2	3.7	0.01
5%ile	0.82	5.69	0.14	1.5	0.06	0.05	0.06	1.00	0.49	−0.17	0.09	0.93	1.09	0.6	5.1	<0.01
1.Dec	0.78	5.30	0.14	1.9	0.03	0.04	0.01	0.97	0.41	−0.14	0.07	0.95	1.04	0.7	5.9	<0.01
2.Dec	0.66	4.49	0.07	1.5	0.07	0.11	<0.01	0.92	0.20	−0.01	0.03	0.97	0.92	0.4	4.9	<0.01
3.Dec	0.58	4.21	0.03	0.6	0.27	0.53	0.08	0.90	0.12	0.04	0.01	0.97	0.88	0.6	5.4	<0.01
4.Dec	0.54	4.08	<0.01	−0.1	0.46	0.80	0.15	0.89	0.09	0.06	<0.01	0.98	0.86	0.3	5.7	<0.01
5.Dec	0.53	4.01	−0.01	−0.3	0.37	0.62	0.17	0.88	0.07	0.07	<0.01	0.98	0.85	0.5	5.0	<0.01
6.Dec	0.46	3.97	−0.06	−1.8	0.04	0.10	<0.01	0.88	0.06	0.08	<0.01	0.98	0.87	0.2	4.0	<0.01
7.Dec	0.45	4.00	−0.08	−2.0	0.02	0.08	<0.01	0.88	0.06	0.07	<0.01	0.98	0.90	0.3	5.0	<0.01
8.Dec	0.42	4.12	−0.15	−3.1	<0.01	0.01	<0.01	0.90	0.09	0.07	0.02	0.97	0.92	0.0	4.1	<0.01
9.Dec	0.40	4.17	−0.18	−3.2	<0.01	<0.01	<0.01	0.91	0.11	0.09	0.01	0.96	0.95	−0.1	4.2	<0.01
10.Dec	0.28	4.39	−0.30	−3.7	<0.01	<0.01	<0.01	0.92	0.23	0.10	−0.03	0.93	1.22	−0.1	4.5	<0.01
95%ile	0.22	4.57	−0.39	−4.5	<0.01	<0.01	<0.01	0.94	0.28	0.11	−0.03	0.92	1.35	−0.1	4.1	<0.01
99%ile	−0.03	4.96	−0.66	−5.1	<0.01	<0.01	<0.01	0.93	0.41	0.14	−0.04	0.82	1.87	−0.2	4.5	<0.01
Bottom	−0.59	9.96	−1.31	−2.7	<0.01	<0.01	<0.01	1.12	0.25	0.50	−0.17	0.23	2.20	2.8	24.8	<0.01
Sprd10%	0.51	2.22	0.44	3.9	<0.01	<0.01	<0.01	0.05	0.18	−0.24	0.10	0.39	−0.18	0.4	4.5	<0.01
Sprd5%	0.61	2.67	0.53	4.0	<0.01	<0.01	<0.01	0.06	0.21	−0.28	0.12	0.36	−0.26	0.5	4.3	<0.01
Sprd1%	0.82	3.82	0.80	3.9	<0.01	<0.01	<0.01	0.11	0.21	−0.40	0.08	0.28	−0.60	0.2	3.1	0.34
Dec9-10	0.12	0.86	0.12	2.2	0.01	0.01	<0.01	−0.01	−0.12	−0.01	0.04	0.24	−0.26	0.8	7.6	<0.01

Since our alphas (and t-statistics) are computed net of expenses (and security-level trading costs), one might wonder what level of pre-expense alphas our ranked funds generate. This question addresses whether stock-picking skills are present; that is, whether or not expenses effectively capture all of the consumer surplus that is generated. In Panel A, we report the time-series average expense ratio for ranked fractile portfolios. The top decile of funds averages a test-year expense ratio of 97 basis points, which increases our point estimate of alpha to roughly 2% per year (since time-series variability in expense ratios is trivial, this point estimate is also bootstrap-significant).[30] Interestingly, near-median funds (e.g., deciles six and seven) exhibit slightly negative alphas when expenses are added back. Thus, these fund managers appear to have some stock-picking skills, but are too inefficient at picking stocks to justify their costs. With our bottom decile of funds, even adding back expenses does not bring them above water—their trading costs apparently far outweigh any selectivity abilities.

Panel B of Table VII repeats our tests using the 1-year past unconditional four-factor alpha as the ranking variable. Shortening the ranking period is important, not from any attempt to try to mine the data for persistence, but rather because it demonstrates that shorter ranking periods result in highly ranked funds with alpha distributions that are much more nonnormal than those from longer ranking periods, and they have higher risk levels. Specifically, as Panel B shows, the skew and kurtosis deviate much more strongly from normality among almost all highly ranked funds and fund portfolios. Thus, funds with extreme positive lagged alphas, based on short-term rankings, are much more likely to hold stocks, perhaps purposely, with a temporarily high standard deviation, skewness, and kurtosis. This leads us to conclude that the bootstrap is more important when evaluating short-term persistence. Nevertheless, standard t-tests agree with most of the bootstrapped results, except that the bootstrap shows that the top three deciles of funds have significant alphas, rather than the top two (as shown by the parametric t-test).

In unreported tests, we repeat these persistence tests among investment objective subgroups, and find that growth-oriented funds exhibit strong persistence, while income-oriented funds exhibit little. These results are again consistent with our baseline (nonpersistence) bootstrap tests of performance in prior sections.

To summarize, we find some important differences from Carhart (1997) with the bootstrap applied to fund persistence. Extreme funds, based on their past alphas, exhibit portfolio returns that deviate very significantly from normality. Controlling for these nonnormal distributions reverses one of the central results of Carhart, that is, we find that performance does indeed strongly persist among the top decile of funds, ranked on their past 3-year four-factor alphas.

[30] In unreported tests, we repeat our baseline persistence tests using gross returns, that is, monthly net returns with expense ratios (divided by 12) added back. The bootstrap confirms what we would suspect: All funds, past winners and losers, have higher alphas, but bootstrap p-values do not change very much.

VI. Conclusion

We test whether the estimated four-factor alphas of "star" mutual fund managers are due solely to luck or, at least in part, to genuine stock-picking skills. In particular, we examine the statistical significance of the performance and performance persistence of the "best" and "worst" funds by means of a flexible bootstrap procedure applied to a variety of unconditional and conditional factor models of performance. Our findings indicate that the performance of these best and worst managers is not solely due to luck, that is, it cannot be explained solely by sampling variability. We also uncover important differences in the cross section of funds that belong to different investment objective categories. While we find strong evidence of superior performance and performance persistence among growth-oriented funds using bootstrap tests for significance, we find no evidence of ability among managers of income-oriented funds. Inference using the bootstrap differs substantially from standard inference tests, especially in smaller samples of funds (or shorter time series) or among groups of funds with lower right-tail levels of performance.

The methods we present in this study may also prove useful in addressing other questions in finance, especially questions interested in analyzing the best and worst performing portfolios drawn from a population that has been ex post sorted. The advantages of our approach include eliminating the need to specify the exact shape of the distribution from which returns are drawn, to estimate correlations between portfolio returns (which is infeasible in the presence of nonoverlapping returns data), and to explicitly control for potential "data snooping" biases that arise from an ex post sort.

In summary, our evidence points to the need for the bootstrap in future rankings of mutual fund performance. At the very least (without the use of the bootstrap), rankings that use the appraisal ratio or the t-statistic of the alpha help to reduce (but do not eliminate) problems with ex post sorts as described above.

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