Return persistence and fund flows in the worst performing mutual funds*

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ABSTRACT

We document that the observed persistence amongst the worst performing actively managed mutual funds is attributable to funds that have performed poorly both in the current and prior year. We demonstrate that this persistence results from an unwillingness of investors in these funds to respond to bad performance by withdrawing their capital. In contrast, funds that only performed poorly in the current year have a significantly larger (out)flow of funds/return sensitivity and consequently show no evidence of persistence in their returns.

Keywords: mutual funds, persistence, fund flows.

JEL classification: G14.

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1 Introduction

In an influential study, Carhart (1997) showed that after controlling for the market return, the Fama-French book-to-market and size factors, and more importantly, momentum, there is no evidence of persistence in mutual fund returns. In addition, a number of papers[1] have identified a very strong relation between the past performance of a mutual funds and the flow of new capital into funds: superior performance is followed by an influx of capital while poor performance is followed by an outflow of capital. Taken together, these two results beg the question: If there is no persistence in performance, why do investors react by moving funds towards good performers and away from bad performers?

In a recent paper Berk and Green (2004) provide an answer to this question. They base their explanation on the idea that investors compete with each other to find skilled managers who can deliver superior performance. Superior past performance is evidence of this skill, so investors rationally move their money into the better performing funds. Because a managers' ability to deliver superior performance is assumed to feature decreasing returns to scale, the inflow of funds degrades performance. This process continues until the size of the fund reaches a point where the manager is no longer expected to outperform in the future, at which point the inflow of funds ceases. Similarly for poorly performing managers, there is an outflow of funds up to the point at which the underperformance ceases. Berk and Green (2004) thus argue that performance is not persistent precisely *because* investors chase good performance and punish bad performance.

In their paper Berk and Green (2004) argue that their model can explain most of the regularities documented in the data. But a test of a theoretical model is not simply its ability to explain facts that are already known. Rather, it should also be able to explain *new* features in the data — empirical facts that were not known at the time the model was developed. The object of this paper is to subject the Berk and Green model to this, more stringent, test.

To construct such a test, we begin with an observation documented by Carhart (1997). He finds that "except for the relative underperformance by last year's worst performing funds, the 4-factor model accounts for almost all of the cross-sectional variation in expected return on portfolios of mutual funds (p.63-5)." If Berk and Green's argument is correct, then the underperformance in the worst performing funds results from an unwillingness of investors to remove capital from these funds.

Note that when a fund does well the entire universe of investors can choose to react to this information by investing money in the fund. However, when a fund does badly, only

[1] see Chevalier and Ellison (1997), Sirri and Tufano (1998), Zheng (1999), or Del Guercio and Tkac (2002)

investors who are currently invested in the fund can withdraw money. That is, because the number of investors who can remove their money from badly performing funds is restricted to investors in these funds, one might expect that capital in those funds might not be as sensitive to past performance as in well performing funds. This is precisely what researchers have found. The performance–flow of funds relation is convex — for the same absolute level of out-performance, the capital inflows of the good performers is much larger than the outflows of poor performers. One might therefore surmise that this lack of sensitivity explains the underperformance documented in Carhart (1997). Unfortunately, because all these facts were known prior to the development of the Berk and Green model, although novel, this explanation for the existence of underperformance does not satisfy our criteria that the identified effect was not known at the time the model was developed.

To accomplish our goal of using the model to explain a previously undocumented feature of the data, we expand on this insight using an empirical observation from the mortgage industry. It is well documented in that industry that significant heterogeneity exists in homeowner's willingness to prepay their mortgages which results in a phenomenon known in the mortgage backed security market as *burnout*.[2] When interest rates fall, different pools of mortgages experience different prepayment rates; pools that have previously experienced a drop in interest rates have lower prepayment rates than pools that have not previously experienced a drop in interest rates. This behavior is attributed to homeowners heterogeneous responses to interest rate drops (presumably because they face different costs of refinancing their homes). Because the homeowners with the higher interest rate sensitivity prepay first, they exit the mortgage pool at the first drop in interest rates. Consequently a pool that has previously experienced a drop in interest rates has a disproportionate number of borrowers with low interest rate sensitivity and hence has an overall lower prepayment-interest rate sensitivity going forward. Christoffersen and Musto (2002) and Elton, Gruber, and Busse (2004) document similar heterogeneity among mutual fund investors.

We can exploit this heterogeneity to derive a new prediction for the flow of funds in the worst performing funds. As we have already argued, when a fund performs badly only the investors in the fund can react by withdrawing capital. Because the most responsive investors exit first, going forward, a poorly performing fund is likely to have a greater proportion of less responsive investors. Consequently, if such a fund experiences yet another year of poor performance, one should expect lower sensitivity of flow of funds to performance. This is precisely what we find in our empirical work below. Funds that performed poorly two years in a row have a significantly lower flow of funds–performance sensitivity than funds that have performed poorly in just the current year.

[2]See, Schwartz and Torous (1989), Deng, Quigley and Order (2000), Hayre (2002) or Fabozzi (2001)

Given these differences in the flow of funds–performance sensitivity, the Berk and Green model implies that we should observe differences in persistence — we should see more persistence amongst funds that perform poorly two years in a row than amongst funds that perform poorly in just the past year. As we report, this is exactly what we find. In fact, the persistence amongst the worst performing funds documented by Carhart (1997) appears almost completely attributable to funds that have performed poorly both in the current and prior years. Funds that have only performed poorly in the current year do not show statistically significant evidence of persistence in the following year. Taken together, our results provide strong evidence in favor of the Berk and Green model of mutual fund flows.

The remainder of the paper proceeds as follows. Section 2 reviews the literature. Section 3 explains the empirical design. Section 4 describes the data. Section 5 presents the results. Section 6 concludes.

2 Literature Review

This paper is related to two independent strands in the literature. The first strand is the research on persistence in mutual fund returns and the relation between returns and the flow of funds. The early research in this area includes Grinblatt and Titman (1992) who report persistence in mutual fund returns over five years and Hendricks, Patel and Zeckhauser (1993) who find that the relative returns of the mutual funds persist from one to eight quarters and that a hot-hand investment strategy can provide statistically significant abnormal returns. Later research questioned this evidence pointing to two possible explanations — survivorship bias and misspecification of the benchmark models of risk (Brown *et al* (1992), Brown and Goetzmann (1995), and Malkiel (1995)).

Survivorship bias does not appear to explain the observed persistence. Elton, *et al* (1996) find persistence of short-run risk-adjusted mutual fund performance from 1 to 3 years even after adjusting for the survival bias in the data and Gruber (1996) documents that in a data set relatively uncontaminated by survivorship bias, expenses, raw returns, and risk-adjusted returns can robustly predict the future performance over both one year and three year intervals. Instead, Carhart (1997) convincingly argues that model misspecification is the likely cause of the apparent persistence. After controlling for the Fama-French factors and a momentum factor and with the exception of the worst performing funds, he finds no evidence of persistence in mutual fund returns beyond one year. The one puzzle left unexplained in Carhart (1997) — persistence amongst the worst performing funds — has remained a mystery. Brown and Goetzmann (1995), who were the first to document the strong evidence of persistence amongst the worst performing funds, show that high fees alone could not account for the persistence and Carhart (1997) himself shows that expense ratios and turnover also cannot explain this persistence.

More recently researchers have uncovered evidence of persistence for short holding periods. Bollen and Busse (2001) find persistence in superior performance over time periods less than a year. Spiegel, Mamaysky and Zhang (2006) show that correcting for the systematic bias in the estimated alphas and betas may allow the selection of some funds that can produce risk-adjusted abnormal returns even after transactions costs. This evidence suggests that it takes investors up to a year to react to the information in past performance. As distinct from identifying persistence, some authors have identified predictability in mutual fund performance, based on some conditioning variables. Avramov and Wermers (2006) show that trading strategies that incorporate predictable manger skills related to industry effects, make superior returns. Cohen, Coval and Pastor (2006) form portfolios on the basis of whether past performance is related to commonality of the stocks held or the stocks traded in the fund managers' portfolios. By conditioning on fund manager skills, they find evidence of predictability after adjusting for risk.

The evidence on persistence has sparked a host of empirical studies on the relation between fund flows and performance including Gruber (1996), Chevalier and Ellison (1997), Sirri and Tufano (1998), Zheng (1999), and Del Guercio and Tkac (2002). These papers focus on the decision by investors to measures of past returns. The question of why capital flows are responsive to performance when performance is largely unpredictable, is the focus of Berk and Green (2004).

As we pointed out in the introduction, the second strand of research that is related to this paper is the evidence of heterogeneity in investors' willingness to move their capital in response to information. Christoffersen and Musto (2002) use investor heterogeneity to explain the dispersion of mutual-fund fees: some money fails to flow from worse- to better-prospect funds, increasing the density of performance-sensitive investors in better funds and performance-insensitive investors in worse funds. Elton, Gruber, and Busse (2004) examine the choices among index funds by investors and conclude that some investors are clearly making bad decisions in choosing index funds (p. 282). They argue that inferior funds can exist and prosper because there is heterogeneity in investors' willingness to move their capital.

In this paper we attribute underperformance predictability in the worst performing funds to precisely this heterogeneity — we document an an unwillingness of a group of investors to withdraw their capital from a fund when they observe poor performance. We do not provide a reason for this heterogeneity. We implicitly presuppose the existence of some investors who are disadvantaged in mobilizing their capital. Plausible explanations for the existence of such disadvantaged investors include: tax efficiency considerations, switching costs, back-end loads[3] and other embedded market frictions. Pontiff (1996) invokes similar arguments to explain predictability in closed end fund returns.

3 Empirical Design

In this section we first present a hypothesis for the persistent underperformance observed in mutual funds, and then present the empirical design that we use to test this hypothesis.

In Berk and Green (2004), returns are not predictable because investors make full and rational use of the information about funds past returns to learn about managerial ability. The resulting fund flows compete away any abnormal profits thereby ensuring that all returns are unpredictable. That is, the testable hypothesis in the Berk and Green model is:

> **H0:** If a strong flow of funds–performance relation exists then future excess returns should not be predictable.

In this paper we will test the (logically equivalent) contrapositive of this statement, that is,

> **H1:** If returns are predictable, then a strong flow of funds–performance relation should not exist.

This testable hypothesis of the Berk and Green model contrasts with the hypothesis that has been tested in previous work on the relation between the flow of funds and predictability. Prior research has looked for (and has been unable to find) evidence of the reverse causality. For example, Sirri and Tufano (1998) motivate their empirical work by arguing that current performance would drive flows most strongly when there is evidence that current performance was more likely to predict future performance. That is, the working hypothesis in that paper is that performance predictability should be associated with *larger* fund flow sensitivity, the opposite of H1.

There are two assumptions on which H0 relies. First, investors update their beliefs on future expected returns immediately and rationally. Second, investors react immediately by supplying or withdrawing funds with perfect elasticity. Neither assumption is likely to be satisfied in reality. Furthermore, the degree to which these assumptions hold is likely to vary across investors.

When a fund does well, investors who update fastest are more likely to invest. Hence, this creates a selection bias in the type of capital that flows into a successful fund — it is likely to

[3] Nanda, Narayan and Warther (2000) show that in the presence of investors with differing liquidity needs, high quality fund managers structure their funds to have high exit fees which prevent withdrawals.

come from investors with the highest elasticity of supplied capital to past performance. But more importantly for our purposes, because any investor in the world can choose to invest in a fund that has performed well, one would expect the supply of new capital to be large enough to ensure that there is no predictability going forward.

The opposite selection bias exists when a fund does poorly. Investors with high capital to past performance elasticities exit first, implying that the remaining investors have lower elasticities. Because only investors who are currently invested in the fund can exit, following a period of poor performance, the sensitivity in the flow of funds–performance relation for that fund should fall. A fund that has poor performance over an extended period of time is likely to have attenuated capital outflows. Thus, if the mechanism that ensures that performance is unpredictable is the flow of funds, the performance of these funds should be predictable: they should continue to underperform.

With these arguments in mind, we proceed to test H1 in two stages. First we identify funds that have performed poorly for two years in a row and empirically verify that these funds continue to underperform. Second, we then show that a strong flow of funds–performance relation does not exist in these funds.

4 Data

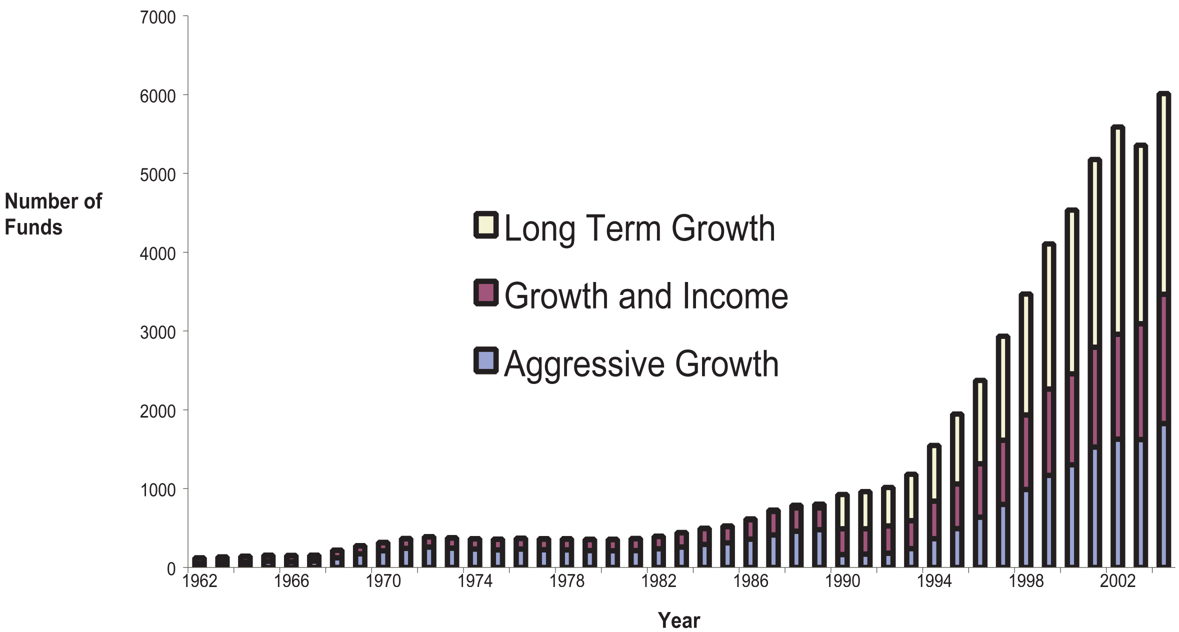
The mutual fund data is obtained from the CRSP Survivorship Bias Free Mutual Fund Database originally constructed by Carhart (1995). As in Carhart (1997) we restrict our sample to domestic, well-diversified, all-equity funds in the time period January 1962 to December 2004. As such we follow what has become common practice in this literature and exclude sector funds, bond funds, balanced funds and international funds. The details on how we selected funds are reported in the Appendix.

The monthly returns reported by CRSP is the return investors actually earn and so are net of transaction costs and expenses. To obtain annual returns at the end of December each year we compound the previous 12 monthly returns. The net asset value (NAV) or total assets under management for each fund is reported at the end of each year for the duration of the sample.

Figure 1 shows the growth in the number of mutual funds over the sample period 1962-2004, for those funds that had a minimum of 12 consecutive monthly returns at the end of any year. There are a total of 9,830 funds in our sample, which grew from 124 funds in January 1962 to 6,012 (live) funds in December 2004. A total of 3,818 funds left the sample during the sample period.

Table 1 provides further insight into how the composition of funds has changed over the

Figure 1: **Growth in Number of Equity Mutual Funds by Fund-type 1962-2004:** The plot shows the number of Aggressive Growth, Long Term Growth and Growth and Income funds over the sample period. To be included, the fund must have had a minimum of 12 consecutive monthly returns.



period of our sample. The explosion in the number of funds (especially in the last 10 years) has been accompanied by a significant increase in the number of small funds. Although mean fund size has increased five fold since the beginning of the sample, median fund size has only tripled and has in fact decreased significantly over the last 10 years of our sample. The size of the 10th percentile fund in 2004 is not much higher than it was at the beginning of our sample — indeed, 10% of funds in 2004 had values less than just $1.8 million. Another outstanding characteristic of the data is the significant drop in fees in the last 10 years most likely reflecting the increasing fraction of index funds in the sample.

There are two measures of the flow of capital into and out of mutual funds that have been used in the literature: relative flows and absolute flows. Most studies have used relative flows — the percentage change in total assets under management, net of internal growth, under the assumption that all dividends and other distributions are reinvested at the realized

Table 1: **Cross-Sectional Characteristics of the Equity Mutual Fund Sample in 1962, 1983, 1994, 2004**

		1963	1983	1994	2004
Number of Funds		133	444	1,548	6,012
of which:	Aggressive Growth	61	260	364	1,828
	Growth and Income	69	180	478	1,639
	Long Term Growth	3	4	706	2,545
Fund Size	Mean	\$90.41m	\$196.5m	\$404.4m	\$567.0m
	S.D.	\$205.52m	\$317.0	\$1,492.2m	\$3,314.3m
	10th Percentile	\$0.5m	\$7.7m	\$4.0m	\$1.8m
	Median	\$14.3m	\$75.0m	\$72.9m	\$43.8m
	90th Percentile	\$264.7m	\$548.6m	\$798.9m	\$809.0m
Age of fund	Mean	13.1 years	18.4 years	9.3 years	7.2 years
Fees	Mean	1.62%	1.46%	1.69%	0.36%

annual return for that fund:

$$Flow_{it} \equiv \frac{\mathrm{NAV}_{it} - \mathrm{NAV}_{it-1}(1 + r_{it})}{\mathrm{NAV}_{it-1}(1 + r_{it})} \tag{1}$$

where NAV_{it} is the net asset value of fund i at the end of year t and r_{it} is the realized annual return earned by investors (assuming all distributions are reinvested) from the end of year $t-1$ to the end of year t. The absolute flow of funds for fund i from the end of year $t-1$ to the end of year t is defined to be

$$Flow_{it} \equiv \mathrm{NAV}_{it} - \mathrm{NAV}_{it-1}(1 + r_{it}). \tag{2}$$

The measure of the relative flow of funds that we use, Eq. (1), differs from what is traditionally used (see Sirri and Tufano (1998); Chevalier and Ellison (1997); Zheng (1999), Del Guercio and Tkac (2002)) because it has $\mathrm{NAV}_{it}(1+r_{it})$ rather than NAV_{it} in the denominator.

That is, prior researchers have measured the relative flow of funds as:

$$\frac{\mathrm{NAV}_{it} - \mathrm{NAV}_{it-1}(1 + r_{it})}{\mathrm{NAV}_{it-1}} \tag{3}$$

As Berk and Green (2004) note, this measure does not fully capture the percentage change in new funds because it incorrectly attributes some of this change to the change due to internal growth. For example, if the fund has a very low (negative) return that results in liquidation, (i.e., NAV_{it}=0), by the definition of liquidation, the flow of funds is -100%. However, in this case (3) evaluates to a number larger than -100%.

When a fund leaves the database, CRSP identifies the reason for the fund's death. In those cases in which the fund was liquidated, merged into another fund or changed management, we assume the current fund manager lost all capital under management and set the absolute flow of funds equal to the negative of the NAV at the time and the relative flow of funds to -100%. Clearly this is the appropriate adjustment to make when the fund is actually liquidated (or the management company is replaced). But often failed funds are not actually liquidated. Instead they are merged into existing funds in the same family so the current manager effectively loses all his funds under management. Hence we include merged funds as well.[4]

Unfortunately, because of the growth in the number of very small funds in the last 10 years, the relative measure of the flow of funds presents a problem that did not exist 10 years ago. In our sample 25% of funds start with a net asset values of less than $1 million. Some of these funds stay very small and eventually drop out of the sample. Others experience growth and relative to their small initial sizes, these inflows can be extremely large. For example, in December 1996 the net asset value of *Masters Select Equity Fund* was just $1,000. One year later its net asset value was $296 Million, implying an inflow of funds of over 200,000%. As is clear from Table 2, this example is not unique — on 770 occasions fund inflows exceed 1,000%. Furthermore, this example is representative — almost all these outliers occur at the beginning of a fund's life and most of them are concentrated in funds with market values less than $1 million. Although such outliers are large on a relative basis, on an absolute basis most of these fund flow observations are small. By using percentage flows (as the past literature has generally done) rather than absolute flows as the measure of capital flows, our results would be heavily influenced by these few, economically inconsequential, outliers.

[4]Note that these adjustments increase the relative importance of flows in poorly performing funds. Including $\mathrm{NAV}_{it}(1 + r_{it})$ in the denominator of the relative flow of funds measure will increase the importance of fund flows when fund returns are low, and assuming the manager loses all funds when a fund is merged or liquidated also magnifies the flow measure for these funds. These two adjustments work against identifying a weak flow of funds–performance relation in poorly performing funds.

Table 2: **Outliers in the Relative Flow of Funds Measure**

	$\geq 100,000\%$	$\geq 10,000\%$	$\geq 1,000\%$	$\geq 100\%$	$= -100\%$
Full Sample	39	156	770	5,668	3,167
In the time period before a fund's NAV reaches:					
\$5 Million	39	155	663	2,487	1,325
\$1 Million	39	148	502	1,249	742
\$100,000	32	102	221	326	404

The table shows the number of percentage flow of funds observations that exceed the column headings. The first row uses the full sample and the other rows restricts the sample to the time period before a fund reaches the indicated NAV. The last column shows the number of observations that have percentage outflows equal to -100%.

Because almost all these outliers occur after 1995, when the number of small funds increased substantially, past studies did not encounter these data points and so using percentage flows did not present a problem.

There is no a priori reason for using percentage flows rather than absolute flows as the measure of capital flows. Consequently, we will primarily use the absolute flow of funds in our analysis. For ease of comparison to past studies we also report our results using the relative flow of funds measure. To ensure that small startup funds with very large relative but small absolute inflows do not drive our results, when we report our results for individual funds using the relative flow measure we restrict the sample to only include funds once their net asset values reach \$1 million. As is clear from Table 2 applying this selection criterion effectively removes almost all the outliers. Table 2 also shows in the final column the number of observations where a fund is liquidated or merged and has percentage flows of -100%

In our subsequent work we will first rank funds by past returns and then measure the subsequent flow of funds. At the end of year t, we rank the funds, i, on the basis of their past 12-month performance which we label r_{it}. We will also rank the best and worse performing funds based on their performance a year earlier — the twelve months preceding December of year $t-1$, which we label r_{it-1}. We then compute annual flows of these funds over the following twelve months — the twelve months following December of year t, that is, $Flow_{it+1}$

5 Results

We begin by first identifying a set of funds that have performed poorly for two years in succession. Following the methodology in Carhart (1997), at the end of each year we rank

Table 3: **Persistence in the Top and Bottom Deciles**

Prior Year	Current Year	
	Top	Bottom
Top	20.7%	12.4%
Bottom	9.4%	18.7%

At the end of each year funds are sorted into deciles based on their raw returns over the year and for the 10% of funds in the top decile the fraction that were also in the top (bottom) decile in the prior year is calculated. Similarly for the 10% of funds in the bottom decile, the fraction that were also in the top (bottom) decile in the prior year is calculated. The average value of these fractions over our sample period from 1963 to 2004 is reported in the table. Of funds that are in the top (bottom) decile in the current year the average fraction of funds that were in the same decile and the bottom (top) decile in the prior is reported.

funds by their one-year raw return over the current year, t. We then divide the funds into ten equally sized porfolios based on their performance in that year, r_{it}. The funds in the worst performing decile are further subdivided into two sub-categories: those that were also in the worst performing decile in the prior year, $t-1$ and those that were not. We label the former category (the funds that were in the worst performing decile for the last two years, in years $t-1$ and t) as *seasoned* and the latter category as *unseasoned*. For example, take 1995 as the formation year. At the end of 1995, we sort funds into ten deciles based on their returns in 1995. We then divide the bottom decile into two sub-categories. The seasoned sub-category consists of the funds that were also in the bottom decile in 1994 and the unseasoned sub-category consists of the rest of the funds in the bottom decile in 1995. As a control we repeat the same classification for the best performing (top) decile. In this case seasoned funds are funds that are in the top decile in the current and prior year and unseasoned funds are the rest of the funds in the top decile in the current year.

Under the null hypothesis that mutual fund returns are independently and identically distributed, on average only 10% of funds in the lowest decile (or one in a hundred funds) would be classed as seasoned. Table 3 shows the fraction of seasoned funds in the lowest decile is 18.7% — evidence of the persistence in realized returns that has been documented in the literature. Consistent with this evidence, Table 3 also reveals that the distribution in the top decile is similar; 20.7% of the funds are seasoned. Although Carhart demonstrated that this persistence disappears once you control for his four factors, it is nevertheless important to this study because it effectively doubles the sample of seasoned funds.

5.1 Performance

We next test for persistence in the performance of the seasoned funds in the bottom decile. For ease of comparison we follow the methodology used in Carhart (1997) as closely as possible. Thus, we estimate abnormal fund performance in the 12 months following decile formation using Carharts 4-factor specification. Measuring performance relative to the 4-factor Carhart specification has become standard in this literature. However, like Carhart (1997), we do not take a stand on whether the 4-factor specification we use is a good measure of risk. Instead we simply point out that because investors can relatively costlessly mimic the strategy on which the specification is based, investors would require any managerial talent to have at least beaten this benchmark.

Following Carhart (1997), after classifying funds into deciles at the end of the year, at the beginning of each month over the following year, we form an equally weighted portfolio of the remaining funds in each decile. We divide the bottom decile into seasoned and unseasoned funds and form two equally weighted portfolios out of the remaining funds in each of these sub-categories in each month. As a control, we also form seasoned and unseasoned portfolios for the top decile as well. We then record the return of each portfolio over the following month. We repeat this process for each month in the year and every year in the sample, leaving us with 504 monthly returns over the sample period for the decile portfolios. We need an extra years worth of data to form the sub-category portfolios, so we only have a maximum of 492 monthly returns for the sub-category portfolios. In addition there were 3 (1) years in which no seasoned funds existed in the bottom (top) decile, so for these portfolios we only have 456 (480) monthly return observations.

The excess return of each decile and sub-category portfolio is then calculated by subtracting off the monthly return on 30 day Treasury Bills. The following regression equation is then estimated:

$$r_{kt} - r_f = \alpha_k + \beta_k MKT_t + \gamma_k SMB_t + \delta_k HML_t + \lambda_k MOM_t + \varepsilon_{kt}$$

where r_{kt} is the monthly return on the k^{th} decile portfolio, r_f is the monthly return on 30 day Treasury Bills, MKT is the monthly excess return on the CRSP value-weighted aggregate market proxy for all NYSE, Amex, and Nasdaq stocks, SMB and HML are monthly returns to value-weighted, zero-cost Fama-French factor-mimicking portfolios. We augment the Fama-French factor-mimicking portfolios with Carhart's momentum factor-mimicking portfolio, which captures one year momentum in stock returns as documented by Jegadeesh and Titman (1993). The returns of the four factor-mimicking portfolios are obtained from the CRSP.

Table 4: **Post-ranking Risk Adjusted Performance of Portfolios**

Panel A: Risk-adjusted performance of portfolios

Portfolios	Average Monthly Excess Returns	α_i
Top Decile (10)	0.79%	0.0001
		(0.09)
Seasoned (10s)	0.80%	0.0008
		(0.61)
Unseasoned (10u)	0.77%	0.0000
		(0.01)
Bottom Decile (1)	0.09%	-0.0021*
		(-2.37)
Seasoned (1s)	-0.14%	-0.0052**
		(-4.10)
Unseasoned (1u)	0.12%	-0.0017
		(-1.91)

Panel B: Risk-adjusted performance of spread-position portfolios

Portfolios	Average Monthly Excess Returns	α_i
10-1	0.70%	0.0021
		(1.57)
10u-10s	-0.09%	-0.0008
		(-0.63)
1u-1s	0.28%	0.0031**
		(2.72)
10-1s	1.00%	0.0052**
		(3.16)
10-1u	0.66%	0.0018
		(1.29)

At the end of each year from 1962 to 2003, we form equally weighted portfolios of the funds in each decile. We divide the bottom and top decile (1) into seasoned (1s) and unseasoned (1u) sub-categories and form two equally weighted portfolios out of the funds in each of these sub-categories. The excess return of the portfolios is the monthly return over the 30 day T-bill rate over the 12 months following the formation year. By repeating this procedure for each year in our sample, we produce monthly excess returns for each portfolio over the period January 1963 through December 2004. The second column in Panel A lists the average excess return for each portfolio over our sample period. The third column lists intercept from the time series regression of the portfolio excess returns on the Carhart's four factors (t-statistics are reported in parentheses). In panel B, we repeat the analysis using zero cost portfolios constructed out of the original portfolio! s: 10-1 is long the 10th decile and short the 1st decile; 10s-10u is long the seasoned funds in the top decile and short the unseasoned funds in the top decile; 1u-1s is long the unseasoned funds in the bottom decile and short the seasoned funds in the bottom decile; 10-1s is long the funds in the and short the seasoned funds in the bottom decile; 10-1u is long the funds in the top decile and short the unseasoned funds in the bottom decile. Statistical significance at the 1% (5%) level is indicated with an ** (*).

Table 4, Panel A contains the average excess returns and α estimates for each portfolio. As in Carhart (1997, Table 3) there is evidence of performance persistence in the excess returns of both the top and bottom portfolios. However after applying the four-factor model, the alpha estimate of the top decile is insignificantly different from zero with a value of only 0.01%. In contrast the bottom decile still reveals persistence with a significantly negative alpha of -0.21%, mirroring Carhart's results. But more importantly, the table shows that this persistence is concentrated in the seasoned portfolio — its alpha estimate is -0.52% per month which is highly significantly negative. In contrast, the unseasoned portfolio in the bottom decile shows no statistically significance evidence of persistence at 95 per cent confidence limits: the alpha estimate of -0.17% is less than a third of the estimate for unseasoned funds. In addition the difference in the alpha estimates between seasoned and unseasoned fund in the bottom deciles is significantly different from zero — the alpha of a portfolio formed by going long unseasoned funds and short seasoned funds is 0.31% (see the "1u-1s" spread position in Table 2, Panel B). In our control sample (the top decile) the alpha estimates for both subcategories are insignificantly different from zero and each other.

In his study, Carhart emphasized the outperformance of a spread portfolio that went long the top and short the bottom decile. Somewhat surprisingly, with 12 years of additional data, Table 4, Panel B shows that this portfolio no longer provides significantly positive returns largely because the performance of the top decile in our data is not as good as it was in Carhart's sample. But more importantly from our perspective is the comparison between the spread portfolio that goes long the top decile and short seasoned funds in the bottom decile (10-1s) and the spread position that goes long the top decile and short unseasoned funds in the bottom decile(10-1u). The former spread portfolio has a significantly positive alpha, while the alpha estimate of the latter portfolio is insignificantly different from zero. Seasoned funds in the bottom decile appear to account for *all* the predictability within that decile.

Carpenter and Lynch (1999) find that these performance ranked portfolio strategies tests are the most powerful tests for identifying persistence particularly in the presence of survivorship biases. But there are alternative fund-based strategies that also test for persistence Following Brown *et al* (1992) and Grinblatt and Titman (1992), we examine whether fund-performance measured by Jensen alphas over some time period is followed by similar fund-performance over a subsequent period. To implement these fund-based tests, Jensen alphas are calculated for every fund in the dataset from a four-factor model starting in December 1964 using previous 36 months excess returns, and subject to the restriction that each fund has at least 30 observations. This follows the same criteria as in Carhart (1997). These Jensen-alpha regressions are recalculated every three years up to December 2003. Table 5

Table 5: **Predictability in Fund Level Excess Returns**

Independent Variable	(1)	(2)	(3)
Prior alpha	0.0196	0.0036	-0.0403
	(1.08)	(0.02)	(-0.19)
Decile_1*prior alpha		0.2198	0.1789
		(1.02)	(0.87)
Decile_1*prior alpha*Seasoned			0.3418**
			(3.31)
Decile_10*prior alpha		-0.1312	-0.0458
		(-0.61)	(-0.22)
Decile_10*prior alpha*Seasoned			-0.051
			(1.07)
Constant	-0.0011**	-0.0004**	-0.0007**
	(-20.02)	(-5.42)	(-6.66)
# obs	8,322	8,322	8,322
R^2	0.001	0.02	0.03

Following Brown et al (1992) and Grinblatt and Titman (1992), Jensen alphas are calculated for every fund in the dataset from a four-factor model starting in December 1964 using previous 36 months excess returns, and subject to the restriction that each fund has at least 30 observations. These Jensen-alpha regressions are recalculated every 36 months up to December 2004. The table reports the results of a pooled cross-section and time series regression of lagged alphas on alphas to determine persistence (first column). The persistence test is re-estimated with an interactive term on a dummy variable for all but one decile (Decile_i) and prior alpha (second column), though only the results for the top and bottom deciles are reported (all t-statistics are insignificant). Finally, the persistence test is re-estimated with an interactive term on a dummy variable for seasoned funds (Decile_i*Seasoned) and prior alpha (third column). In each case t-statistics on the independent variables are derived from boot-strapped standard errors because there is evidence of non-normality in the residuals. Statistical significance at the 1% (5%) level is indicated with an ** (*).

reports the results of a pooled cross-section and time series regression of lagged alphas on subsequent alphas.

In these fund-based tests, persistence is measured by the significance of the t-statistic on the prior alpha variable. As is clear from the first column of Table 5, there is no evidence of persistence in our sample. In order to examine whether persistence varies over the deciles, the persistence test is re-estimated with interactive terms on decile dummies and prior alpha (second column), though only the interactions on the top and bottom deciles are reported. None of the interaction terms are statistically significantly different from zero. Finally we re-estimated with an interactive term on the seasoned subcategory dummies and prior alpha (third column). The coefficient on the seasoned bottom subcategory interactive term is significant and positive, implying that there is evidence of positive persistence in this subcategory: for seasoned funds in the bottom decile, poor performance over one three-year period at the fund-level persists over the subsequent three-year period. No equivalent evidence of persistence exists in the top decile.

Seasoned funds in the bottom decile appear to have performance persistence. In light of this evidence, the Berk and Green model implies that such funds should display a weak flow of funds–performance sensitivity. In the next section we test this hypothesis.

5.2 Flow of Funds

Under hypothesis H1, the persistence in the worst performing seasoned funds occurs because the outflow of funds is attenuated in that subcategory. Hence, we begin by first examining the flow of funds across the deciles. We define the absolute flow of funds in a decile (subcategory) as the total flow of funds into or out of the decile. We define the percentage flow of funds similarly, that is, we compute the weighted average percentage flow of the individual funds that make up the decile (subcategory), where the weight of the i^{th} fund is $\frac{\mathrm{NAV}_{it-1(1+r_{it})}}{\sum_i \mathrm{NAV}_{it-1(1+r_{it})}}$. The average annual fund flow for each decile and subcategory is recorded in Table 6.

Not surprisingly, there is a net outflow of capital in the bottom decile and inflow of capital in the top decile. But more importantly for our purposes are the differences in the flow of funds in the subcategories. In the bottom decile the outflow of funds in the seasoned and unseasoned subcategories is not significantly different from each other even though the return in the seasoned category was 22 b.p. lower. In contrast, the return of seasoned funds in the top decile was 25 b.p. higher than unseasoned funds in the same decile and as a result these funds experienced a significantly larger inflow of capital. The fact that the difference in returns over the formation year between seasoned and unseasoned funds in the top and bottom deciles is quite similar, yet the flow of funds reaction to these returns is

Table 6: **Fund Flows and Performance in the Top & Bottom Deciles**

	Decile Portfolio	Seasoned Funds	Unseasoned Funds	t-test of the Difference
Bottom Decile (1)				
Absolute Flow of Funds	-13.797**	-13.059**	-13.456**	0.539
	(-4.42)	(-4.48)	(-3.88)	
% Flow of Funds	-0.064**	-0.096**	-0.065**	-0.807
	(-4.80)	(-3.38)	(-4.72)	
Return in % per month	-0.58**	-0.78**	-0.56**	-0.608
	(-2.43)	(-2.79)	(-2.44)	
Top Decile (10)				
Absolute Flow of Funds	71.338**	129.222**	60.123**	3.192**
	(5.46)	(4.92)	(5.08)	
% Flow of Funds	0.267**	0.566**	0.224**	2.636**
	(5.58)	(4.45)	(6.55)	
Return in % per month	2.53**	2.77**	2.52**	0.846
	(11.60)	(10.13)	(22.44)	

The table shows the average monthly flow of funds and average monthly return of the top and bottom deciles, seasoned funds and unseasoned funds for the period from January 1964 to December 2004. At the end of each year funds are sorted into deciles based on their raw returns over the previous twelve months. The bottom decile is further subdivided into two categories: funds that were also in the bottom decile in the prior year ("seasoned funds") and ones that were not in the bottom decile in the prior year ("unseasoned funds"). The flow of funds over the following 12 months is calculated for the top and bottom, seasoned and unseasoned deciles. The 12 month return over the year of the decile formation is also reported. Because the relative flow of funds measure is weighted by the size of the fund, the outliers are less important and so we use full sample in all cases. The t-statistics are in parentheses and are calculated as the time series average divided by the standard error. The final column contains the t-statistic for difference in means between the unseasoned and seasoned funds:

$$\frac{\mu_{s-u}}{\sqrt{\frac{\sigma^2_{s-u}}{N}}}$$

where μ_{s-u} is the mean annual difference in the flow on the seasoned portfolio and unseasoned portfolios, σ^2_{s-u} is the variance of the difference between the seasoned and the unseasoned portfolio, N is the number of annual observations. Statistical significance at the 1% (5%) level is indicated with an ** (*).

different suggests that the flow of funds–performance relations are different in seasoned and unseasoned funds in the top and bottom deciles.

It is well documented that past one-year performance predicts future one-year fund flows. For example, Del Guercio and Tkac (2002), Sirri and Tufano (1998) and Ellison and Chevalier (1997) all report positive coefficients when future fund flow is regressed on past performance. Although the specifications differ, all three studies find that other determinants for fund flows are also important. For example, fund age, fund size and fees have all been found to influence flows. In addition, the relation is convex — inflows have a stronger response to good performance than outflows to bad performance.

To investigate the flow of funds–performance relation in the subcategories of the bottom decile we use the controls that have been identified by prior research. So, in our regressions of future fund flows ($Flow_{it+1}$) on the prior year return (r_{it}), we include the annualized Jensen-alpha, estimated over the previous 36 months from a 4-factor model, the associated tracking error estimated as the variance of the residuals from the 4-factor model, fund size ($\log \text{NAV}_{it}$), fees, age, and the prior year fund flow ($Flow_{it}$). To account for the documented convexity in the flow of funds–performance relation, we also include the square of the prior year return (r_{it}^2). We include (but do not report) year dummies and a dummy for fund type.

We test for any differences between seasoned and unseasoned funds by including a number of interactive terms. First we include an interactive term between the prior year return and a dummy variable for the top and bottom decile. Second we include interactive terms between prior year return and whether the fund is a seasoned fund in either the top or bottom decile. These second two interaction terms therefore provide the additional effect (over and above the effect of being in either the top or bottom decile) on the flow of funds–performance relation of a fund being identified as seasoned. We have already established in Table 4 and Table 5 that seasoned funds in the bottom decile display negative persistence while unseasoned fund do not have persistence. Then, under H1, we should expect an attenuated flow of funds–performance relation amongst seasoned funds in the bottom decile, that is, a negative coefficient on the seasoned interaction term in the bottom decile. The results of these regressions are reported in Table 7.

The regressions indicate that the flow of funds–performance relation is significantly attenuated amongst seasoned funds compared to unseasoned funds in the bottom decile. In both the absolute flow regression and the relative flow regression, the interactive term for seasoned funds in the bottom decile is negative and significant. Furthermore, the size of interactive term coefficient estimates, -135.29 and -0.481 respectively, are large relative to the coefficient estimate on prior return. In fact, for both the absolute and relative flow regressions, a Wald test on the sum of the three coefficients (on Prior Year Return, Bottom

Table 7: **Fund Flow and Performance Relationship in the Top & Bottom Deciles**

Independent Variable	Absolute Flows	Percentage Flows (Restricted Sample)
Jensen's Alpha	235.807**	2.267**
	(6.79)	(8.56)
Prior Year Return	291.829**	1.446**
	(14.75)	(11.78)
(Prior Year Return)^2	-161.145**	-0.543*
	(-8.83)	(-2.00)
Tracking error	-528.96	-9.224
	(-1.43)	(-1.81)
Fund Size	-1.348	-0.11**
	(0.84)	(-8.37)
Fees	3.348	-0.029
	(0.8)	(-1.09)
Age	-0.323	-0.001*
	(-1.45)	(-1.96)
Prior Year Flow	0.568**	0.00
	(8.08)	(0.94)
Bottom Decile Interaction	-152.148** (-5.90)	-0.715** (-5.63)
Bottom Decile Seasoned Interaction	-135.29** (-5.61)	-0.481* (-2.30)
Top Decile Interaction	85.152** (2.99)	0.664** (2.62)
Top Decile Seasoned Interaction	-86.899 (1.87)	-0.64 (-1.42)
Constant	-16.038*	0.289**
	(-2.04)	(7.05)
R^2	31.09%	4.01%

The table reports OLS coefficient estimates of the determinants of fund flows from a pooled cross-section time-series regression. Dependent variables $Flow_{it+1}$ are the absolute fund flow (Eq. 2) in period t+1 (second column) and relative fund flows (Eq. 1)in period t+1 (third column). Independent variables include the Jensen-alpha, estimated over the previous 36 months from a 4-factor model, the prior year return (r_{it}), the square of the prior year return (r_{it}^2), the associated tracking error defined as the variance of residuals from the rolling 4-factor model, log of the fund's total net assets, fees, fund age and the prior year fund flow ($Flow_{it}$). In addition the product of a dummy for the top (bottom) decile with prior year's return (Top (Bottom) Decile Interaction) and the product of the prior year return with seasoned and decile dummy variables is included(Top (Bottom) Decile Seasoned Interaction). Control variables for year dummies and dummy for fund type (GI or LG) are included but not reported. t-statistics estimated from robust standard errors are given in brackets below the coefficient estimates. Statistical significance at the 1% (5%) level is indicated with an ** (*).

Decile Interaction, and Bottom Decile Seasoned Interaction) indicates that the response of seasoned funds in the bottom decile is insignificantly different from zero. In short, there appears to be no relationship between past performance and fund flows amongst seasoned funds in the bottom decile.

The contrast in the response of funds in the top decile is striking. The top decile interaction does not alter the inference on the importance of prior return — funds in this decile display a strong, statistically significant flow of funds–performance relation. Furthermore, the seasoned interaction term in this case is not statistically different from zero; the flow of funds–performance relation amongst seasoned and unseasoned funds in the top decile is statistically indistinguishable.[5]

The specification of the absolute and percentage flows regressions in Table 7 is very close to that in Del Guercio and Tkac (2002), and our estimated coefficients on the Jensen-alpha and lagged returns are of the same sign and similar order of magnitude to their results. Like Del Guercio and Tkac we find that the absolute flows equation has a much better fit (much higher R^2) than the percentage flows specification. The reason for this difference is the explanatory power of the lagged absolute fund flow variable — when that variable is left out of the former specification, the R^2 coefficient drops to 3.57% — indicating that absolute flows are very persistent. This persistence most likely derives from the heterogeneous response of investors to fund performance; some investors respond to good news immediately while others have a slower response leading to persistence in absolute fund flows. In light of these results, it is puzzling why we do not see a similar effect in the percentage flow regressions. The most likely cause of this difference is the distribution of the percentage flow variable. Although the largest outliers were removed in the restricted sample when we only included funds once their NAV reached $1 millon, many of the remaining smaller funds have extreme observations. Because an extreme inflow or outflow significantly changes the subsequent size of the fund and thereby the subsequent percentage flow, there is much less persistence in relative flows than in absolute flows. In support of this hypothesis, when we include past absolute flows in the percentage flow regression, this variable is highly significant. In any event, the significance of the decile dummy variables, and thus the main inferences of this paper, are robust to including past absolute or relative flows in either regression specification.

Taken together these results provide strong support for H1. Seasoned funds in the top decile display no evidence of predictability and also have the same flow of funds–performance

[5]The negative coefficient on the square of the prior year return might appear surprising at first, given the convex relationship documented in the prior literature. In fact, in a regression with only prior year return and prior year return squared, the coefficient is positive. Presumably, the negative coefficient in these regressions derives from the fact that other variables such as the interaction dummies also capture the convex relation.

response as other funds. Seasoned funds in the bottom decile display evidence of predictability and have a significantly attenuated flow of funds–performance relation. The predictability of seasoned funds in the bottom decile appears to derive from a reduced willingness of the investors remaining in the fund to withdraw their money.

6 Conclusions

In this paper we test the hypothesis that persistence in mutual fund returns is a consequence of an attenuated relationship between past returns and capital flows into and out of funds. Consistent with our hypothesis we document that the observed persistence in the returns of the worst performing funds can be attributed to funds that do not have a strong flow of funds-performance relation. Funds in the worst performing decile that do show evidence of a strong flow of funds-performance relation do not have persistent returns. Taken together our results provide strong support for the Berk and Green model of mutual fund flows.

Based on our results, we can explain the one remaining puzzle left unexplained in Carhart (1997). Because the persistence in the bottom decile derives exclusively from seasoned funds in that decile, this persistence results because these funds have lost their highly elastic investors, and so the remaining investors do not respond adequately to the bad performance.

The results in this paper point toward a potentially fruitful line of research. There are other sectors where mutual fund returns appear to be predictable. For example, Spiegel, Mamaysky and Zhang (2006), and Avramov and Wermers (2006) find evidence of performance predictability in monthly returns. If we accept the notion that predictability in fund returns results from an attenuation in the flow of funds relation, then it might be interesting to investigate the flow of funds–performance relation for these funds.

A Appendix: Data Selection

In the CRSP database there are four different fund type or fund objective descriptors. The first is the original Weisenberger type code which classified funds into three broad investment strategies from 1961 to 1990: Growth (G), Stability (S) and Income (I). Funds were classified as either belonging to one of these categories or combinations of these categories. In 1990 Weisenberger introduced a new, more detailed classification. Prior to 1990 a second type code exists in the data base, termed POLICY. This code broadly identifies the type of securities that the funds invest in. The third and fourth fund types are the Investment Company Data Institute (ICDI) and Strategic Insight (SI) classifications of the fund's objectives. These were both introduced in 1992 and both are much more detailed than the Weisenberger type code.

Unfortunately, the different classifications are not always consistent and it appears that sometimes a code from one objective descriptor is entered in another objective descriptor (e.g. a Weisenberger descriptor entered in a ICDI objective code). In addition the fields often have missing data. To address these issues, we decided to err on the side of including too many funds. Consequently, if a Weinsenberger code was missing in a particular year, but was available for that fund in an earlier year, we used the earlier classification in that year. If an earlier classification was not available, but a Weisenberger code was available in a later year, we used the later classification in that year. Because of the availability of the ICDI and SI codes after 1993 we only used this algorithm for the data prior to 1993. We then followed the selection criteria in Carhart (1995) (see, in particular, footnote 6), and select funds that satisfy *any one* criteria listed in Table 8. Finally! we drop funds where POLICY indicates that the fund is a balanced fund (Bal), Canadian and international fund (C & I), sector fund (Spec), money market fund (MM or TFM), or the fund invests in bond and preferred stocks (B & P), bonds (Bonds) or government securities (GS). We then assign the funds into one of three categories, aggressive growth (AG), growth and income (GI) and long-term growth (LTG), along the criteria listed in Table 8.

Most of the inconsistencies occurred either between Weisenberger and POLICY or during the period 1990-1993 when the Weisenberger classification and the ICDI/SI classifications exist together in the data. We therefore back-filled the ICDI and SI codes to January 1989. Thus, because we include a fund if it satisfies any one criteria in Table 8 we err on the side of including funds rather than rejecting them. If the inconsistency meant that categorizing the fund into AG, GI or LTG was ambiguous, we assigned the fund to LTG if that was indicated, or GI if LTG was not indicated. Our selection criteria results in a total sample of 11,318 funds that were alive for at least one month. Our sample criteria is very similar to Chen *et* *al* (2004), who report that their selection criteria generates 1,508 funds in 1993. Our criteria in comparison generates a very similar 1,513 funds at this time. We require funds to have at least 12 months of consecutive data at the end of any year, so the sample falls to 9,830 funds. The number of live and dead funds in our sample, and their distribution across fund types is given in Table 8. The growth in the sample over time is illustrated in Figure 1.

Table 8: **Fund Selection**

	Weisenberger	ICDI	SI
Aggressive Growth	G, MCG SCG	AG	AGG, FLG, SCG
Long Term Growth	G-S-I, LTG	LG, TR	FLX, GRO, ITG, VAL
Growth and Income	G-I, G-I-S, I, I-G,I-G-S, I-S, I-S-G,IEQ	GI G-S-I	GCI, GRI, IEQ, ING

The table shows the selection criteria use to form the data set in this paper. In any year, a fund is included in the data set if it satisfies any one criteria in the table. In cases where the codes were inconsistent so that categorizing the fund was ambiguous, we resolved the ambiguity by assigning the fund to Long Term Growth if that was indicated, or Growth and Income if Long Term Growth was not indicated.

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