# **Mutual Fund Surviorship**

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# MUTUAL FUND SURVIVORSHIP


## ABSTRACT

Survivorship induces a variety of biases in mutual fund research. I show analytically that biases in performance estimates depend on sample length and whether funds disappear after one or many poor returns. Using a sample free of survivor bias, I document higher risk and predominantly multiple-year underperformance in nonsurviving funds. This causes the bias in mean return estimates to increase in the time-length of the sample. In my data set, the bias is 0.43 percent per year in five-year samples and approximately one percent for samples longer than fifteen years. I also find downward bias in persistence tests and both upward and downward bias in the relations between performance and fund attributes depending on the type of selection bias. The results cast doubt on the conclusions of many published mutual fund studies.

Survivorship affects almost every mutual fund study. Commercially available mutual fund data sets include only funds currently in operation, and many commonly used research methodologies impose additional selection biases. With the exception of a few recent papers, however, researchers frequently ignore selection biases altogether or argue that their effect is insignificant. This attitude is unfortunate, as selection-bias issues pervade most empirical studies of panel data sets.

This paper offers a comprehensive study of survivorship issues in mutual fund research. I examine how survivorship affects mutual fund studies both theoretically and empirically, measuring the effects of survivorship in a new mutual fund data set carefully created to mitigate selection-bias problems. I study the effect of survivorship on three types of mutual fund studies: (1) estimates of average performance, (2) tests of performance persistence, and (3) cross-section estimates of the relation between performance and fund attributes. The analysis divides survivorship into the separate but related issues of survivor bias and look-ahead bias, an important distinction rarely acknowledged in the literature. My results indicate that survivorship substantially alters the inferences from mutual fund studies, but that the effects vary across test type, form of survivorship, and sample time length.

A number of recent papers also address issues in mutual fund survivorship. Brown, Goetzmann, Ibbotson and Ross (1992) and Carpenter and Lynch (1997) study the effects of survivorship on tests of performance persistence. In simulated samples, they show that the direction of test bias depends on variation in fund risk and the performance selection criteria. Grinblatt and Titman (1989) and Wermers (1997) study the effect of survivorship on a database of underlying stock holdings. From their estimates of gross returns, both Grinblatt and Titman and Wermers find that survivorship biases mean returns an economically small 0.20 percent per year. In addition, Wermers finds that survivorship does not significantly bias his persistence tests.

Malkiel(l995) estimates the effects of survivorship in Lipper Analytical Service's database. He finds that conditioning on ten-year survival upwardly biases mean annual return estimates by 1.4 percent per year, and that expense ratios explain one percent of the bias. Brown and Goetzmann (1995) estimate a survivor bias of 0.8 percent in their ten-year sample of mutual fund returns. They also find that nonsurvivors underperform the average fund in each of their last three years and that conditioning on two-year survival negatively biases persistence tests. Finally, Elton, Gruber and Blake (1996) study survivorship issues in the cohort of larger funds listed in the 1977 issue of Wiesenberger's *Investment Companies.* They estimate that survivorship upwardly biases annual performance measures between zero and two percent, and that the bias depends on the performance measurement model imposed.

My results suggest that nonsurvivors exhibit slightly higher total risk than survivors and disappear primarily because of multiple-year underperformance. The multiple-year performance selection criteria is predominant in nonsurvivors and causes survivor-biased estimates of average performance to increase nonlinearly in the time-length of the sample. In my sample, I measure the bias in annual return at 17 basis points for one-year samples, 43 basis points for five-year samples, and approximately one percent for data sets longer than fifteen years. This dependency of survivor bias on sample length is unrelated to Elton, Gruber and Blake's (1996) result that nonsurvivors make up a larger component of their data set as the sample time length increases. Unlike Elton, Gruber and Blake, I find that survivor bias differs across fund objective groups and does not depend significantly on the performance measurement model.

I also find that survivorship attenuates performance persistence. This result is consistent with results in Brown and Goetzmann (1995), Brown, Goetzmann, Ibbotson and Ross (1992), Grinblatt and Titman (1992) and Carpenter and Lynch (1997). However, a short-term look-ahead bias does not significantly alter inferences from persistence tests. This suggests that the downward bias from multiple-year performance survival criterion is mostly offset by an upward bias due heterogeneity in mutual fund risk.

Finally, I document significantly varying effects of survivorship in the cross-section relations between performance and mutual fund attributes. Conditioning on survival at the end of the period upwardly biases the relation between performance and expenses, turnover and load fees, and downwardly biases its relation to fund size. However, imposing a long-term look-ahead bias causes a downward bias in the performance relation to load fees and an upward bias in the relation to fund size. The biases frequently reverse inferences, casting doubt on previous studies of this issue.

My study contributes to the mutual fund literature in providing a model for the relation between survivorship criterion and bias in estimates of average performance, and in empirically measuring the bias in common mutual fund tests. In addition, I introduce a new, comprehensive and carefully-constructed data set that corrects for almost all of the selection-bias problems in previous research. Section 1 defines important terms, demonstrates theoretically how the selection process biases inferences, and presents models of performance measurement. Section 2 describes the data set and its relation to other survivor-bias correct data sets. Section 3 presents empirical estimates of survivorship for various tests in my data set, and. Section 4 concludes.

## 1 Theory

## A. *Selection bias definitions*

To mitigate potential confusion, I define some important terms used in *this* study. *Selection rule* refers the criteria which causes funds to disappear from the data set. A one-period selection rule means that only funds with current period returns greater than some threshold are observed at the end of the period. A multiple-period selection rule means that funds appear in the data set only if their past n-period return exceeds some threshold.

*Survivorship* includes two separate but related selection problems, *survivor bias* and *look-ahead bias. Survivor bias* refers to the effects of testing only on the selected sample of funds extant at the end of the time period. *Look-ahead bias* is the effect of considering only funds surviving a minimum length of time. Survivor bias is solely a property of a data set, whereas look-ahead bias usually results from a test methodology imposing a survival condition. The distinction between these two biases is not always acknowledged in the literature, as some studies consider data sets free of survivor bias but then impose look-ahead-biased methodologies. An example of a survivor-biased sample is Morningstar OnDisc, which reports performance since January 1976 only for funds still existing at the end of the sample period. In principle, correcting for survivor bias is simply an issue of data collection, although in practice the missing data is often not completely obtainable.

In contrast, the cornrnon performance persistence test methodology of regressing future n-period performance on a measure of past performance suffers from an n-period look-ahead bias, since the test conditions on survival for another *n* periods beyond the evaluation date. Some degree of look-ahead bias inherently results from any test of performance persistence due to the balanced 'future and past performance sample.' Mitigation of look-ahead bias requires minimizing the *look-ahead period,* the time period over which future performance is measured. The methodology I utilize below requires looking forward only one month.

Since a mutual fund sample is a panel data set, a method of aggregation across funds and time must also be selected. One approach is to pool all of the time-series and cross-section observations. Due to significant recent growth in the number of funds, this method skews results towards relations in final few years of the sample. A second approach calculates statistics on the individual funds, then averages cross-sectionally. This method treats a fund like Manning & Napier Tax-Managed Fund, with a performance history of one month, the same as the 35-year-old T. Rowe Price New Horizons Fund. In addition, since risk adjustment requires a minimum number of observations, this method also imposes a look-ahead bias. A third approach calculates statistics cross-sectionally for each time period, then averages these estimates through time. This approach is probably the most sensible, and is frequently employed in the mutual fund literature. Where applicable, I rely primarily on the third approach for aggregation.

*B. Survivor Bias Effects on Estimates of Average Performance*

This section demonstrates that, similar to the persistence results in Brown, Goetzmann, Ibbotson and Ross (1992), the bias in estimates of average performance in a survivor-only sample depends on the selection rule. Under relatively simple assumptions, the bias in performance estimates will be increasing in the time period of the study under a multiple-period selection rule, but independent of the time period under a single-period selection rule. I demonstrate this both analytically and with Monte Carlo simulations.

Let periodic returns, $R$, on $n$ mutual funds be independent and identically distributed with mean $\mu$ and variance c?. After observine returns for the period. funds with (single- or multiple-period) performance below the performance threshold, $b$, liquidate or merge into surviving funds, so their performance disappears from survivor-biased samples. For convenience, assume that the probability of fund disappearance each period is $x$, and that nx new funds are created each period. This keeps the mutual fund sample size approximately constant through time.

Further assume that the econometrician constructs a survivor-biased sample of mutual funds looking back $k$ periods. Thus, the sample includes up to $k$ periods performance history on all funds operating at the end of the sample. Naturally, the sample also includes the performance history on newer funds operating less than $k$ periods. As is frequently assumed, let the estimate of average performance be defined as the time-series average of period-by-period equal-weight cross-sectional averages.

***Proposition 1:*** *If a single-period selection rule causes fund disappearance, the bias in estimates of average performance will be independent of the time length of the sample.*

*Proof* In every period of the data set, the bias in the average performance estimate on surviving funds is

$$E\lfloor R|R > b\rfloor - \mu \qquad \text{Q.E.D.}$$

***Proposition*** *2: If an m-period selection rule causes fund disappearance, the bias in estimates of average performance will increase in the time length of the sample, k.*

*Proof* For convenience, let $k>m$ and $T$ denote the last period in the sample. Because the sample consists of funds of various ages, the cross-sectional average performance estimate will be a weighted average of the returns on funds of different ages. The bias in the one-period cross-sectional average performance estimate for all years prior to and including $T$-$m$ is

$$B_{\geq m} \equiv x\, E\left[R_t \mid C_{t+m}\right] + x(1-x)\, E\left[R_t \mid C_{t+m-1}, C_{t+m}\right] + \cdots + x(1-x)^{m-1} E\left[R_t \mid C_{t+1}, \ldots C_{t+m-1}, C_{t+m}\right]$$
$$+ (1-x)^m\, E\left[R_t \mid C_t, C_{t+1}, \ldots C_{t+m}\right] - \mu$$

where the conditioning statements are

$$C_t \equiv \sum_{\tau=t-m}^{t} R_\tau > b$$

However, in each successive period between $T$-$m$ and $T$, one of the conditioning statements is lost because of the impossibility of conditioning on returns beyond the end of the sample period, $T$. In period $t=T-(m-1)$ the bias is

$$B_{m-1} \equiv x\, E\left[R_t\right] + x(1-x)\, E\left[R_t \mid C_{t+m-1}\right] + \cdots + x(1-x)^{m-1} E\left[R_t \mid C_{t+1}, \ldots C_{t+m-1}\right]$$
$$+ (1-x)^m\, E\left[R_t \mid C_t, C_{t+1}, \ldots C_{t+m-1}\right] - \mu$$

while in period $t=T$, the bias is

$$B_0 \equiv (1-(1-x)^m)\, E\left[R_t\right] + (1-x)^m\, E\left[R_t \mid C_t\right] - \mu$$

Since average performance is the time-series average of periodic cross-sectional averages, the bias in the average performance estimate resulting from a survivor-biased sample of $k$ periods is

$$bias = \frac{1}{k}\left(\sum_{\tau=0}^{m-1} B_\tau + (k-m)\; B_{\geq m}\right)$$

*Now,*

$$B_i > B_j \quad \forall \quad i > j$$

since

$$E\left[R_t \mid C_t, C_{t+1}\right] = E\left[E\left[R_t \mid C_t\right] \mid C_{t+1}\right] > E\left[R_t \mid C_t\right]$$

Therefore, the bias increase in $m$. *Q.E.D.*

A simple example illustrates why a multiple-period selection rule causes the bias in average performance to increase in the time length of the sample. Suppose funds survive based on two-year performance. Then observing the first-year return on a fund surviving three years is conditioned on the sum of the first and second year returns exceeding the threshold. Similarly, observing the last-year return is conditioned on the sum of the second and third year returns. However, observing the second-year return on this fund is conditioned both on the sum of the first- and second-year returns *and* on the sum of the second- and third-year returns. Since a low second-year return requires a relatively higher return hurdle in both the first- and third-years while a high second-year return imposes a relatively lower return hurdle in the first and third years, it is more likely that the second-year return is higher than either the first or third. Multiple-period selection rules cause larger biases in average performance than a single-period selection rule because of this overlap in performance conditioning. The longer the time-period in the selection criteria, the larger will be the bias. Note also that this result is independent of the probability of survival or the increasing number of nonsurviving funds in longer samples.

This result is related to Brown, Goetzmann and Ross' (1995), who condition stock-market indices on a final price level. They find that conditioning on survival at the end of the sample upwardly biases mean return estimates by an amount that increases in the time-period of survival. Since a final price level is simply the initial price compounded by the stock-market's cumulative return, their survival rule equates roughly to conditioning on the average return over all periods.

To gauge the effects of various selection rules and time lengths of the sample, I undertake a simple Monte Carlo simulation study of mutual fund returns. I simulate independent and identically distributed returns for 1,000 mutual funds with standard deviation 5 percent per period.

This corresponds to the annualized average residual standard deviation from the CAPM in Carhart (1997).[2] For a given m-period selection rule and k-period sample, returns are generated for $k+2m$ periods. The first $2m$ periods bring the fund disappearance rate to its steady-state value. In each period, I drop funds according to the selection rule and replace them with the same number of new funds. I calibrate the threshold return, $b$, for each selection rule so that an average of 3.5 percent of funds disappear each period, the approximate annual rate reported in Carhart (1997). Maintaining the same disappearance rate across scenarios requires the threshold return to increase slightly in $m$. To calculate the bias in the average performance estimate for a given simulation, I calculate the time-series average of the last k-period cross-sectional average returns across surviving funds.

The simulation experiment is repeated 10,000 times, and the average bias in performance across simulations is reported in Table 1. As shown in propositions 1 and 2 above, the results demonstrate that survivor bias is constant for any sample time length when funds are evaluated on one-period performance, and increases in the time length of the sample under a multiple-period selection rule. In addition, the performance bias increases nonlinearly: for relatively shorter survivor-biased samples, the bias actually decreases in the length of the selection rule. As the sample length increases, the bias increases quickly. According to the simulation, a 20-year survivor-biased sample and a lo-year selection rule result in a bias in average performance estimates of approximately 0.67 percent per year.

Elton, Gruber and Blake (1996) report a similar result which is unrelated to the one above. Since they estimate performance by cross-sectionally averaging the time-series average performance across individual funds and they ignore new funds, their estimator for survivor bias increases in the number of dead funds.

c. *Survivor and Look-Ahead Bias Effects on Estimates of Persistence in Performance*

Brown, Goetzmann, Ibbotson and Ross (1992) provide a thorough theoretical analysis of the effect of look-ahead bias on estimates of persistence in mutual fund performance, where persistence is defined as a positive relation between performance rankings in an initial ranking period and the subsequent period. Under the assumptions that mutual funds returns are independently distributed with the same mean but differing variances and that a single-period selection rule causes fund disappearance, Brown et al. show that tests on biased samples show spurious persistence. Intuitively, high variance losers perish and high variance winners survive, exerting an upward bias on resulting persistence estimates conditional on survival until the end of the subsequent period. While Brown et al's analysis covers only look-ahead bias, it seems reasonable that their results would generalize survivor-biased samples. I examine this empirically in Section 3.

Brown, et al. and Grinblatt and Titman (1992) also show that independent and identically distributed returns and a multiple-period selection rule along with look-ahead bias cause reversals instead of persistence in performance. Intuitively, repeat losers disappear leaving relatively more winner-loser and loser-winner funds (reversals) than winner-winner funds (persistence.) In a simulation exercise, Carpenter and Lynch (1996) extend this result to the case of different variance funds and multiple-period selection rules. They show that even with arbitrarily large heterogeneity in variance across funds, multiple-period selection rules cause reversals in performance. They also show that their results are robust to choices of the minimum return hurdle.

Given these theoretical results, the interesting empirical questions for persistence tests are: (1) what is the primary survivorship selection rule? And (2), are nonsurviving funds riskier? My empirical results, reported in Section 3, indicate that a multinle-period selection rule dominates the survivorship process. In addition, I find that nonsurviving funds exhibit somewhat higher total risk than surviving funds. In my sample, this causes survivor bias to downwardly bias tests of persistence, consistent with the simulations in Carpenter and Lynch (1997).

### *D. Performance measurement*

I employ two methods of performance measurement. The first method simply subtracts from fund returns the equal-weight average return on all funds with the same objective in that period. I call this the "group-adjusted" performance measure. When funds change objectives, the group-adjusted measure uses the new objective average, likewise. Brown and Goetzmann (1997) document that some funds game their stated objectives to improve their relative performance, so I reconstruct the annual series of stated objectives to remove short-term objective "flips." In my data set, the change in benchmark increases prior-year's group-adjusted performance an average of only 61 basis points (t-statistic of 1.63), considerably less than the 9.8% reported by Brown and Goetzmann.

The second performance measure is the time-series regression intercept from asset pricing models, commonly called "alphas" after Jensen's (1968) work. I use two such models: the Capital Asset Pricing Model (CAPM) described in Sharpe (1964) and Lintner (1965), and Carhart's (1997) 4-factor model; For the CAPM, I use Fama and French's (1993) market proxy, updated to 1995. The 4-factor model uses Fama and French's (1993) 3-factor model plus an additional factor capturing Jegadeesh and Titman's (1993) one-year momentum anomaly. The model is:

$$r_{it} = \alpha_{iT} + b_{iT}\, RMRF_t + s_{iT}\, SMB_t + h_{iT}\, HML_t + p_{iT}\, PR1YR_t + e_{it} \qquad t = 1,2,\ldots,T$$

where $r_{it}$ is an asset return in excess of the one-month T-bill return; RMRF is the excess return on a value-weighted aggregate market proxy; and SMB, HML. and PR1YR are returns on value-weighted, zero-investment, factor-mimicking portfolios for size, book-to-market equity, and one-year momentum in stock returns. Carhart (1997) describes the 4-factor model in greater detail and finds it prices passively-managed portfolios formed on size, book-to-market equity and one-year return momentum considerably better than the CAPM or Fama and French's (1993) 3-factor model. Further, Carhart (1995) finds that dynamic performance measurement models like Ferson and Schadt (1994) do not substantially alter his performance estimates.

## 2. Data

### A. *Description and summary statistics*

My database covers diversified equity mutual funds monthly from January 1962 to December 1995 and excludes sector funds, international funds and balanced funds. The data are free of survivor bias, since they include all known equity funds over this period. I obtain data on surviving funds and for funds that disappear after 1989 from Micropal/Investrnent Company Data, Inc. (ICDI.) For all other nonsurviving funds, the data are collected from *FundScope Magazine, United and Babson Reports*, Wiesenberger's *Investment Companies*, the *Wall Street Journal*, and ICDI's past printed reports. I partition the sample into three primary investment objectives using Wiesenberger and ICDI classifications: aggressive growth, growth and income, and long-term growth. All funds in the sample start as general equity funds in one of these three objectives. Funds frequently change objectives during the sample but I never drop a fund once in the sample.

The data set includes monthly returns and annual attributes. Return series are as complete as can practically be obtained, but do not include final partial-month returns on merged funds as in Elton, Gruber and Blake (1996.) Carhart (1995) for more detail on database construction

The sample differs from Carhart (1997) in two primary ways. First, the data deal with the relatively new phenomenon of multiple share class funds. Multiple share class funds divide a common pool of assets into share classes with differing distribution costs, typically those with either a front-end load, a rear-end load, or a 12b-1 fee. ICDI treats each share class as a separate fund, and Carhart (1997) does likewise. The sample in this paper, however, includes only the original share class for each fund. For fund size, I use the sum of the total net assets over all share classes. My treatment of multiple share class funds results in the removal of 62 funds from Carhart (1997). Second, the data are extended two years to 1995 and remove several duplicate and improperly categorized funds from Carhart (1997). This results in 194 new funds along with the removal of 28 funds.

Table 2 reports annual summary statistics on the data set as well as time-series averages over the complete period. My sample includes a total of 2,071 diversified equity funds, 1,346 of them still operating as of December 3 1, 1995. In an average year, the sample includes 545 funds with average total net assets (TNA) of $179.5 million and average expenses of 1.19 percent per year.

To measure net additions and withdrawals, I also measure *Flow* as:

$$Flow_{it} = \frac{TNA_{it} - (1 + R_{it})TNA_{it-1} - MGTNA_{it}}{Avg\ Monthly(TNA_{it-1}, TNA_{it})},$$

where $MGTNA_{it}$ is the increase in fund *i*'s assets in period *t* due to merger and the denominator is the average monthly total net assets of fund *i* from *t-1* to *t*. *Flow* is similar to Sirri and Tufano's (1992) flow measure except that it adjusts for TNA changes due to merger, and it uses average monthly assets instead of beginning assets. On average, the typical fund receives net inflows of 7.0

percent per year as measured by *Flow*.

In addition, funds trade 82.5 percent of the value of their assets (*Mturn*) in an average year. Since reported turnover is the minimum of purchases and sales over average TNA, I obtain Mtum by adding to reported turnover one-half of the absolute value of *Flow*. Also, over the full sample, average maximum load fees are 7.05 percent, and 59.8 percent of funds charge them in a given year. Maximum load is the total of the maximum initial, rear and deferred sales charges, as a percentage of assets invested.

In an average year, I find that 3.6 percent of funds disappear. Of this total, 2.2 percent per year disappear due to merger and 1 .0 percent disappear because of liquidation. My estimate differs slightly from Elton, Gruber and Blake (1996), who estimate a non-survival rate of 2.3 percent in their sample. However, Elton et al. study only a single cohort of funds, so each year's sample conditions on survival for a given time period. When subdivided by investment objective, I find that aggressive growth funds perish at an annual rate of 4.5 percent, which is statistically significantly larger than 2.9 percent for long term growth and 3.3 percent for growth and income funds. In addition, unlike Elton et al., I find that the annual disappearance rate is significantly negatively related to the previous year's market return, with a t-statistic of -2.30.

The annual summary statistics indicate substantial. variation in mutual fund properties through time. For example, the rate at which assets enter and leave the industry varies, with alternating periods of high growth/low disappearance rates and low growth/high disappearance rates. In addition, the nominal size of funds (TNA) and average expense ratio mostly increase over the 34-year period, while both the load fees and the proportion of funds charging load fees decrease.

Table 2 also demonstrates that the equity mutual funds in our sample earned reported returns approximately 0.6 percent per year below the value-weighted CRSP index, occasionally under or over performing the CRSP index by as much as 9 percent per year. Reported returns are net of all operating expenses (expense ratios) and security-level transactions costs, but do not include sales charges. Perhaps more surprising, funds only hold 83.2 percent of their portfolios in common stocks in an average year. In the remainder of their portfolios, funds hold 10.2 percent in cash and 6.6 percent in preferred stocks and bonds (not reported.) Considering the large amounts invested in cash and bonds, the average fund return of only 0.6 percent below the market is surprising. In Section 3, I show that mutual funds tilt their stock portfolios toward smaller stocks, which suggests that funds earn somewhat higher returns by taking on additional risk.

*B. Comparison to Other Mutual Fund Data Sets*

While my sample is probably the most complete survivor-bias-free mutual fund database available, Brown and Goetzmann (1995), Elton, Gruber and Blake (1996), Grinblatt and Titman (1989), Malkiel (1995), and Wermers (1996) study related mutual fund databases. Elton et al. follow the cohort of funds listed in Wiesenberger's 1977 volume from 1976 until 1993 and successfully track uninterrupted return histories up to the date of merger for funds with assets greater than $15 million. My sample differs from Elton et al. in that mine includes all funds between 1962 and 1995, including new funds as they appear, and includes performance on even the smallest funds. Elton et al. also aggregate performance differently, choosing to estimate performance on individual funds first, and then averaging these estimates cross-sectionally. Their sample construction and methodology is most relevant for understanding the look-ahead bias in Ippolito (1989) and similar studies.

Grinblatt and Titman (1989) and Wermers (1996) use quarterly "snapshots" of the mutual funds' underlying stock holdings since 1975 from CDA/Spectrum to estimate returns gross of transactions costs and expense ratios, while my data set uses only the net returns. Wermers' data set permits him to make more specific statements about the investment strategies of funds and gross investment performance than mine. CDA's 1995 data set overlaps with 89.4 percent of the funds in my sample, but includes a large number of institutional and foreign-owned diversified US equity funds that are excluded from mine. In addition, the CDA data do not permit return calculations on nonsurvivors in their final periods before disappearance.

The data set studied by Brown and Goetzmann (1995) is quite similar to mine, except that it covers only the period from 1977 to 1988 and uses annual returns estimated from Wiesenberger's *Investment Companies.* As Brown and Goetzmann acknowledge, the voluntary and infrequent reporting in Wiesenberger probably upwardly biases their return estimates somewhat. Finally, Malkiel's (1995) data set uses quarterly returns from 197 1 to 199 1, obtained from Lipper Analytical Services. None of these samples completely obtains returns on nonsurvivors. Because my sample uses data from multiple sources, I believe it describes the cross-section of performance on the complete sample of mutual funds better than these related data sets.

### *C. Properties of Nonsurviving Mutual Funds*

Table 3 reports that, not surprisingly, nonsurviving funds exhibit considerably worse performance than surviving funds. After estimating the group-adjusted and 4-factor model performance on individual funds over their complete return series, I calculate the cross-sectional average of these estimates for survivors and nonsurvivors. By these measures, nonsurviving funds under-perform survivors by 3 1 basis to 36 points per month, or about 4 percent per year.

Not unexpectedly, nonsurviving funds are smaller and have higher expense ratios and turnover than surviving funds. I calculate a measure of relative size using the ratio of each fund's annual TNA to the average TNA for that year. Relative TNA is the time-series average of annual cross-sectional averages of this ratio. Relative expense ratio and turnover (Mturn) are measured identically. To measure relative flow, I use the difference in Flow instead of the ratio, again taking the time-series average of annual cross-sectional averages. By these measures, surviving funds are approximate 45 percent larger than the average fund and growing faster by 1.2 percent per year, while nonsurviving funds are less than one-third the size of the average fund and declining. Similarly, surviving funds have expense ratios about 11 percent lower than average, while nonsurvivors charge expenses 23 percent above average. Nonsurviving funds also trade about 15 percent more than the average fund, while survivors trade about 4 percent less. These results are consistent with Brown and Goetzmann (1995) and Malkiel (1995), who also document the higher expenses and smaller size of nonsurvivors.

I subdivide the defunct mutual fund sample by reason for disappearance into four broad categories: (1) mergers, (2) liquidations, (3) other self-selected reasons, and (4) not self-selected or unknown reasons. Table 3 shows that about 58 percent of all defunct funds disappear because of merger and 36 percent disappear due to liquidation. A further 2 percent vanish through other self-selected means, usually at the fund manager's request for removal.

Approximately 5 percent of nonsurviving funds depart for unknown reasons or are dropped from the sample by the database manager, not the fund itself. Sixteen of these are tax-free exchange (TFE) funds. TFEs, nonexistent today, permitted a tax-free exchange of an investor's stock portfolio for shares in the fund, allowing investors to defer capital gains recognition.[3] Congress withdrew this tax loophole in 1967 and these funds disappeared from our sources in the same year. Five funds are dropped from the sample because they are variable annuity investment vehicles, and the reason for disappearance is unknown for fifteen funds.

While all nonsurviving fund groups underperform, liquidated funds exhibit the worst relative performance and the smallest size and highest expense ratios. Liquidated funds are only five percent of the average fund's size and have expenses and turnover 85 percent and 53 percent higher than the average fund, respectively. In contrast, funds that subsequently merge earn performance similar to the average nonsurvivor. Not surprisingly, merged funds are larger and have lower expense ratios than the typical nonsurviving fund, as the revenues for maintaining the assets under management are proportional to the assets in the fund. For the smallest funds, the reorganizational costs of merger probably outweigh the present value of expected future money management revenues, forcing liquidation instead of merger.

Funds disappearing for reasons other than merger or liquidation mostly underperform, also. However, the performance on split, variable annuity and tax-free exchange funds are not abnormally negative. Performance is significantly negative for funds voluntarily removing themselves from the sample, funds reorganizing as closed-end status, and funds disappearing for unknown reasons. These findings are not surprising. Since Sirri and Tufano (1992) and others clearly show that investors respond to past performance, poorly-performing funds may stem the tide in negative flows by changing to closed-end or removing themselves from commercial mutual-fund-ranking services.

## 3. Empirical Results

*A. Evidence on the Selection Rule*

I now examine the relative performance of nonsurviving funds in their final five years of existence to ascertain whether funds disappear primarily because of a single poor return or a sequence of poor returns. As discussed in Section 1, the selection rule affects the direction, magnitude and time-dependence of survivor bias in average performance estimates and persistence tests.

Although I believe my nonsurviving fund returns are fairly complete, my data set still does not include every monthly return on every fund in my sample. Of the 725 nonsurviving funds, I obtain the date of merger, liquidation, or reorganization for 475 funds from ICDI, Wiesenberger *Investment Companies, FundScope Magazine* and *Investment Dealer's Digest.* Within the sample of funds with known termination dates, the return series end within one week of the termination date for 330 funds. Of the remaining 145 funds, 32 do not include the final partial- or full-month return, 20 do not include the final two- to three-month return, 81 do not include the final four- to twelve-month return, and 12 funds are missing more than one year's returns. Of the 250 nonsurviving funds without exact termination dates, I do not observe any returns on 53 funds, often because they are too small to appear in any published sources.

While my sample does not include a number of nonsurviving fund returns, the bias induced by the last few omitted returns is probably quite small. Since mergers and liquidations need shareholder approval, these reorganizations require at least several months to complete, and probably closer to four to six months. Thus, missing final returns probably do not differ substantially from the prior observed returns on these funds. The evidence from Elton, Gruber and Blake's (1996) sample supports this conclusion: Marty Gruber indicates that the final partial-month return on merged funds does not significantly differ from the average nonsurvivor's return.

Figure 1 suggests that nonsurvivors underperform consistently over their last five years of existence, but especially their final year. The figure presents the annual group-adjusted performance on an equal-weight portfolio of nonsurviving funds in each of their last five years.[4] This performance is gross of expense ratios in order to remove the effect of declining fund size on performance. The figure suggests that most nonsurvivors disappear after underperforming for multiple years, and perhaps also that some funds disappear after only one particularly poor final year return. However, the portfolio average does not directly reveal the distribution of individual fund performance.

The evidence in Table 4 shows that multiple-period performance dominates the selection process. The table reports the proportion of all nonsurviving funds with group-adjusted performance below various performande fractiles of all funds. In their final twelve months, 62 percent of nonsurvivors report performance below the median, and 24.8 percent report performance in the bottom decile of all funds. Similarly, over their last five years, almost 80 percent are below the median, 33 percent in the bottom decile, and 21 percent fall in the bottom 5 percent. In addition to the large proportion of individual funds that underperform over their final five years, the relative performance of nonsurviving funds worsens as the performance measurement periods lengthens. This indicates that most funds vanish after underperforming for several years; if funds disappeared after only a single poor return, the relative performance of nonsurviving funds would increase as the performance measurement period lengthened. However, there is also evidence that funds sometimes disappear after only one poor return. Relatively more funds appear in the bottom one percent nerforrnance fractilc for their last year than their last twn tn five years

*B. Performance and Risk of Survivors and Nonsurvivors*

This section studies how survivorship affects estimates of average performance and risk in my sample. I measure risk and performance on equal-weighted portfolios of mutual funds, as in Carhart (1997). The portfolios include nonsurviving funds in the equal-weighted average until they disappear, then readjust the portfolio weights appropriately.[5] This procedure mitigates look-ahead bias.

Table 5 shows that the equal-weighted portfolio of all mutual funds underperforms by 5 basis points per month relative to the CAPM and 15 basis points relative to the 4-factor model. The 4-factor model estimate amounts to a sizeable underperformance of 1.8 percent per year. As in Carhart (1997), the significant difference in performance estimates between the CAPM and 4-factor model is due to mutual funds holding smaller, lower book-to-market and higher momentum stocks which increases their expected return by a net of 10 basis points per month.

The performance on the portfolios of survivors and nonsurvivors is considerably different. Survivors achieve abnormal performance of +3 basis point per month relative to the CAPM, and -7 basis points relative to the 4-factor model. Nonsurvivors, however, earn CAPM and 4-factor model performance -measures of -24 and -33 basis points per month, respectively. The 4-factor model explains more than 17 percent of the variation in monthly return spread between survivors and nonsurvivors, while the CAPM explains nothing. From the 4-factor loadings, I infer that surviving funds hold smaller proportions in cash and small stocks and a larger proportion in high book-to-market stocks than do nonsurvivors.

In my sample, survivor bias does not depend on the performance measurement model. The simple return, CAPM and 4-factor model estimates of survivor bias over the complete time period--the difference between estimates of performance using survivors only versus the complete sample--are all 8 basis points per month. I also find that survivor bias differs significantly across fund objective groupings. In annual returns, aggressive growth survivors outperform all aggressive growth funds by 1.9 percent per year. For growth and income and long-term growth funds, the bias is 0.4 and 1 .1 percent, respectively. Thus, omitting nonsurvivors from estimates of average performance downwardly biases risk-adjusted mutual fund performance by approximately one percent per year. This applies only for the complete sample period; in the next section, I measure survivor bias in annual return estimates for varying sample lengths.

To better understand the differences in risk characteristics between survivors and nonsurvivors, I estimate 4-factor model time-series regressions on individual funds (instead of the equal-weighted portfolio reported above) and average the loadings and adjusted r-square estimates across funds. This procedure is complicated somewhat due to varying frequency returns on some funds. For example, when a fund is missing a particular month's return, my sample includes the following month's two-month return. In addition, early historical performance on some funds includes only quarterly or annual returns.[6] To use the complete fund history in 4-factor model time-series regressions, I splice the various-frequency excess returns into a single vector of dependent variable observations. Then, wherever a return observation represents an n-month return, the independent variable constant is multiplied by $n$ and I substitute the factor-mimicking portfolio returns over the same n-month period. Finally, I assume the residual variance of this observation is n-times the monthly variance and make an adjustment for heteroscedasticity. I require a minimum of 36 return observations to estimate these regressions, so the sample in these tests conditions on survival for at least three years.

I report the cross-sectional average 4-factor model loadings and r-squares in Table 6. The loading estimates between the two groups differ significantly for all factors. Survivors appear to hold about five percent less in cash than nonsurvivors, fewer small and one-year momentum stocks, and more high book-to-market stocks. In addition, nonsurvivors appear to hold less well diversified portfolios, as evidenced by their significantly lower 4-factor model adjusted r-squares. When subdivided by category (not reported), I find that aggressive growth survivors, in particular, hold more high book-to-market and fewer one-year momentum stocks, and that growth and income survivors hold less cash and fewer small stocks.

This evidence contrasts with Wermers (1997), who documents higher survival rates for funds following momentum strategies. Our conflicting results probably derive from differences in our data sets and measures of fund momentum. Where I measure the momentum of individual funds from the coefficient on the PR1 YR one-year momentum factor-mimicking portfolio, Wermers estimates momentum as the correlation between changes in fund weights and prior period returns.

Finally, I estimate the *total* risk between the surviving and nonsurviving samples, and find that nonsurvivors are significantly riskier. To remove the sensitivity of variance to time period, I estimate the 4-factor model on each fund's complete time history, then compute a fitted total variance as residual variance plus the sum of squared 4-factor loadings times the factor-mimicking portfolio's unconditional variances. The average total standard deviation for survivors, reported in Table 6, is 4.53 percent per month versus 4.88 percent for nonsurvivors. The t-stat for this difference in average total risk is 2.70. Thus, the empirical evidence supports a small degree of heteroscedasticity in mutual fund returns.

## C. *Survivor Bias in Average Performance Estimates as a Function of Sample Time Length*

I now measure the bias in average performance estimates due to survivor bias as a function of the sample time length. My objective is to obtain a rough rule of thumb on appropriate corrections for researchers using survivor-biased equity mutual fund samples. As shown in Section 1, the bias in average performance estimates will depend on the time length of the sample if a multiple-period performance selection rule dominates. Further, the results in Section 3 demonstrate that funds disappear primarily due to multiple-year performance. In my data set, I find a strong relation between survivor bias and sample time length, as predicted by the theory and empirical evidence above.

I consider all the possible survivor-biased samples that might be assembled from my database over the 1962 to 1995 period. For example, a researcher might assemble a five-year sample in 1972 or a ten-year sample in 1985. For each sample time length $k$, I consider all the possible (usually overlapping) annual return samples, and estimate the bias in average annual return induced by including only survivors. I report the average survivor bias across all possible $k$-year samples for various sample lengths. I also calculate correlation-adjusted standard errors assuming independent and identically distributed annual returns.'

Table 7 shows that survivor bias strongly increases in the sample time length. For a survivor-biased sample of only one year, the bias in average return is only 17 basis points, whereas the bias is 43 basis points per year for survivor-biased samples of five years. For samples greater than fifteen years, the hypothesis that survivor bias is one percent per year is not rejected. Interestingly, over the complete 34-year period, the survivor-biased sample outperforms the value-weighted CRSP index by 0.5 percent per year while the unbiased sample underoerforms the index by 0.6 percent annually.

Figure 2 plots the survivor bias in average performance estimates over all sample time lengths. The figure suggests that survivor bias levels off at about one percent per year for intervals of around fifteen years or longer. Thus, while there is no single rule of thumb on the magnitude of survivor bias, for time periods of fifteen years or longer, one percent is probably a good approximation of the bias in mean annual return estimates.

## D. *Effects of Survivor and Look-Ahead Bias on Persistence Tests*

I now examine the effect of survivor and look-ahead bias on the persistence tests of Hendricks, Pate1 and Zeckhauser (1993) and Carhart (1997). Annually, I form ten equal-weighted portfolios of mutual funds on a lagged performance measure. I hold the portfolios for one year, then re-form them. This yields a time-series of monthly returns on each decile portfolio over the complete time period, 1962 to 1995, less the initial performance estimation period. The performance measures are one-year return, five-year return, and three-year estimates of alpha from the 4-factor model.

The results in Table 8 suggest that survivor bias attenuates the evidence of persistence in mutual fund performance. Panel A reports persistence test statistics using the complete sample. Consistent with Carhart (1997), the portfolios demonstrate strong persistence in mean return, most of which is explained by the 4-factor model and expense ratios. For the portfolios sorted on one-year return, the post-formation spread in monthly returns between deciles 1 and 10 is a sizeable and statistically significant 63 basis points per month. The 4-factor model explains all but 24 basis points per month of this spread, and this remainder is insignificantly different from zero. The difference in average annual exnense ratios of 52 basis points between deciles 1 and 10 explains a further 4 basis points of this spread in performance on these portfolios. With a p-value of 14.8 percent, the Spear-man rank ordering test also fails to reject the hypothesis that the 4-factor alphas are randomly ordered. The results for the lagged five-year return and 4-factor alpha portfolios offer similar conclusions on performance persistence, except that the 4-factor model explains smaller amounts of the measured persistence in expected return.

The evidence favoring persistence in mutual fund returns weakens when considering only survivors. Panel B repeats the tests of Panel A using the survivor-biased sample of funds. Spreads in mean return and 4-factor model performance shrink considerably relative to the complete sample, and the statistical evidence weakens, also. In some cases, an econometrician using the survivor-biased sample may incorrectly reject persistence. Evidently, excluding nonsurvivors attenuates persistence because nonsurvivors consistently underperform. While the 4-factor alphas are somewhat larger for all portfolios in the survivor-biased sample, decile 10's performance is especially increased, amounting to approximately 20 basis points per month.

I also examine the effect of look-ahead bias separately from survivor bias. These tests utilize the full sample of survivors and nonsurvivors, but condition on a minimal survival period. I impose look-ahead bias in these tests by requiring that funds survive for the complete performance measurement period after portfolio formation. That is, the lagged one-year results include only funds surviving a full year after sorting on the previous-year's return, and the lagged five-year sample requires survival for an additional five years after sorting. This is the bias simulated by Brown, Goetzmann, Ibbotson and Ross (1992) and Carpenter and Lynch (1997).

Although the results are still downwardly biased, the look-ahead-biased tests in Panel C do not impact the results from persistence tests as much as survivor bias. Look-ahead bias changes the inference only for the five-year returns-sorted portfolios, the longest look-ahead period. Since I previously demonstrated that funds vanish principally for a sequence of poor annual returns rather than a single bad year, the relative insensitivity of the results to look-ahead bias are not surprising. As Brown, Goetzmann, Ibbotson and Ross (1992) and Carpenter and Lynch (1997) surmise, the effects of a multiple-period selection rule and heterogeneity in mutual fund total risk approximately offset one another, resulting in relatively unbiased persistence tests.

Finally, I undertake Hendricks, Pate1 and Zeckhauser's (1996) test for spurious persistence due to survivorship. Hendricks et al. show that when performance is categorized finely, the relation between pre- and post-period rankings will be J-shaped in a look-ahead-biased sample. They devise a regression test for this convexity, which I employ in my survivor- and look-ahead-biased samples. Under the hypothesis that performance persists spuriously due to survivorship, the HPZ J-shape t-statistic should be reliably negative. They are positive in all of my samples, suggesting that observed performance persistence is not spurious.

### E. *Effects of Survivor and Look-Ahead Bias on Cross-Section Tests*

Carhart (1997) reports some interesting new relations between performance and expenses, turnover and load fees previously unobserved in selection-biased data sets and methodologies. In this section, I demonstrate that survivor bias and look-ahead bias substantially affect these cross-section relations, and that the direction and magnitude of the biases depend on the type of selection bias.

The cross-section methodology follows Carhart (1997). In each month, I estimate the cross-section regression:

$$\alpha_{it} = a_t + b_t x_{it} + \varepsilon_{it} \qquad i = 1,\ldots, N,\ t = 1,\ldots, T$$

where a, is an individual fund performance estimate and $x_{it}$ is a fund characteristic. As in Fama and MacBeth (1973), I estimate the cross-sectional relation each month, then average the coefficient estimates across the complete sample period. To mitigate look-ahead bias, I estimate a, as a one-month abnormal return from the 4-factor model, where the 4-factor model loadings are estimated over the prior three years. I consider expense ratio, turnover, modified turnover (Mturn), *ln(*TNA), and maximum load fees as explanatory variables. As in Carhart, *ln(*TNA) and load fees are lagged one year. Further, the estimates on the turnover measures, size and load fees, use reported returns after adding back expense ratios to remove possible collinearity between these variables and expenses. I report the results from these tests in Table 9.

Although the point estimates differ slightly from Carhart (1997), the cross-section relations using the full sample strongly support Carhart's conclusions that performance is strongly negatively associated with expense ratios, turnover and load fees, and is unrelated to fund size. The -1.36 coefficient on expense ratio indicates that for every loo-basis-point increase in expense ratios, abnormal return drops by 136 basis points, somewhat more than one-for-one. The modified turnover coefficient implies round-trip transaction costs of 86 basis points, quite similar to the 95-basis-point estimate reported by Carhart. Although fund size exhibits no explanatory power, load fees are significantly negatively related to future performance. The coefficient point estimate of -0.08 implies that annual abnormal returns are reduced by 8 basis points for every 1 00-basis-point increase in load fees. This amounts to a reduction in annual return of 56 basis points for a load fund with the average load fee of 7 percent, a slightly lower estimate than in Carhart (1997).

These cross-section relations differ substantially in the selection-biased samples. When including only survivors, the negative relations between performance and expenses, turnover and load fees decline substantially and are no longer statistically significant for modified turnover or load fees. In addition, size is now strongly negatively related to abnormal performance, suggesting that small funds outperform large funds. If the complete sample is utilized but the tests condition on survival for at least five years after the initial performance estimate, the negative relations between performance and turnover and load fees are again reduced relative to the complete sample even though the expense-ratio and fund-size performance relations are not substantially affected. In the final sample, which imposes a 25-year look-ahead bias, performance varies only in load fees (negatively) and the estimate is almost twice that in the unbiased sample. This sample corresponds closely to the sample often employed by mutual fund researchers that use a survivor-biased data set like Morningstar but include only those funds which survive the complete period.

The cross-section tests on data sets with various selection biases imposed cast doubt on the conclusions from many published studies on the cross-section relations between performance and mutual fund attributes. Not only do the results differ substantially between complete and selection-biased data sets, but there is also little commonality in the direction and magnitude of the biases across variables. Among previous studies, only Carhart's (1997) estimates appear reliable.

## 4. Summary

In this paper, I introduce a new database of mutual fund performance and characteristics that substantially mitigates selection bias. I find that surviving mutual funds exhibit higher total risk than survivors and disappear primarily because of multiple-year performance rather than a single poor annual return. I demonstrate both analytically and empirically that this selection process in mutual funds causes the bias in estimates of average annual return to increase in the time-length of the data set. In my sample, the bias is economically small at 17 basis points for one-year samples, but a significantly larger one percent for samples longer than fifteen years.

In tests of mutual fund performance persistence, survivorship weakens the evidence of persistence. This sometimes results in rejections of persistence when the evidence is statistically significant in the full sample. However, the evidence favoring persistence does not support the existence of skilled or informed portfolio managers; Carhart (1997) shows that persistence is mostly explained by investment expenses and common factors in stock returns, primarily the one-year momentum effect of Jegadeesh and Titman (1993).

Finally, I replicate the negative relations between performance and expenses, turnover and load fees reported in Carhart (1997), and demonstrate the extreme sensitivity of these results to survivorship. Conditioning on survival at the end of the sample (survivor bias) or for a particular time length (look-ahead bias) can result in completely opposite biases in these estimates, casting doubt on most previous studies of these relations.

In this paper, I have attempted to provide a comprehensive analysis of survivorship issues in mutual fund studies. I document a few simple rules of thumb for the effects of survivorship in these studies, but offer no single scheme for dealing with biases induced by survivorship. In general, researchers can say little about the direction or the magnitude of bias in survivorship-biased databases without very precise knowledge on the properties of the missing sample, some properties of which I provide for my sample. The results stress the importance of choosing data sets and methodologies free of selection bias.

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# ENDNOTES

1. I thank Will Goetzmann for this clarification.

2. Since the selection rule removes funds according to relative performance, the simulations require only the use of residual returns. While fund-wise heteroscedasticity might better describe the real world, this simplified simulation suffices to demonstrate the effect of sample time-length on survivor bias.

3. Exchange was the only method of acquiring shares in these funds, although shares were redeemable for cash.

4. When a fund's termination date is unknown, I assume the fund terminates in the month after its last return observation in my sample.

5. I obtain only annual returns on many nonsurvivors. Excluding these funds from my monthly portfolio returns upwardly biases performance estimates. To mitigate this potential bias, I compare the average annual return on all funds to those with only monthly returns. If they differ for any year, I add one-twelfth of this difference equally to all months of that year (using continuously compounded returns.) The difference in mean annual return is typically less than 20 basis points.

6. In my total sample of approximately 202,000 returns, I have 1,34 1 annual returns, 258 quarterly returns and 5 13 two-month returns. Of these, nonsurvivors account for 1,039 annual returns, 90 quarterly returns and 509 two-month returns. Thus, excluding these returns might significantly bias my estimates and inferences.

7. I assume the database is compiled one year after the last year of the database which simplifies the categorization of survivors and nonsurvivors. The standard error is calculated as

$\left(\frac{1}{T-n+1}\right)\left(2\sum_{i=1}^{n-1}\left(\frac{i}{n}\right)^{2}+T\,2(n-1)\right)^{\frac{1}{2}} std(R)$, where n is the sample time length in years, T is the number of samples in the database, and std(R) is the standard deviation in annual returns.

**Table 1**

Monte Carlo Simulations of Survivor Bias in Average Return Estimates

| Selection rule | Time length of sample (years) 1 | 2 | 3 | 4 | 5 | 10 | 20 |
|---|---|---|---|---|---|---|---|
| One year | 0.386% | 0.386% | 0.386% | 0.386% | 0.386% | 0.386% | 0.386% |
| Two years | 0.283% | 0.412% | 0.441% | 0.457% | 0.465% | 0.484% | 0.493% |
| Three years | 0.249% | 0.344% | 0.444% | 0.475% | 0.494% | 0.535% | 0.554% |
| Four periods | 0.224% | 0.305% | 0.387% | 0.468% | 0.502% | 0.563% | 0.595% |
| Five periods | 0.210% | 0.281% | 0.352% | 0.422% | 0.495% | 0.582% | 0.627% |
| Ten periods | 0.170% | 0.218% | 0.260% | 0.301% | 0.337% | 0.552% | 0.666% |

The table reports estimates of survivor bias in average annual mutual fund returns from Monte Carlo simulations. Annual residual returns for a hypothetical sample of 1,000 funds are drawn from the normal distribution with mean zero and 5 percent standard deviation. In each year over a k-year sample, the survivor-biased sample drops the lowest 3.5 percent based on their last m-period residual returns and the same number of new funds are created, thus maintaining a constant sample size. For a given simulation, survivor bias is the time-series average of the k annual cross-sectional average returns across surviving mutual funds. Each k-year sample and m-year selection rule is simulated 10,000 times. The survivor-bias estimates reported in the table represent averages of survivor bias across the simulations.

**Table 2**
Mutual Fund Database Annual Annual Summary Statistics, 1962 to 1995

| Year | Beg Total Funds | New Funds | Disappearing Funds Merg | Liq | 0th | Nonsurvi Rate (%/yr) | Avg TNA ($ mil) | Avg Exp Ratio (%/yr) | Avg Mtum (%/yr) | Avg Flow (%/yr) | Percent with Load | Avg Total Load | Percent Common Stock | EW Fund Return | CRSP Return |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1962 | 213 | 16 | | 1 | | 0.5% | 72.0 | 0.82% | NA | 9.8% | 82.9% | 7.70 | 86.9% | -15.8% | -10.3% |
| 1963 | 228 | 13 | 2 | 5 | 1 | 3.5% | 82.0 | 0.94% | NA | 0.4% | 80.7% | 7.73 | 88.4% | 18.4% | 20.9% |
| 1964 | 233 | 12 | 2 | 2 | | 1.7% | 95.1 | 0.82% | NA | 5.5% | 80.5% | 7.76 | 89.0% | 12.4% | 16.3% |
| 1965 | 241 | 21 | 1 | | | 0.4% | 110.0 | 0.84% | NA | 6.7% | 78.0% | 7.80 | 88.1% | 23.0% | 14.4% |
| 1966 | 261 | 26 | 3 | 2 | | I.9% | 101.0 | 0.84% | 72.6% | 11.4% | 78.4% | 7.91 | 84.4% | -5.8% | -8.7% |
| 1967 | 282 | 40 | 2 | 3 | | 1.8% | 121.7 | 0.89% | 75.1% | 14.0% | 77.9% | 7.86 | 84.2% | 36.7% | 28.6% |
| 1968 | 316 | 66 | 1 | | | 0.3% | 135.1 | 0.95% | 8I.4% | 27.8% | 78.4% | 7.96 | 81.5% | 16.3% | 14.1% |
| 1969 | 382 | 94 | | 3 | | 0.8% | 100.2 | 1.04% | 86.6% | 20.8% | 76.7% | 8.10 | 80.6% | -14.1% | -10.8% |
| 1970 | 473 | 68 | 8 | 12 | 3 | 4.9% | 83.3 | 1.19% | 89.5% | 10.4% | 74.5% | 8.06 | 81.5% | -9.4% | 0.1% |
| 1971 | 517 | 39 | 6 | 14 | 2 | 4.3% | 93.2 | 1.38% | 87.6% | 5.5% | 71.6% | 8.04 | 85.5% | 19.8% | 16.2% |
| 1972 | 535 | 19 | 12 | 19 | | 5.8% | 102.7 | 1.27% | 79.5% | 1.4% | 66.9% | 8.10 | 87.1% | 10.8% | 17.3% |
| 1973 | 523 | 11 | 26 | 8 | 3 | 7.1% | 81.8 | 1.26% | 67.1% | -1.0% | 66.2% | 8.10 | 82.2% | -24.5% | -18.8% |
| 1974 | 497 | 3 | 23 | 10 | 2 | 7.0% | 62.3 | 1.36% | 57.7% | 1.0% | 65.7% | 8.11 | 78.1% | -24.9% | -27.8% |
| 1975 | 465 | 4 | 31 | 9 | | 8.6% | 86.4 | 1.41% | 59.7% | -1.5% | 64.5% | 8.11 | 83.4% | 33.6% | 37.4% |
| 1976 | 429 | 7 | 19 | 5 | 10 | 7.9% | 98.8 | 1.27% | 65.1% | -10.5% | 63.7% | 7.90 | 87.5% | 24.9% | 26.8% |
| 1977 | 402 | 15 | 18 | 5 | 1 | 6.0% | 89.3 | 1.31% | 53.7% | -6.8% | 59.7% | 7.77 | 82.1% | 2.3% | -3.0% |
| 1978 | 393 | 11 | 21 | 4 | 3 | 7.1% | 88.5 | 1.32% | 71.3% | -8.5% | 57.4% | 7.69 | 82.7% | 11.2% | 8.5% |
| 1979 | 376 | 6 | 6 | 4 | 3 | 3.5% | 101.1 | 1.28% | 71.4% | -10.5% | 55.3% | 7.67 | 83.6% | 30.0% | 24.4% |
| 1980 | 369 | 18 | 5 | 2 | 1 | 2.2% | 124.5 | 1.21% | 84.2% | -3.9% | 53.6% | 7.69 | 84.4% | 32.2% | 33.2% |
| 1981 | 379 | 17 | 9 | 3 | | 3.2% | 116.4 | 1.17% | 77.5% | -0.5% | 51.1% | 7.68 | 79.1% | -1.2% | -4.0% |
| 1982 | 384 | 34 | 9 | 2 | | 2.9% | 141.3 | 1.29% | 90.6% | 7.6% | 48.8% | 7.64 | 81.6% | 25.9% | 20.4% |
| 1983 | 407 | 52 | | 2 | 2 | 1.0% | 177.3 | 1.15% | 97.9% | 17.2% | 46.2% | 7.50 | 82.9% | 19.6% | 22.7% |
| 1984 | 455 | 52 | 2 | 2 | 3 | 1.5% | 171.5 | 1.12% | 85.6% | 7.8% | 44.0% | 7.36 | 80.7% | -1.4% | 3.3% |
| 1985 | 500 | 91 | 1 | 2 | 4 | 1.4% | 203.5 | 1.17% | 97.5% | 12.0% | 42.7% | 7.12 | 81.6% | 26.8% | 3 1.4% |

**Table 2 - continued**

| Year | Beg Total Funds | New Funds | Disappearing Funds Merg | Liq | 0th | Nonsurvi Rate (%/yr) | Avg TNA ($ mil) | Avg Exp Ratio (%/yr) | Avg Mtum (%/yr) | Avg Flow (%/yr) | Percent with Load | Avg Total Load | Percent Common Stock | EW Fund Return | CRSP Return |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1986 | 584 | 100 | 2 | 3 | 1 | 1.0% | 218.5 | 1.20% | 96.9% | 18.8% | 44.0% | 6.53 | 80.9% | 13.3% | 15.6% |
| 1987 | 678 | 116 | 3 | 3 | | 0.9% | 206.8 | 1.28% | 104.7% | 12.2% | 45.1% | 6.06 | 80.8% | 0.8% | 1.8% |
| 1988 | 788 | 86 | 10 | 8 | 3 | 2.7% | 203.6 | 1.41% | 87.4% | -1.4% | 45.5% | 5.64 | 78.9% | 14.3% | 17.6% |
| 1989 | 853 | 57 | 20 | 12 | 5 | 4.3% | 254.2 | 1.39% | 83.5% | 4.1% | 46.2% | 5.42 | 80.1% | 23.5% | 28.5% |
| 1990 | 873 | 83 | 18 | 18 | 3 | 4.5% | 232.1 | 1.42% | 101.2% | 2.4% | 48.1% | 5.07 | 79.2% | -6.0% | -6.0% |
| 1991 | 917 | 88 | 28 | 17 | 2 | 5.1% | 332.3 | 1.32% | NA | 15.1% | 46.9% | 4.96 | 82.0% | 34.5% | 33.6% |
| 1992 | 958 | 159 | 34 | 11 | 11 | 5.8% | 389.8 | 1.36% | 91.3% | 21.5% | 52.0% | 4.74 | 82.0% | 8.3% | 9.0% |
| 1993 | 1061 | 178 | 25 | 12 | 6 | 4.1% | 466.4 | 1.31% | 93.1% | 22.2% | 49.2% | 4.78 | 83.3% | 13.0% | 11.3% |
| 1994 | 1196 | 217 | 23 | 19 | 1 | 3.6% | 478.0 | 1.31% | 83.5% | 12.3% | 45.7% | 4.61 | 87.1% | -1.7% | -0.6% |
| 1995 | 1370 | 55 | 47 | 36 | 12 | 6.9% | 681.0 | 1.30% | 99.3% | 14.7% | 45.3% | 4.62 | 88.6% | 30.4% | 35.7% |
| Mean | 545 | 55 | 13 | 8 | 4 | 3.6% | 179.5 | 1.19% | 82.5% | 7.0% | 59.8% | 7.05 | 83.2% | 11.1% | 11.7% |
| Std | 287 | 52. | 12 | 7 | 3 | 2.4% | 137.8 | 0.19% | 13.3% | 9.7% | 13.6% | 1.23 | 3.0% | 16.9% | 16.3% |

The table reports annual cross-sectional averages from 1962 to 1995. Total funds are the total number of funds at the beginning of the year and new funds are funds started during that year. Merg is the number of funds that merged, Liq is the number of funds that liquidated and 0th is the number of funds that disappeared for a reason other than merger or liquidation. Nonsurvival rate is the lumber of disappearing funds divided by the total number of funds at the beginning of the year. TNA is total net assets, Exp ratio is :he total annual management and administrative expenses divided by average TNA. Mtum is modified turnover and represents reported turnover plus 0.5 times the absolute value of Flow. Flow is the percentage change in TNA adjusted for investment return and mergers. Maximum load is the total of maximum front-end, rear-end and deferred sales charges as a percentage of the nvestment. Percent stock is the proportion of total net assets invested in equity securities. EW fund return is the equally-weighted average annual mutual fund return, and CRSP return is the value-weighted average annual return on all NYSE, Amex and NASDAQ stocks in the CRSP database.

**Table 3**

Properties of Surviving and Nonsurviving Mutual Funds

| Group | Number of Funds | Cross-Sectional Average Abnormal Monthly Performance: Group Adjusted | Cross-Sectional Average Abnormal Monthly Performance: 4-Factor Alpha | Relative TNA | Relative Exp | Relative Mtum | Relative Flow |
|---|---|---|---|---|---|---|---|
| By current status | | | | | | | |
| Survivors | 1,346 | 0.10% | -0.03% | 1.45 | 0.89 | 0.96 | 1.2% |
| Nonsurvivors | 725 | -0.26% | -0.34% | 0.28 | 1.23 | 1.15 | -4.7% |
| Nonsurvivors by reason for disappearance | | | | | | | |
| Merged with another fund | 417 | -0.19% | -0.29% | 0.32 | 1.13 | 1.14 | -4.6% |
| Liquidated | 258 | -0.45% | -0.54% | 0.05 | 1.85 | 1.53 | -4.0% |
| Other, self-selected | | | | | | | |
| *Removed at fund request* | 11 | -0.10% | -0.25% | | | | |
| *Changed to closed-end fund* | 2 | -0.86% | -0.64% | | | | |
| *Split into multiple funds* | [illegible] | 0.26% | 0.09% | | | | |
| | 14 | -0.18% | -0.28% | 0.34 | 1.19 | 0.77 | -0.1% |
| Other, not self-selected | | | | | | | |
| *Tax-Free Exchange Fund* | 16 | 0.23% | NA | | | | |
| *Variable Annuity* | 5 | -0.07% | 0.04% | | | | |
| *Unknown* | 15 | -0.14% | -0.33% | | | | |
| | 36 | 0.03% | NA | 0.52 | 1.16 | 0.75 | -1.9% |

The table reports averages across individual mutual funds of performance and attributes by survival category. Survivors are those funds still operating December 31, 1995 and nonsurvivors are funds disappearing before this date. Group-adjusted performance is the time-series average of the difference between a fund's return and the average return on all other funds with the same declared fund objective over the fund's complete history. 4-factor alpha is the intercept from a time-series regression of a fund's excess returns on the 4-factor model factor-mimicking portfolios over the fund's complete history. Relative TNA is the time-series average of the ratio between a fund's TNA and the average TNA of all funds in the same year, and relative expense ratio and Mtum are calculated in the same manner. Relative flow is the time-series average of the difference between a fund's annual flow and the average flow on all funds in the same year, where flow measures the change in TNA adjusted for investment returns and fund mergers.

## Table 4
### Relative Performance on Nonsurviving Mutual Funds in their Final Years

| Performance Group | Group adjusted performance | | | | |
|---|---|---|---|---|---|
| | Last year | Last 2 years | Last 3 years | Last 4 years | Last 5 years |
| Bottom 50% | 62.0% | 69.1% | 75.1% | 79.0% | 76.8% |
| Bottom 10% | 24.8% | 29.8% | 32.8% | 32.4% | 33.0% |
| Bottom 5% | 15.7% | 20.4% | 21.7% | 20.3% | 20.8% |
| Bottom 1% | 6.3% | 4.8% | 5.6% | 5.4% | 5.9% |
| Number of funds | 637 | 598 | 531 | 463 | 409 |

The table presents the performance of nonsurviving funds relative to all funds over their final 60 months of operation. Fund performance is the fractile of mean group-adjusted return in the distribution of mean group-adjusted returns on all funds over the same period. Returns are measured over the final 12-, 24-, 36-, 48-, and 60-month periods prior to fund termination, not the final calendar years. The table reports average fractiles across all nonsurviving funds and the number of funds included in each estimate.

Table 5

**Performance** of Equal-weight Mutual Fund Portfolios by Survival Category and Fund Objective

| | | | **CAPM** | | | **4-Factor** Model | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean Monthly | Standard | | | Adjusted | | Loadings on | | | | Adjusted |
| **Portfolio** | Return | Dev | Alpha | Beta | R-square | Alpha | **RMRF** | SMB | HML | **PR1YR** | R-square |
| All funds | 0.93% | 4.36% | -0.05% | 0.97 | 0.938 | -0.15% | 0.89 | 0.33 | -0.06 | 0.09 | 0.978 |
| | | | (-0.96) | (76.91) | | (-4.17) | (105.65) | (25.66) | (-4.36) | (9.09) | |
| ***By current status*** | | | | | | | | | | | |
| All survivors | 1.00% | 4.31% | 0.03% | 0.97 | 0.952 | -0.07% | 0.90 | 0.29 | -0.05 | 0.09 | 0.984 |
| | | | (0.56) | (84.23) | | (-2.34) | (117.57) | (26.21) | (-4.62) | (10.87) | |
| All nonsurvivors | 0.74% | 4.38% | -0.24% | 0.97 | 0.917 | -0.33% | 0.88 | 0.37 | -0.07 | 0.09 | 0.966 |
| | | | (-3.65) | (63.12) | | (-7.42) | (74.96) | (21.05) | (-3.11) | (6.30) | |
| Survivors - all funds | 0.08% | 0.25% | 0.08% | -0.01 | 0.009 | 0.08% | 0.00 | -0.04 | 0.01 | 0.00 | 0.208 |
| | | | (6.39) | (-2.12) | | (6.52) | (0.59) | (-9.14) | (2.85) | (0.05) | |
| ***By fund objective*** | | | | | | | | | | | |
| Aggressive growth | 1.04% | 5.16% | -0.01% | 1.11 | 0.863 | -0.16% | 0.94 | 0.61 | -0.16 | 0.18 | 0.965 |
| | | | (-0.06) | (47.66) | | (-3.06) | (69.62) | (31.33) | (-6.80) | (11.20) | |
| Growth and income | 0.89% | 3.68% | -0.02% | 0.84 | 0.967 | -0.12% | 0.82 | 0.13 | 0.08 | 0.04 | 0.978 |
| | | | (-0.54) | (109.46) | | (-3.92) | (113.44) | (11.27) | (5.39) | (4.87) | |
| Long-term growth | 0.92% | 4.41% | -0.07% | 0.99 | 0.951 | -0.13% | 0.91 | 0.28 | -0.10 | 0.09 | 0.981 |
| | | | (-1.43) | (83.31) | | (-3.99) | (105.18) | (21.35) | (-6.47) | (8.25) | |

The table reports performance on equal-weighted portfolios of mutual funds by survival and objective category from 1962 to 1995. RMRF, SMB HML are Fama and French's (1993) market proxy and factor-mimicking portfolios for size and book-to-market equity. **PR1YR** is a factor-mimick portfolio for one-year return momentum. The t-statistics are in parentheses.

## Table 6
Cross-Sectional Average Risk of Survivors and Nonsurvivors

| | Cross-Section Averages | | | | | |
|---|---|---|---|---|---|---|
| | 4-Factor Model Loadings | | | | | Monthly Fitted |
| Current Status | RMRF | SMB | HML | PR1YR | Adjusted R-square | Standard Deviation |
| Survivors | 0.905 | 0.275 | -0.052 | 0.066 | 0.830 | 4.53% |
| Nonsurvivors | 0.854 | 0.340 | -0.077 | 0.090 | 0.773 | 4.88% |
| Survivors - Nonsurvivors | 0.051 | -0.065 | 0.025 | -0.024 | 0.058 | -0.35% |
| [t-statistic) | (9.81) | (-6.47) | (3.68) | (-5.56) | (14.47) | (2.70) |

After estimating the 4-factor model loadings and adjusted r-squares on all funds with a minimum of 36 return observations, I calculate cross-sectional averages of the loadings, r-squares and Fitted standard deviations by survival category. Fitted standard deviation is the square root of fitted variance, where fitted variance is residual variance from the 4-factor model plus the sum of squared loadings times the unconditional variances on the factor-mimicking portfolios. The table includes 1,302 funds with the minimum performance history.

Table 7

Survivor Bias Estimates as a Function of the Mutual Fund Sample Time Length

This table compares the mean annual return estimates on a survivor-biased sample to a complete sample. The table averages all possible biased and unbiased samples of a given time length that might be assembled from my database over the 1962 to 1995 period. Survivor bias is the difference between the mean annual return estimates in the two samples. The table also reports correlation-adjusted standard errors in the estimate of survivor bias, assuming independent and identically distributed annual returns.

| Sample Time Length (years) | Number of Samples | Mean Annual Return Estimate: Survivor-biased Sample | Mean Annual Return Estimate: Unbiased Sample | Survivor Bias | Standard Error |
|---|---|---|---|---|---|
| 1 | 34 | 11.17% | 11.00% | 0.17% | 0.02% |
| 5 | 3 0 | 11.51% | 11.08% | 0.43% | 0.02% |
| 10 | 25 | 11.51% | 10.80% | 0.71% | 0.02% |
| 15 | 20 | 12.17% | 11.25% | 0.91% | 0.02% |
| 20 | 15 | 12.30% | 11.25% | 1.04% | 0.03% |
| 25 | 10 | 11.91% | 10.80% | 1.10% | 0.06% |
| 30 | 5 | 12.27% | 11.08% | 1.19% | 0.12% |
| 34 | 1 | 12.15% | 10.91% | 1.24% | 0.33% |

**Table 8**

The Effects of Survivorship on Persistence Tests

| Portfolio Sorting Variable | Decile 1-10 Spread: Mean Monthly Return | Decile 1-10 Spread: t-stat | 4-Factor Model Alphas: Decile 1 | 4-Factor Model Alphas: Decile 10 | 4-Factor Model Alphas: Decile 1-10 | 4-Factor Model Alphas: t-stat | Spearman Test p-value | Decile 1-10 Spread: Expense Ratios | Decile 1-10 Spread: Turnover (Mturn) | HPZ J-shape t-stat |
|---|---|---|---|---|---|---|---|---|---|---|
| **A. Full Sample** | | | | | | | | | | |
| 1-Year Returns | 0.63% | 4.52 | -0.13% | -0.37% | 0.24% | 1.79 | 0.148 | -0.52% | 4.2% | |
| 5-Year Returns | 0.23% | 2.09 | -0.10% | -0.34% | 0.24% | 2.06 | 0.025 | -0.66% | -2.2% | |
| 3-Year 4-Factor Alpha | 0.36% | 5.04 | -0.01% | -0.36% | 0.36% | 4.60 | 0.000 | -0.47% | -11.5% | |
| **B. Survivor-Biased Sample** | | | | | | | | | | |
| 1-Year Returns | 0.52% | 3.93 | -0.05% | -0.15% | 0.10% | 0.84 | 0.204 | -0.14% | 20.0% | 1.74 |
| 5-Year Returns | 0.18% | 1.85 | -0.07% | -0.19% | 0.12% | 1.15 | 0.027 | -0.42% | 12.5% | 1.48 |
| 3-Year 4-Factor Alpha | 0.19% | 2.66 | 0.01% | -0.17% | 0.18% | 2.30 | 0.000 | -0.29% | -5.0% | 2.40 |
| **C. Look-Ahead Biased Sample** | | | | | | | | | | |
| 1-Year Returns | 0.62% | 4.44 | -0.14% | -0.36% | 0.21% | 1.60 | 0.174 | -0.48% | 5.1% | 1.76 |
| 5-Year Returns | 0.20% | 1.84 | -0.11% | -0.29% | 0.17% | 1.34 | 0.052 | -0.52% | 3.2% | 0.38 |
| 3-Year 4-Factor Alpha | 0.34% | 4.73 | -0.00% | -0.34% | 0.33% | 4.07 | 0.000 | -0.31% | -8.4% | 1.80 |

Mutual funds are sorted on January 1 each year into decile portfolios based on a lagged performance measure. The performance measures are 1-year return, 5-year return and 4-factor alpha measured over the prior 3 years. The portfolios are equal-weighted monthly so the weights are readjusted whenever a fund disappears. Funds with the highest lagged performance measure comprise decile 1 and funds with the lowest comprise decile 10. The Spear-man nonparametric test measures the correlation in rank ordering of post-formation portfolio performance measures. Here the null hypothesis is that the performance measures are randomly ordered. The t-statistic on the HPZ J-shape measures the convexity in the relation between pre- and post-formation portfolio ranks. A reliably negative t-statistic is consistent with spurious performance persistence due to survivorship.

**Table 9**

The Effects of Survivorship on Cross-Section Regressions

Estimated univariate cross-sectional regressions for each month from July 1966 to December 1995. The dependent variable is the monthly residual from the 4-factor model, where the factor loadings are estimated on the prior 3 years of monthly returns. The independent variables are expense ratio, turnover, modified turnover (Mturn), the natural log of TNA lagged one year, and maximum load fees lagged one year. Expense ratio is management, administrative, and 12b-1 expenses divided by average TNA. TNA is total net assets. Turnover is the minimum of purchases and sales divided by average TNA. Modified turnover represents reported turnover plus 0.5 times the absolute value of percentage change in portfolio TNA adjusted for investment returns and mergers. Maximum load is the sum of maximum front-end, back-end and deferred sales charges. All estimates are annualized by multiplying the monthly estimates by 12. The reported estimates are time-series averages of monthly cross-sectional regression slope estimates as in Fama and MacBeth (1973). The t-statistics are on the time-series means of the coefficients. The regressions on TNA, maximum load, and the turnover measures use the residuals from reported returns after adding back expense ratios.

| Independent Variables (coefficients x 100) | Full Sample Estimates (t-statistic) | Survivor-biased Sample Estimates (t-statistic) | Five-year Look-ahead Biased Sample Estimates (t-statistic) | 25-year Look-ahead Biased Sample Estimates (t-statistic) |
|---|---|---|---|---|
| Expense Ratio (t) | -1.36 | -0.80 | -1.15 | -0.59 |
| | (-4.88) | (-2.69) | (-3.97) | (-1.20) |
| Turnover (t) | -1.15 | -0.93 | -0.62 | 0.08 |
| | (-3.03) | (-2.21) | (-1.57) | (0.18) |
| Modified Turnover (t) (Mturn) | -0.86 | -0.64 | -0.52 | -0.12 |
| | (-2.33) | (-1.58) | (-1.31) | (-0.43) |
| In TNA (t-l) | -0.03 | -0.21 | 0.02 | 0.09 |
| | (-0.47) | (-3.24) | (0.39) | (1.15) |
| Maximum Load (t-l) | -0.08 | -0.04 | -0.02 | -0.14 |
| | (-3.28) | (-1.62) | (-0.65) | (-2.45) |

## FIGURE LEGENDS

**Figure 1.** Final 5-Year Performance on Nonsurvivors. The figure presents the average annual group-adjusted performance, gross of expense ratios, on nonsurviving funds in each of their five years prior to termination. Group-adjusted performance is the difference between the fund's return and the equal-weighted portfolio of all funds with the same objective in that period. The dotted lines represent two-standard-error boundaries.

**Figure** 2. Survivor Bias as a Function of the Sample Time Length. The figure reports the bias in average annual return estimates when using a survivor-biased sample instead of the complete, as a function of the time-length of the sample. The bias is the average over all possible samples of a given time length that might be assembled from my database over the 1962 to 1995 period. The dotted lines represent two-standard error boundaries in the average bias.

Figure 1. Final 5-Year Performance on Nonsurvivors

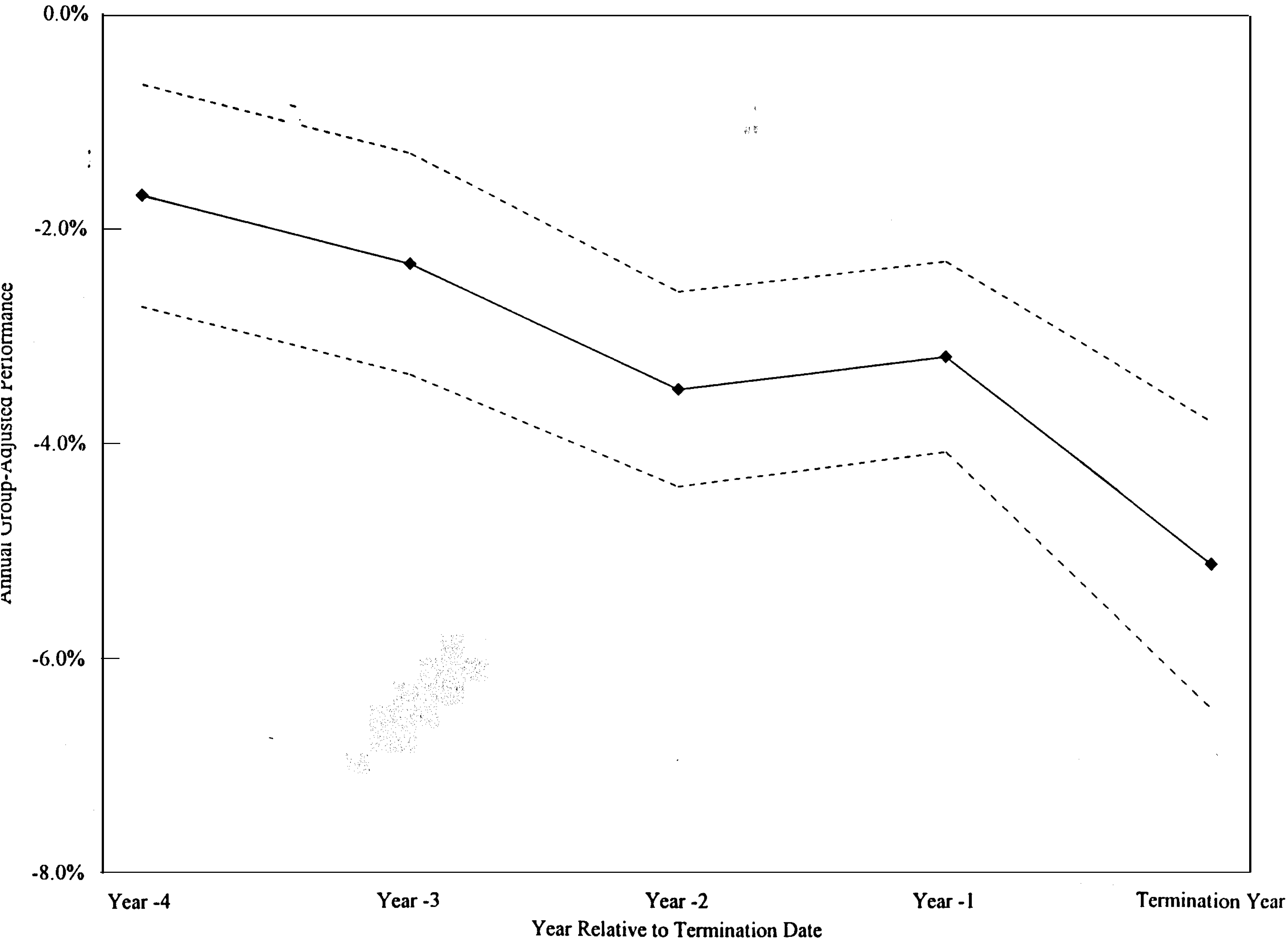
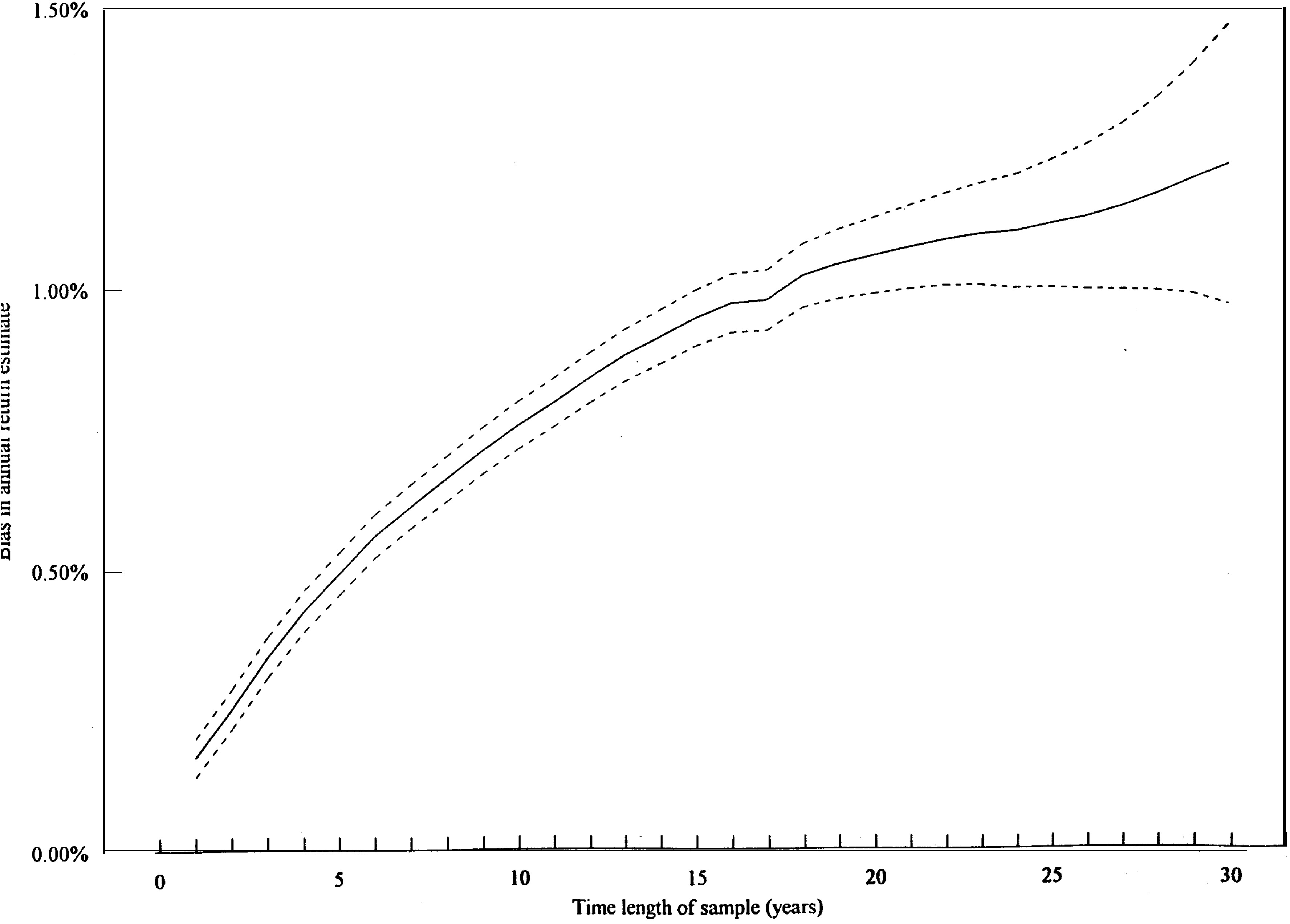


Figure 2. Survivor Bias as a Function of the Sample Time Length



# Mutual Fund Surviorship

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