Assessing Hedge Fund Performance with Institutional Constraints

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Abstract

Standard tests for anomalies in hedge fund returns are not consistent with investment practices because they ignore performance reporting delay, overlook fund selection standards of institutional investors, and often use portfolios with too many funds. This paper introduces a set of tests based on a large scale simulation framework and stochastic dominance methodology. These tests incorporate constraints that are standard practice in the institutional investment field. We investigate momentum in performance of hedge funds in the managed futures industry considering these constraints. From various tests based on the simulation, we find the evidence of the performance persistence in the institutional investment areas.

Keywords: Hedge Funds, Commodity Trading Advisors, Performance Persistence, Institutional Investment
JEL: : G11; G12; G23

1. INTRODUCTION

Momentum is regarded as a market anomaly that has been observed in various financial markets. Cross-sectional momentum in returns has been documented in US equities (Jagadeesh and Titman, 1993; Fama and French, 1996), international equity markets (Rouwenhorst, 1998), industries (Moskowitz and Grinblatt, 1999) and equity indices (Asness et al., 1997), Bhojraj and Swaminathan, 2006). Momentum in returns is present in foreign exchange (Shleifer and Summers, 1990) and commodities markets (Erb and Harvey,2006, Gorton et al., 2013). Asness et al. (2013) analyze cross-sectional momentum and value strategies across several asset classes including individual stocks, stock indices, currencies, commodities and bonds. They find significant momentum in every asset class considered in their studies.

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In mutual funds and hedge funds, momentum is regarded as the effect of anomalies. Hendricks et al. (1993) test for momentum in mutual fund returns. They find persistence in relative performance of mutual funds with the difference in the risk-adjusted performance of the top and bottom octile portfolios of six to eight percent per year. Carhart (1997) uses decile methodology to evaluate persistence in mutual fund performance. He finds strong persistence in performance of the worst performing managers and no evidence of skilled or informed mutual fund portfolio managers who consistently provide better risk-adjusted returns. Agarwal and Naik (2000a, 2000b) document significant quarterly persistence of hedge fund returns primarily driven by the worst performing funds. Capocci and Hübner (2004) use decile methodology to discover lack of persistence among the top and bottom decile funds and little persistence among middle decile funds.

Kosowski et al. (2007) apply a Bayesian estimate of Jenson alphas (Jenson, 1968), introduced by Pástor and Stambaugh (2002), to demonstrate performance persistence over a one-year horizon. Jagannathan et al. (2010) use weighted least squared and GMM approaches to find significant performance persistence among the top performing hedge funds and little evidence of persistence among the bottom performing funds. They rank funds using the t-statistic of alpha and report superior performance of portfolios of all funds in the top decile and the top tercile to demonstrate the practical importance of their approach for institutional investors.

The techniques used to test for momentum in various asset classes and mutual funds are often relevant to institutional investors, who can relatively easily build large long-short portfolios of winners-losers and rebalance them monthly, although these investors still need to deal with practical implementation issues of transaction costs and market impact. Similar techniques are used to evaluate persistence in performance of hedge funds. However, these techniques cannot be implemented by prudent institutional investors because the methodology ignores the delay in hedge fund reporting and, therefore, investment recommendations are based on information that is not available at the time of investment decisions. In addition, the studies consider funds that have assets under management that are too small for institutional investors, have very short track records (sometimes as little as 12 months) and involve portfolios with too many funds be practically investable. The failure to account for these common industry constraints may limit the applicability of the academic research to actual investment practice.

The objective of this paper is to examine the issue of performance persistence within a framework that incorporates common industry constraints by institutional investors when creating and rebalancing portfolios of hedge funds. These constraints serve to reduce transaction costs over multiple periods and include limitations on individual funds: the size of assets under management and the length of the fund track record. The approach also places a limit on the number of funds to be included in the portfolio and on the turnover of these funds. Specifically, our model assumes that the institutional investor selects a discrete number of otherwise acceptable funds and that, once selected, a fund will stay in the portfolio until it no longer satisfies the selection criteria. The imposition of these constraints results in a very large number of feasible portfolios in each period. The model employs a large scale simulation framework designed to test for anomalies in hedge fund returns in a way that is consistent with requirements of large institutional investors.

Little work has been done in investigating performance persistence among Commodity Trading Advisors (CTAs), a subset of hedge funds that is primarily known for utilizing trend-following or time-series momentum strategies in futures and options markets. Institutional interest in CTAs has increased in response to the performance of these funds during the Global Financial Crisis with assets growing from US \$131 billion in 2005 to US \$330 billion in the first quarter of 2015 according to BarclayHedge Group. The simulation model provides a test for persistence in CTA performance that could be used by institutional investors who are interested in allocating to this segment of the alternative investments space. The model incorporates two important constraints. First, it excludes funds who are in the bottom 30% in assets under management or whose track record is fewer than 60 months old. Second, the performance of the remaining funds is measured by calculating the t-statistic of alpha with respect to the CTA benchmark using data from the previous 60 months. In the first month, the model creates an initial portfolio by selecting 20 funds from the top quintile of the performance distribution and also randomly selects another 20 funds from the entire sample. In each subsequent month, funds that liquidate or fail to meet the selection criteria are randomly replaced from the pool of funds that meet the corresponding selection criteria. A single observation consists of a time-series returns over the entire out-of-sample period from 1999 to 2013. The simulation engine repeats this procedure 10,000 times and then compares the performance of the strategy applied to all funds to the strategy that only considers funds in the top quintile. The dataset contains 4,909 funds over the period 1994 through 2013. We show that the strategy of selecting top quintile funds significantly improves risk-adjusted performance of hypothetical portfolios of institutional investors in out-of-sample. We perform robustness analysis to evaluate robustness of our findings across different market environments.

The evaluation of out-of-sample results is challenging primarily because simulation results are not independent since the returns of the same funds are used across many simulation; therefore, standard statistical tests are inappropriate. The model employs a bootstrapping procedure to approximate the sampling properties of the test results. The comparison is based upon stochastic dominance (SD) methodology, developed by Hanoch and Levy (1969), Hadar and Russell (1969), and Rothschild and Stiglitz (1971), has been used in both decision theory with uncertainty and used as an alternative to mean-variance analysis to evaluate portfolios (Levy and Sarnat, 1970). As Fischmar and Peters (1991) describe, stochastic dominance is a comprehensive measure of portfolio return and risk in that, unlike mean-variance analysis which only considers mean and variance, it utilizes the entire distributions of returns to compare benefits of various portfolios to a broad set of investors without having to make assumptions about each investor's utility function. Second order stochastic dominance is particularly attractive because it highlights the situations when all risk averse investors would agree that one distribution is better than the other.

The rest of the paper is organized as follows. Section 2 describes the data and treatment for eliminating biases; Section 3 discusses the methodologies used in the performance persistence literature, introduces the large scale simulation framework as a an alternative methodology and a stochastic dominance framework used to evaluate out-of-sample performance; Section 4 presents empirical results and Section 5 includes concluding remarks.

2. Data

This study is based upon the Barclay Hedge database, the largest publicly available database of Commodity Trading Advisors. Joenväärä et al. (2012) compare five databases (BarclayHedge, TASS, HFR, Eurekahedge and Morningstar) and find that BarclayHedge has the largest number of funds (10,520), compared to 8,788 funds in the TASS data base. Buraschi et al. (2014) summarizes two advantages of the usage of BarclayHedge database. The first advantage is this database is least likely to be affected by survivorship bias because it includes the largest number of active funds and dead funds. The second one is that it has the longest asset under management (AUM) history and the fewest missing values of AUM.

Diz (1999), Gregoriou et al.(2005), Molyboga et al. (2014), and Bhardwaj et al.(2014) document backfill/incubation, liquidation and survivorship biases in CTA databases.[1] They provide evidence that these biases can skew results of CTA performance persistence, suggesting the importance of appropriately adjusting for biases in CTA performance persistence studies.

The database includes 4,909 active and defunct funds over the period between December of 1991 and December of 2013 with the out-of-sample period between January of 1999 and December of 2013. Multi-advisors funds are removed from the analysis because they are outside of our research scope[2]. Funds with the peak value of AUM under US$ 10 million are eliminated because they are too small. institutional investors often have provisions that prevent them from representing more than 50% of AUM of any fund. These small funds also tend to have lower quality returns. Finally, the sample only includes funds that report returns net-of-fees to ensure comparability of returns.

The data filtering procedure accounts for survivorship, backfill/incubation and liquidation biases that are common for CTA and hedge fund databases. The survivorship bias is straightforward to address. The sample includes the graveyard database that contains defunct funds to account for the survivorship bias. But backfill and incubation biases are more difficult to address. Backfill and incubation biases typically arise from the voluntary nature of self-reporting. Funds usually go through an incubation period during which they build a track record using proprietary capital. If the fund's track record is attractive, then fund managers will choose to start reporting to a database to raise capital from outside investors and backfill the returns generated prior to their inclusion in the database. Since funds with poor performance are unlikely to report their returns to the database, this results in the incubation/backfill bias. Aragon and Nanda (2014) highlight significant differences in reporting and disclosures requirements for mutual funds and hedge funds. Corporate managers have limited discretion in the timing and quality of information release due to strict requirements of the U.S. Securities and Exchange Commission (SEC), GAAP and stock exchanges. In contrast, hedge funds managers are free to set their own disclosure policies when they report to public databases. Mutual funds must register with the SEC and are subject to regulations intended to protect investors (Securities Act of 1933; Securities Act of 1934; the investment company Act of 1940; the Investment Advisers Act). Unlike mutual funds, hedge funds are not required to register with the SEC and provide performance information. Malkiel and Saha (2005) suggest that the regulatory reporting requirements and audits by the regulators significantly reduce the backfill bias in mutual fund returns whereas hedge funds are able to strategically delay reporting to public databases which introduces a backfill bias in returns.

This study uses two approaches to mitigate backfill and incubation biases. The first methodology, suggested by Fama and French (2010), limits the tests to funds that reach US$ 10 million AUM in 2013. Once a fund passes the AUM minimum, it is included in all subsequent tests to avoid creating selection bias. Unfortunately, many funds, including very successful and established CTAs, originally reported only net returns for an extended period of time prior to including AUM data several years later. Using the methodology of Fama and French (2010) exclusively would completely eliminate large portions of valuable data for such funds. To include this data, the technique suggested by Kosowski et al. (2007) that eliminates the first 24 months of data for such funds is applied. The liquidation bias estimate of 1% as suggested in Ackermann et al.

[1] Bhardwaj et al.(2014) report that survivorship bias is 2.21% for volume weighted and 4.15% for equally weighted, backfill bias is 1.92% for volume weighted and 3.66% for equally weighted

[2] This study focuses on direct investments of institutional investors in funds whereas multi-advisors are fund of funds.

(1999) is also employed. After accounting for the biases, the database includes returns data for 1,753 funds for the period between December of 1993 and December of 2013 as shown Table 1. The evaluation period starts in December of 1993 because prior to that the dataset does not include defunct funds which would introduce survivorship bias.

Table 1: Data filtering information

Original dataset [a]	4,909
Eliminated funds	3,156
Funds with short track record	1,778
Funds with low AUM	1,378
Remaining Funds (Original dataset - Eliminated funds)	1,753

a. The original dataset excludes multi-advisors because this study focuses on direct investments.

This paper reports the number of funds in the original dataset and the number of funds eliminated due to short track record or low assets under management (AUM). The original number already excludes multi-advisors that are outside of the scope of this paper. 1,778 funds have 24 monthly returns or fewer after cleaning is performed (such as zeros eliminated at the end of the track record and incubation period excluded), 1,378 funds have less than US$ 10 million in assets under management during the whole duration of the study.

There are a number of benchmarks that can be used to evaluate performance of CTAs. Schneeweis and Spurgin (1996) discuss two types of commodity and managed future indices. The first group of indices is based on the returns of futures contacts and cash markets. It includes Dow Jones Futures and Spot Commodity Index, Commodity Research Bureau Index (CRB), Goldman Sachs Commodity Index (GSCI), JP Morgan Commodity Index (JPMCI), Bankers Trust Commodity Index (BTCI), and Mount Lucas (MLM) Index. The other group of indices is based on performance of CTAs. It includes the CISDM [3], Barclay, TASS, Newedge and Evaluation Associates (EACM) indices. Schneeweis et al. (2013) and Fabozzi et al. (2008) report that among them the Barclay index is not affected by survivor or backfill bias.

The Barclay CTA index is commonly used in the CTA industry because it represents bias-free performance of the industry. It currently has 535 programs included in the calculation of the index in 2015. It is equally weighted and rebalanced at the beginning of each year. To qualify for inclusion in the CTA Index, an advisor must have four years of prior performance history. Additional programs introduced by qualified advisors are not added to the Index until after their second year. [4]

In this study we use the the Barclay CTA index as the CTA benchmark. This is based upon the idea that the institutional investor first determines an allocation to the asset class and then looks for superior performance of funds within the class. The first allocation decision is typically based upon a benchmark index in comparison with other benchmarks. The risk free rate employed is the 3-month Treasury bill (secondary market rate) series with ID TB3MS from the Board of Governors of the Federal Reserve System.

3. Methodology

The standard methodologies used to evaluate performance persistence of hedge fund returns cannot typically be implemented by institutional investors. In addition to the problems of information delay, funds that are too small or do not have a long enough track record, there are simply

[3] formerly, Managed Account Reports (MAR) index

[4] see, http://www.barclayhedge.com/research/indices/cta/sub/cta.html

too many funds in existence for an institutional investor to consider. The large scale simulation framework incorporates real-life constraints, and the stochastic dominance framework is used to evaluate out-of-sample simulation results. Since simulation results are not independent, a bootstrapping procedure is used to approximate the sampling properties of the test results and allow for statistical inference.

3.1. Review of performance persistence methodologies and investment practices

Most performance persistence tests are similar to the techniques used to test for cross sectional momentum. For example, Asness et al. (2013) perform comprehensive tests for cross-sectional momentum in eight diverse markets and asset classes including individual stocks in the United States, the United Kingdom, continental Europe, and Japan as well as country equity index futures, government bonds, currencies and commodity futures. As in Jegadeesh and Titman (1993), Fama and French (1996), Grinblatt and Moskowitz (2004), they use the common measure of the past 12-month cumulative raw return on the assets, skipping the most recent months return, MOM2-12. The most recent month is typically skipped in the literature to avoid the one-month reversal in stock returns potentially driven by liquidity and microstructure issues (Jegadeesh, 1990; Lo and MacKinaly, 1990, Boudoukh et al., 1994; Grinblatt and Moskowitz, 2004). However, excluding the most recent month of returns is irrelevant in other asset classes because the one-month reversal is insignificant outside of stocks (Asness et al. 2013). The standard approach is to sort the remaining sample using the momentum measure and track performance of top third, middle third and bottom third portfolios. The Sharpe ratio and the t-statistic of alpha of the spread between the top and bottom portfolios are used to provide evidence of momentum. Implicitly, this procedure assumes that the investor is long all of the instruments in the top third and is short all of the instruments in the bottom third. Further, the instruments included in the top and bottom third may vary on a monthly basis potentially leading to high turnover. Though some other studies use deciles (Jegadeesh and Titman, 1993; Fama and French, 1996) either approach results in a finding that can be replicated by institutional investors as long as they overcome the practical challenges of portfolio rebalancing and trading expenses that include transaction costs and market impact. The issues of market impact are explicitly addressed in Korajczyk and Sadka (2004).

Similar techniques are used to evaluate persistence in performance of hedge funds (Capocci and Hübner, 2004; Kosowski et al., 2007, Jagannathan et al., 2010). Though the ranking methodologies used in the studies are very relevant for fund evaluation, institutional investors cannot directly benefit from the findings because i) the studies often ignore the delay in hedge fund reporting, thus requiring information not available at the time of investment decision, ii) consider funds that have assets under management that are too small for institutional investors, iii) have very short track records and iv) involve portfolios with the number of funds that are too large to be practical.

Examination of hedge fund returns should consider the reporting delay in hedge funds performance which is not present in mutual funds [5]. However, previous studies ignore the delay in hedge fund reporting (Capocci and Hübner, 2004; Kosowski et al., 2007; Jagannathan et al.,

[5]Performance of mutual funds can be aggregated quickly because they report their performance daily. In contrast, hedge funds and CTAs report their performance monthly and it often takes several weeks to finalize end-of-month performance values.

2010) which introduces a look-ahead bias [6]. if hedge funds are evaluated on January 1st, only returns through the end of November of the previous year are available in the database and December returns are unavailable until the end of January. In this study, a lag of one month is used to account for the delay in performance reporting of CTAs.

This article suggests a methodology that accounts for investment practices. In general, there are no hard rules describing standards of institutional investors but there are several publications that summarize best practices. According to a Greenwich Roundtable report[7], institutional investors should avoid relying on a fund's short term track records because it might overstate a fund manager's skill; while long track records that capture performance across different market conditions are more likely to provide greater insight into advantages and risks of the manager's investment approach. Therefore, we utilize two minimal requirements that are relevant for investment practices the length of track record and AUM size. However there is not universal agreement on the levels of the minimal requirements. The preceding studies consider funds with track records that are too short or very low level of AUM. Capocci and Hübner (2004) consider funds with any amount of assets under management and as little as 12 months of data. Jagannathan et al. (2010) consider funds with any amount of assets under management and the length of track record that is potentially as short as 36 months of returns, thus, potentially including funds that prudent institutional investors would not consider. Kosowski et al. (2007) require the level of assets under management of US$20 million and consider funds with as few as 24 months of data but also demonstrate that performance persistence results are not driven by the short look-back period and small funds by repeating analysis using 36, 48 and 60 months of data and considering large (above median AUM) and small (below median AUM) funds separately. Although the minimum AUM threshold level of US$20 million eliminates hedge funds with low level of assets under management, it does not account for the substantial growth in assets under management in the hedge fund space. Burghardt and Walls (2004) consider 42 managers with 10 years of data and at least US $100 million AUM as examples of established managers who have been successful at building their businesses. Barclay CTA index includes advisers with at least four years of track record. Since there are no hard rules, we apply reasonable requirements for the length of track record and AUM. We require the track record of 60 months which seems to be sufficient to draw inferences about fund manager's skill but not too long to be overly restrictive. We utilize a dynamic AUM approach that reflects the dynamic nature of the industry size and excludes the smallest 30 percent of fund managers based on their AUM to focus on the managers that are large enough for institutional investors.

Institutional investors can hold highly diversified portfolios of mutual funds, but that approach is not practical with hedge fund investments due to higher minimum investment requirements and significant oversight cost relative to marginal portfolio benefit. While mutual funds have low minimum investment requirements (around US$1,000 or less than US $1,000) to open an account and no minimum for additional subscription amounts, hedge funds require significantly higher minimum investments. Moreover, the Investment Company Act of 1940 limits investments in hedge funds to investors with at least US $5 million in investments to protect unsophisticated investors [8]. The preceding studies consider performance of decile or trecile hedge

[6]The most notable example of adjusting for data availability in academic research is the accounting book value in definition of book-to-market used in Fama-French (1992). They suggest utilizing a 6-month lag which is sufficient to account for delay in accounting reporting.

[7]see, Best Practices in Alternative Investing: Due Diligence (2010) available at http : //www.greenwichroundtable.org/system/files/BP – 2010.pdf

[8]see, http : //www.ici.org/files/faqs_hedge

fund portfolios that include a very large number of funds. Jagannathan et al. (2010) employ trecile portfolios with 252 funds and decile portfolios with 77 funds. Since institutional investors allocate to a significantly smaller number of funds, that will potentially result in a tracking error that is very high. This practical implementation issue is outside of the scope of typical performance persistence studies. In order to address all the above issues, this paper introduces a large scale simulation framework with real-life constraints that is capable of evaluating fund selection approaches in a way that is relevant for institutional investors.

3.2. Large scale simulation framework

The large scale simulation framework that we introduce in this paper is designed to evaluate fund selection approaches with real life constraints. The out-of-sample period is between January of 1999 and December of 2013, the longest out-of-sample backtesting period in empirical research of CTAs. The simulation framework uses a lag of one month to account for the delay in performance reporting of CTAs and employs 10,000 simulations. A single simulation run results in several time-series that represent monthly out-of-sample returns of equally weighted (or equally risk-weighted) portfolios of randomly selected CTAs and CTAs chosen from the top quintile based on the *t*-statistic of alpha with respect to the CTA benchmark.

3.2.1. Random CTA selection

The in-sample/out-of-sample framework mimics actions of an institutional investor who makes allocation decisions at the end of the month.[9] The first decision is made in December of 1998. Because the delay of CTA reporting, the investor has returns information through November of 1998, the investor considers all funds that have complete set of 60 months of returns between December of 1993 and November of 1998. First, the investor eliminates all funds in the bottom 30 percent of AUM among the funds considered. This relatively AUM threshold approach is more appropriate than a fixed AUM approach commonly used in the literature (Kosowski et al., (2007)) because the level of AUM has gone up substantially over the last 15 years. Then the investor randomly chooses 20 funds[10] from the remaining pool of CTAs and allocates to them either equally notionally (also known as 1/N approach) or equally after adjusting for volatility approach. Equal notional allocation (hereafter, EN) is not commonly used in momentum literature, DeMiquel et al. (2009) argue that EN outperforms most variations of mean-variance optimization in out-of-sample portfolio optimization. Equal volatility adjusted (hereafter, EVA) allocation approach is very similar to EN except that each asset's weight times its volatility is the same for each asset in the portfolio, rather than each asset's weight is the same. Volatility is estimated using sample standard deviations over the previous 60 months, allowing for a one-month reporting lag. The return of both EN and EVA portfolios is calculated for January of 1999 using the liquidation bias adjustment for the funds that liquidate during the month. At the end of January of 1999, the pool of CTAs is updated and defunct constituents of the original portfolio are randomly replaced with funds from the new pool at which point the portfolio is rebalanced again using EN and EVA approaches. The process is repeated until the end of the out-of-sample period in December of 2013. A single simulation results in two out-of-sample return stream between January of 1999 and December of 2013 - one for EN and the other one for EVA approach.

[9]Though in this paper we use monthly rebalancing which is common in managed futures due to its high liquidity, the framework can be easily modified to account for quarterly, semi-annual or annual rebalancing.

[10]The number of funds in a portfolio is a variable that can be defined for each investor. The use of 20 funds is a compromise between managing idiosyncratic risk and portfolio complexity.

3.2.2. *Restrictive CTA selection*

The in-sample/out-of-sample framework follows a very similar process when an institutional investor decides to limit the CTA pool only to those CTAs that rank in the top quintile based on the t-statistics of alpha with respect to the CTA benchmark. The first decision is made in December of 1998. The investor considers all funds that have complete set of 60 months of returns between December of 1993 and November of 1998, removes from consideration the smallest 30% of funds based in AUM (the same as in the previous simulation). Then the investor ranks all funds using the t-statistic of alpha with respect to the CTA benchmark and only considers the funds that rank in the top quintile.

In order to calculate ranking for a CTA fund i at time t (such as at the end of December of 1998 for the first investment decision period), a regression of the last 60 months of net-of-fee excess returns of the CTA fund available at that time is run on the corresponding 60 months of excess returns of the Barclay CTA benchmark I_τ

$$r^i_\tau = \alpha^i_\tau + \beta^i_\tau I_\tau + \epsilon^i_\tau \tag{1}$$

with $\tau = t-60, t-59, \ldots, t-1$. The regression is used to estimate the standard error of alpha $\sigma(\alpha)^i_\tau$ and define standard t-statistic of alpha $T^i_\tau = \alpha^i_\tau/\sigma(\alpha)^i_\tau$ as the measure used to rank all available funds. The investor randomly chooses 20 funds from the CTAs in the top quintiles and allocates to them using the EN and EVA approaches. The return of both EN and EVA portfolios is calculated for January of 1999 using the liquidation bias adjustment for the funds that liquidate during the month. At the end of January of 1999, the pool of CTAs is updated following the same procedure of ranking and the constituents of the original portfolio that do not belong to the pool anymore either because they liquidate or disqualified due to relative performance are randomly replaced with funds from the new pool at which point the portfolio is rebalanced again using EN and EVA approaches. The process is repeated until the end of the out-of-sample period in December of 2013. A single simulation results in two out-of-sample return stream between January of 1999 and December of 2013 one for the EN and the other one for the EVA approach.

3.2.3. *Bootstrapping Experiment*

Since simulation results are not independent, we use bootstrapping procedures to approximate the sampling properties of the test results and allow for statistical inference. The bootstrap approach (Efron, 1979; Effron and Gong, 1983) is a standard statistical method for evaluating the sensitivity of empirical estimators to sampling variation used when the sampling distribution is difficult to obtain analytically or a closed form solution does not exist. Since the original methodology assumes homoskedasticity in returns, it is potentially inappropriate for hedge funds and may lead to incorrect statistical inference. Literature suggests several approaches to adjusting for heteroskedasticity in the context of regression bootstrapping. Liu (1988) develops a wild bootstrap approach, designed to overcome the issue of heteroskedasticity of unknown form, an enhancement over the bootstrapping method proposed by Wu (1986) and Beran (1986). Mammen (1993) provides further evidence that the wild bootstrap is asymptotically justified for a broad range of regularity conditions. Flachaire (2005) evaluates asymptotic properties of several versions of wild bootstrap and pairs bootstrap, two bootstrap methods robust to heteroskedasticitiy of unknown form, and suggests a version of wild bootstrap with superior asymptotic properties. However, the issue of heteroskedasticity is not applicable for our dataset of Commodity Trading Advisors. An untabulated homoskedasticity test, applied to the Barclay CTA Index, results in a failure to reject the null hypothesis of homoskedasticity in returns. .[11] Therefore, the bootstrapped procedure used to draw statistical inference about momentum in CTA funds is reasonable but might need to be modified for other types of hedge funds if they have heteroskedastic returns.

We employ two bootstrapping procedures to ensure robustness of results. Both of them eliminate any cross-sectional momentum that might exist in the data but differ in the level of dependence across simulations. The first approach has very little dependence among the simulations, which is more consistent with the random CTA selection simulation, whereas the second approach has very high level of dependence among simulations, which is most consistent with the top quintile CTA selection simulation.

The in-sample/out-of-sample framework of the first bootstrapping approach, denoted by $B1$, is close to the random CTA selection simulation with the exception of replacing all 20 portfolio constituents (instead of only defunct ones) with new funds chosen randomly from the pool of available funds to eliminate any cross-sectional momentum that might have been present in the random CTA selection. The first decision is made in December of 1998. The investor considers all funds that have complete set of 60 months of returns between December of 1993 and November of 1998. First, the investor eliminates all funds in the bottom 30% of AUM among the funds considered. Then the investor randomly chooses 20 funds from the remaining pool of CTAs and allocates to them using EN and EVA approaches. The return of both EN and EVA portfolios is calculated for January of 1999 using the liquidation bias adjustment for the funds that liquidate during the month. At the end of January of 1999, the pool of CTAs is updated and a new set of 20 funds is selected from the new pool at which point the portfolio is rebalanced again using EN and EVA approaches. The process is repeated until the end of the out-of-sample period in December of 2013. A single simulation results in two out-of-sample return stream between January of 1999 and December of 2013 one for EN and the other one for EVA approach.

The in-sample/out-of-sample framework of the second bootstrapping approach, denoted by $B2$, is comparable to the simulation applied to the CTA selection that allocates to funds in the top quintile based on the t-statistic of alpha with respect to the CTA benchmark with the exception of choosing a quintile of funds randomly (without using the t-statistics of alpha) to eliminate any cross-sectional momentum that might have been present in the Random CTA selection.

The results of 10,000 simulations, performed for the CTA selection that allocates to funds in the top quintile based on the t-statistic of alpha with respect to the CTA benchmark, are compared to the results of 10,000 simulations that use Random CTA selection using mean, median and stochastic dominance tests applied to the distributions of Sharpe ratio [12] (i.e., a single simulation results in a single value of the Sharpe ratio of the out-of-sample results and 10,000 simulations give a distribution of Sharpe ratios with the sample size of 10,000).

In order to allow for statistical inference, we approximate the sampling properties of the test results using bootstrapped results (i.e., 400 distributions with 10,000 data points each) for both EN and EVA approaches.

[11] To test homoskedasticity of CTA fund returns, we establish AR(1) model as $I_{t+1} = \gamma_0 + \gamma_1 I_t + \epsilon_{t+1}$ where I_t represents the Barclay CTA benchmark at time t . The p-values of Breusch-Pagan and White's heteroskedasticity test are 0.163 and 0.168 leading to failure to reject the null hypothesis of equal volatility.

[12] We suggest using the Sharpe ratio for evaluation of performance of CTA portfolios because of ease of accessing leverage in managed futures. The stochastic dominance framework allows for alternative performance measures that could be more appropriate to other hedge fund strategies. For example, distributions of alpha with respect to a Fung-Hsieh model can be tested for stochastic dominance.

3.2.4. Stochastic Dominance Framework

Stochastic dominance (SD), documented by Hanoch and Levy (1969), Hadar and Russell (1969), and Rothschild and Stiglitz (1971), has been used in decision theory with uncertainty and as an alternative to mean-variance analysis to evaluate portfolios (Levy and Sarnat, 1970). As Porter (1973) Fischmar and Peters (1991) describe, stochastic dominance is a comprehensive measure of portfolio return and risk in that, unlike mean-variance analysis which only considers mean and variance, it utilizes entire distributions of returns to compare benefits of various portfolios to a broad set of investors without having to make assumptions about each investor's utility function. Conclusions based on stochastic dominance tests are more robust than utility function-based tests because, as Elton and Gruber (1987) point out, most investors do not even know what their utility functions look like.

Let two random variables be X and Y with their cumulative distribution functions F_X and F_Y. X has stochastic dominance of order one over Y if $F_Y(\mu) \geq F_X(\mu)$ for all μ, with strict inequality in some μ. On the other hand, X has stochastic dominance of order two over Y if $\int_{-\infty}^{\mu} F_Y(t)\, dt \geq \int_{-\infty}^{\mu} F_X(t)\, dt$ for all μ, with strict inequality in some μ.

Second order stochastic dominance is particularly attractive because it highlights the situations when all investors with any risk-averse preferences would agree that one distribution is better than the other. The results of the simulation tests demonstrate that investing in the top quintile funds has stochastic dominance of order two over random fund selection and, therefore, the suggested fund selection approach would benefit all risk-averse investors regardless of their utility function.

One of the common ways of testing for stochastic dominance is to use a type of Kolmogorov-Smirnov statistics applied to empirical distribution functions, as suggested by Klecan et al. (1991),

$$KS_1 = \min_{\mu} F_Y^E(\mu) \geq F_X^E(\mu) \tag{2}$$

for stochastic dominance of order one, and

$$KS_2 = \min_{\mu} \left(\int_{-\infty}^{\mu} (F_Y^E(\mu) - F_X^E)\, dt \right) \tag{3}$$

for stochastic dominance of order two. The values of the statistics are either negative or equal to zero because both empirical distribution functions are equal to zero in the left tail beyond the lowest point of the combined observations. Therefore, though a negative value of the statistics result in rejection of the hypothesis of stochastic dominance, a zero value is more difficult to use for stochastic inference since the tests are applied to empirical distribution functions and the results are subject to sampling error. Dardanoni and Forcina (1999) show that the probability of finding a dominance relationship based on two independent random samples of 1,000 observations can be as high as 50 percent. Kroll and Levy (1980) show examples of erroneous conclusions that result from not accounting for the sampling error in stochastic dominance tests applied to empirical distribution functions. Post (2003) and Linton et al. (2010) discuss use of bootstrapping method to account for sampling error in stochastic dominance tests and allow for statistical inference.

4. Empirical Results

In this section we evaluate the empirical results on the out-of-sample period between January of 1999 and December 2013. From the results, we find the outperformance of the restrictive fund selection relative to the random fund selection.

Table 2: Annualized Mean and Standard Deviation, and Sharpe Ratios for the Random and Restrictive Fund Selection

Quintile	Random CTA Selection			Restrictive CTA Selection		
	Ann. Avg. Return	Ann. Std. Deviation	SR	Ann.Avg.Return	Ann. Std. Deviation	SR
EN[a]	0.03	0.09	0.38	0.04	0.06	0.66
EVA[b]	0.02	0.05	0.39	0.03	0.04	0.74

a. Equal notional

b. Equal volatility adjusted

This table reports the annualized average, standard deviation, and Sharpe ratios for the random CTA selection and the restrictive CTA fund selection by employing equal notional (EN) and equal volatility adjusted (EVA) allocations.

Table 2 summarizes the performance measures including annualized mean, standard deviation, and Sharpe ratio for the portfolio rebalanced with both random and restrictive fund selection. When the equal notional allocation is applied, the annualized average return for the restrictive selection is 4%, which is slightly greater than that of random selection, i.e. 3%, while the annualized standard deviation of the restrictive selection is 0.06, which is less than that of random selection, i.e. 0.09. Also, when the equal adjusted allocation is used, the annualized mean and standard deviation of return for the restrictive method are 0.03 and 0.04, which indicate the greater return and less risk in the restrictive funds selection relative to the random selection. This results in a superior Sharpe ratio in the restrictive selection for EN and EVA: for EN, 0.38 in the random selection and 0.66; for EVA, 0.39 in the random selection and 0.74 in the restrictive selection.

Table 3 reports the average AUM threshold level for each year, the average number of funds in the random CTA selection pool and average number of funds in the top quintile between the year of 1999 and the year of 2013. The second column reports the threshold value at the bottom 30% AUM level for each period. The third column presents the average number of funds available for asset allocation over the time periods. The last column shows the average number of fund in top quintile portfolios. It shows the size of the pool of CTA funds has gradually increased up to 12 percent in the year of 2012 since the year of 1999.

4.1. Empirical results for the period from January of 1999 through December of 2013

We analyze distributions of out-of-sample returns over the complete data period using means, medians and stochastic dominance. Since simulations are correlated, we use bootstrapping results to draw statistical inference.

Table 4 summarizes across 10,000 simulation the percentage monthly means and medians of Sharpe ratios on random CTA selection and restrictive CTA selection, which only allocates to the top quintile funds, employing equal notional and equal volatility adjusted during the out of sample period between January 1999 and December 2013. For equal notional allocation, the average of percentage monthly mean and median for random selection are 0.328 and 0.326 whereas the percentage mean and median for restrictive selection are 0.616 and 0.616 respectively. For equal volatility adjusted allocation, it appears that the mean and median of the restrictive selection are greater than those of the random selection (0.292 and 0.638 for mean; 0.295 and 0.635 for median).

Table 3: The Threshold Level of AUM, the Average Number of Funds for Entire Universe and the Top Quintile Portfolio: Year 1999 through Year 2013

Year	AUM Threshold	Avg. Number of Funds	Avg. Number of Funds in the Top Quintile
1999	16,235,000	108	22
2000	13,008,333	109	22
2001	14,098,692	109	22
2002	11,773,625	117	23
2003	15,871,633	127	25
2004	20,156,742	136	27
2005	19,565,008	143	29
2006	21,254,775	146	29
2007	20,856,100	153	31
2008	25,088,608	166	33
2009	22,544,383	191	38
2010	23,794,733	204	41
2011	26,046,017	216	43
2012	24,939,333	230	46
2013	21,730,783	229	46

This table presents average threshold level of assets under management (AUM) at the bottom 30% level, number of funds available for allocation and number of funds in the top quintile for each year between 1999 and 2013. The second column shows the average AUM threshold where is ranked on 70 percentile for each period. The third column shows the average number of available CTA funds excluding bottom 30% AUM in each year. The last column reports the average number of funds that only including the upper 20% AUM.

Table 4: Annualized Mean and Standard Deviation, and Sharpe Ratios for the Random and Restrictive Fund Selection

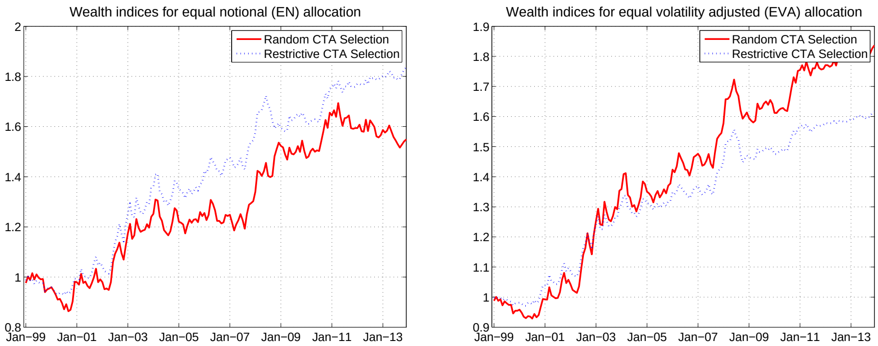
Allocation	Random CTA Selection		Restrictive CTA Selection		p-value (B1)		p-value (B2)	
	Mean	Median	Mean	Median	t-test	signed	t-test	signed
EN^a	0.328	0.326	0.616	0.616	0.00	0.00	0.00	0.00
EVA^b	0.292	0.295	0.638	0.635	0.00	0.00	0.00	0.00

a. Equal notional

b. Equal volatility adjusted

This table reports the annualized average, standard deviation, and Sharpe ratios for the random CTA selection and the restrictive CTA fund selection by employing equal notional (EN) and equal volatility adjusted (EVA) allocations.

To examine the mean difference in monthly Sharpe ratios between the random selection and the restrictive selection methods, we use the t-statistic, which tests the null hypothesis of mean equivalence. Also, to test the median difference in monthly Sharpe ratios between the random selection and the restrictive selection methods, we conduct Wilcoxon singed rank test for the median difference between the random selection and the restrictive selection in order to avoid outlier effect, which may mislead when comparing mean difference between samples. In addition, because of our simulation results are not independent, we design two bootstrapping methods to care for the robustness and the sensitivity of estimated mean and median from the empirical results. For the first bootstrapping method, all the p-values of the t-statistics for equal notional method and equal volatility method reject the null hypothesis that no mean and median difference exists between the random selection and the restrictive selection at 5 % significant level. For the second bootstrapping method, all p-values for equal notional allocation and equal volatility adjusted allocation are 0.00, which strongly indicates the null hypothesis of mean and median equivalence between the random selection and the restrictive selection should be rejected. In sum, it seems



(a) Wealth indices for equal notional allocation (EN)

(b) Wealth indices for equal volatility ajusted allocation (EVA)

Figure 1: Cumulative Risk Adjusted Returns for the Random CTA Selection and the Restrictive CTA Selection between January 1999 through December 2013

that restrictive CTA selection for both EN and EVA outperform the random CTA selection. [13]

When we look through the performance of the restrictive CTA selection and the random CTA selection with respect to time, we see the outperformance persistence of the restrictive CTA selection against the random CTA selection as shown in Figure 1, which exhibits the cumulative risk adjusted returns for the random CTA selection and the restrictive CTA selection for the period between January 1999 and December 2014. Panel A exhibits that in a case where the equal notional allocation is applied, the solid line which represents the cumulative Sharpe ratio for the restrictive selection is mostly above the dotted line representing the cumulative risk adjusted returns along with the out-of-sample period. Panel B shows that in a case where the equal volatility adjusted method is applied, the restrictive random selection that only includes the highest quintile portfolio ranked by asset under management outperforms relative to the random CTA selection along with the time horizon.

Figure 2 depicts the distributions for the risk adjusted return on the random CTA selection and the restrictive CTA selection for equal notional and equal volatility approaches during the sample period of January 1999 through December 2013. From left to right, the first box plot represents the distribution of risk adjusted returns on EVA allocation with the random selection. The second box plot displays the empirical distribution of the risk adjusted returns on EN allocation with the random selection. The third box plot shows the shapes of the risk adjusted returns on EN with the restrictive CTA selection. The last boxplot exhibits the distribution of the risk adjusted returns on EVA allocation with the restrictive selection. Because these box plots provide various statistical information as well as the shapes of risk adjust returns, it is very useful to compare the respective performance for each case: equal notional (EN) allocation with the random selection; equal notional allocation with the restrictive selection; equal volatility adjusted (EVA) allocation with the random selection; equal volatility adjusted allocation with the restrictive CTA selection. Figure 2 suggests the restrictive CTA selection for both EN and EVA better performs than the

[13]Our model uses a large scale simulation scheme that is designed to test for the performance persistence in hedge fund returns considering institutional investors constraint. In addition to test the performance persistence, our framework also examines whether there exists the outperformance of the 1/N strategy against an optimizing portfolio strategy in managing CTA funds. Jorion (1985) and DeMiguel et al. (2009) suggest that the out-of-sample performance of ex-ante naïve equally weighted portfolios may be better than optimal portfolios in short term periods. On the other hand, Fugazza et al. (2010) document an optimizing portfolio strategy exploits the 1/N rule over long term horizons. We also test whether this result holds for long term periods in managing hedge funds. The test result demonstrates that an optimizing strategy does not always outperform the 1/N rule over longer periods. Rather, we clearly observe that our restrictive CTA selection approach improves investment performance regardless of either the EVA optimizing portfolio strategy or the 1/N portfolio one. This implies that for a long term horizon the choice between an optimizing portfolio strategy and the 1/N rule is not a critical factor, but rather the selection of hedge funds is.

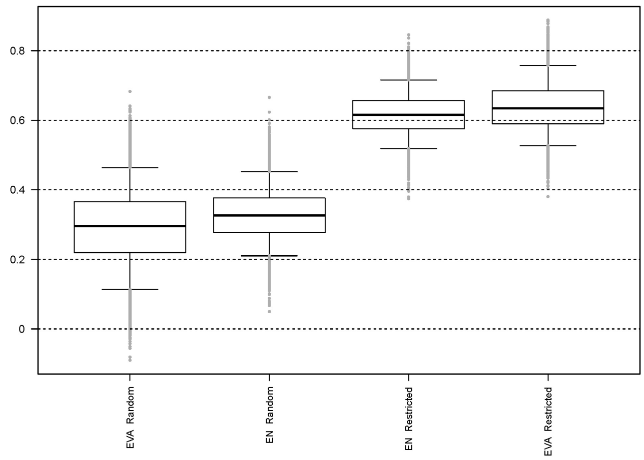


Figure 2: The Distributions of the Sharpe Ratios for the Random Selection and the Restrictive Selection by Equal Notional Allocation and Equal Volatility Adjusted Allocation

random selection in a sense that the distributions for the restrictive method for EN and EVA range between 0.37 and around 0.87 (0,37 through 0.84 for EN; 0.38 through 0.87 for EVA) whereas the distributions for the random selection for EN and EVA are a range from -0.17 and 0.69 (0.09 through 0.65 for EN; -0.17 through 0.69 for EVA).

To examine whether the performance of the restrictive CTA selection is better than the random CTA selection, we employ stochastic dominance, a comprehensive measure of portfolio return and variance. We also apply bootstrapping to treat sampling error in dominance tests as suggested by Post (2003) and Linton et al. (2010). Table 5 documents the first and second stochastic dominance test for equal notional allocation and equal volatility adjusted allocation. Panel A shows the results of the first and second stochastic dominance tests on the basis of the first bootstrapping which considers less dependence across simulations. Kolmogrov-Smirnov (KS) statistics for EN and EVA provide evidence that the restrictive CTA selection has the first and second stochastic dominance over the random CTA selection in the first bootstrapping case because all the p-values (for the first SD, 0.000 for EN and 0.000 for EVA; for the second SD, 0.000 for EN and 0.000 for EVA) result in rejection of the null hypothesis that the restrictive selection does not stochastically dominate the random selection. Panel B reports the test results of the first and second stochastic dominance for the second bootstrapping approach which consider high level of dependence across simulations. All the p-values for Kolmogorov-Smirnov test (for the first SD, 0.003 for EN and 0.003 for EVA; for the second SD, 0.003 for EN and 0.003 for EVA) reject the null hypothesis that no stochastic dominance exists between the random selection and the restrictive selection for EN and EVA. Thus, Table 5 supports argument that the restricted fund selection is superior to the random fund selection for all investors.

Table 5: First and Second Order Stochastic Dominance Tests

Allocation	First Order SD		Second Order SD	
	KS	$p(KS)$	KS	$p(KS)$
Panel A: AUM threshold level of 30% (B1 Dominates Random)				
EN^a	0.000	0.000	0.000	0.000
EVA^b	0.845	0.000	0.858	0.000
Panel B: AUM threshold level of 30% (B2 Dominates Random)				
EN^a	0.083	0.003	0.683	0.003
EVA^b	0.188	0.003	0.808	0.003

a. Equal notional

b. Equal volatility adjusted

This table reports results of the first and second stochastic dominance tests applied to distributions of Sharpe ratios derived using random and restrictive fund selection over the out-of-sample period between January of 1999 and December of 2013. The first column displays the approach used to build portfolios. The second column reports the percentage of time that the boostrapped distributions of Sharpe ratios have the first order stochastic dominance over the distribution generated using the random fund selection approach. The third column reports the p-value of the hypothesis that the restrictive fund selection does not dominate the Random fund selection using the bootstrapped approach. The fourth column reports the percentage of time that the boostrapped distributions of Sharpe ratios have the second order stochastic dominance over the distribution generated using the random fund selection approach. The fifth column reports the p-value of the hypothesis that the restrictive fund selection does not dominate the random fund selection approach using the boostrapped distribution. The results are reported using the threshold level of AUM of 30%. Panel A reports results for the first bootstrapped approach B1. Panel B present results for the second bootstrapped approach B2.

4.2. *Fung and Hsieh Factor Analysis*

Since higher Sharpe ratios can potentially be driven exposures to systematic sources of returns, we employ the Fung-Hsieh hedge funds risk factor models to test whether the restrictive fund selection results in a higher alpha (Fung and Hsieh, 2001, 2004). More specifically, we use the Fung and Hsieh five factor model and seven factor model to examine it. Fundamentally, those models are based on five trend following factors, two equity oriented risk factors, and two bond oriented risk factors. All the factors are from David A Hsiehs hedge fund data library [14] to establish the Fung and Hsieh five factor model (Fung and Hsieh, 2001) and the seven factor model (Fung and Hsieh, 2004). The Fung and Hsieh five factor model of trend following systems includes bond lookback straddle, currency lookback straddle, commodity lookback straddle, short term interest rate lookback straddle, and stock index lookback straddle. On the other hand, in order to capture the systematic risk of a well diversified portfolio of hedge funds, the Fung and Hsieh seven factor model consists of three trend following factors (bond trend following factor, commodity trend following risk factor, and currency trend following risk factor), two equity oriented market factors, and two bond oriented risk factors. The two equity oriented market risk factors are i) the equity market factor, i.e. Standard & Poors 500 index monthly total return (Datastream item: S&COMP (RI)), and ii) the size spread factor, i.e. the spread between Russell 2000 index monthly total return (Datastream item: FRUSS2L(RI)) and Standard & Poors 500 monthly index total return (Datastream item: S&PCOMP(RI)) from DATASTREAM. The two bond oriented risk factors are i) a bond market factor, the monthly change in the 10 year constant maturity yield from the Board of Governor of the Federal Reserve System [15], and ii) a credit

[14] David A. Hsiehs Data Library: https://faculty.fuqua.duke.edu/ dah7/HFRFData.htm

[15] Constant maturity yields at the Board of Governors of the Federal Reserve System: http://www.federalreserve.gov/releases/h15/data/Business_day/H15_TCMNOM_Y10.txt

spread factor, defined as the monthly change in term spread between the Moodys Baa yield [16] and 10 year constant maturity yield.

Additionally, to compare two alphas between the random selection and the restrictive selection, we test the significance of the difference between the alphas for the random selection and the restrictive selection applying bootstrap experiment as suggested by Kosowski et al (2007) and Fung et al. (2008).

Table 6: The Number of Sample Size by Equal Subsample

Periods	Number of Months
Panel A. Complete Sample Periods	
Jan. 1999 - Dec. 2013	180
Panel B. Equal Sub-samples	
Jan. 1999 - Sep. 2002 (Subperiod 1)	45
Oct. 2002 - Jun. 2006 (Subperiod 2)	45
Jul. 2006 - Mar. 2010 (Subperiod 3)	45
Apr. 2010 - Dec. 2013 (Subperiod 4)	45

This table documents the time periods used for the out-of-sample analysis with 180 month (the period between January 1999 and December 2013). The first column reports either the period the sub-period used in the analysis. The second column presents the starting date of the period, the third column displays the ending date of the period, the fourth period reports the number of months in the period. Panel A reports the values for the complete period. Panel B reports the values for the four equal sub-samples.

Furthermore, using these factor models, we conduct additional sub-sample analysis for the same AUM threshold level of the bottom 30% for 180 months from January 1999 to December 2013 so that we examine whether there is the restrictive selection outperforming pattern in alphas over sub-sample periods which are equally divided, i.e., 45 months for each periods, (Jan. 1999 Sep. 2002; Oct. 2002 Jun. 2006; Jul. 2006 Mar. 2010; Apr. 2010 Dec. 2013) as shown in Table 6.

Table 7 summarizes the Fung-Hsieh factor based performance measure for the random CTA selection and the restrictive CTA selection employing 180 months sample data between January 1999 and December 2014. All the alphas for each selection method for EN and EVA are based on the bootstrapping linear regression method so as to obtain robust estimate (see, Appendix B). To compare the coefficients of alphas between two CTA selection methods, we regress the spread in risk adjusted returns between the random fund selection and the restrictive fund selection on the Fung and Hsieh factors (see, Appendix A). Panel A exhibits the respective alphas from the Fung-Hsieh five factor model (five trend following risk factors including bond risk factor, currency risk factor, commodity risk factor, interest rate risk factor, stock risk factor) for the random selection and restrictive selection. The alphas for the random selection and the restrictive selection using EN and EVA shows that all alphas estimated from the Fung and Hsieh five factor model using the restrictive selection have greater values (for EN 0.58 and 0.60 respectively; for EVA 0.34 and 0.42 respectively), and all of t-statistics greater than 2.00 reject the null hypothesis that the alpha is not meaningful. Also, all the alpha for the difference are greater than zero: 0.07 for EN and 0.14, and all t-statistics are 2.00 standard errors from zero, which indicates that these t-statistics reject the null hypothesis and thus suggests evidence of outperforming performance

[16] Moodys Baa yield at Board of Governors of the Federal Reserve system:
http://www.federalreserve.gov/releases/h15/data/Business_day/H15_BAA_NA.txt

Table 7: Alphas from Fung and Hsieh Five Factor Model and Seven Factor Model for the Random and Restricted CTA Fund Selection

Allocation	Random CTA Selection			Restrictive CTA selection			Difference		
	α_{RND}	$se(\alpha_{RND})$	$t(\alpha_{RND})$	α_{RES}	$se(\alpha_{RES})$	$t(\alpha_{RES})$	α_d	$se(\alpha_d)$	$t(d)$
Panel A. Fung Hsieh Five Factor Model									
EN[a]	0.58	0.19	3.03	0.60	0.16	3.87	0.07	0.02	3.49
EVA[b]	0.34	0.11	3.04	0.42	0.11	3.83	0.14	0.01	13.85
Panel B. Fung Hsieh Seven Factor Model									
EN[a]	0.27	0.05	5.38	0.33	0.03	9.48	0.06	0.02	3.12
EVA[b]	0.14	0.02	7.95	0.27	0.02	13.56	0.13	0.01	12.18

a. Equal notional

b. Equal volatility adjusted

This table shows the Fung-Hsieh factor based performance measure for the random CTA selection and the restrictive CTA selection employing 180 months sample data between January 1999 and December 2013. All the coefficients for each selection method for EN and EVA are based on the bootstrapping regression method (thousand iterations) to obtain robust estimate. Panel A exhibits the respective alphas from the Fung-Hsieh five factor model (five trend following risk factors including bond risk factor, currency risk factor, commodity risk factor, interest rate risk factor, stock risk factor), for the random selection and restrictive selection, and the difference between two selection method in their expose to alphas. Panel B exhibits the respective alphas from the Fung-Hsieh seven factor model (equity market factor, size spread factor, bond market factor, credit spread factor, bond trend following factor, currency trend following factor, commodity trend following factor) for the random selection and restrictive selection, and the difference between two selection methods in their expose to alphas. α_{RND} represents the intercept of the Fung-Hsieh factor model for the random selection, while α_{RES} represents the intercept of the Fung-Hsieh factor model for the restrictive selection. $\alpha_d = \alpha_{RES} - \alpha_{RND}$ denotes the difference between the random selection based alpha and the restrictive selection based alpha.

in the restrictive fund selection. Panel B exhibits the respective alphas from the Fung-Hsieh seven factor model (bond trend following factor, currency trend following factor, commodity trend following factor, equity market factor, size spread factor, bond market factor, credit spread factor) for the random selection and restrictive selection, and the difference between two selection methods in their expose to alphas. It also shows all the alphas for EN and EVA for the restrictive selection are greater than those of the random CTA selection and supports this difference in alphas is statistically significant at 5 percent significant level.

Table 8 shows the Fung-Hsieh five and seven factor based performance measure for the significance difference of the alphas for the random CTA selection and the restrictive CTA selection employing four subsample data, which is equally divided to 45 months, for the period January 1999 through December 2013. Panel A, B, C, and D show the respective alphas from the Fung-Hsieh five and seven factor models for the difference between two selection method in their expose to alphas for EN and EVA by four subperiods: January 1999 to September 2002; October 2002 to June 2006; July 2006 to March 2010; April 2010 to December 2013, respectively. Except for Panel C, all the coefficients for $\alpha_d = \alpha_{RES} - \alpha_{RND}$ are positive which shows the greater alpha in the restrictive CTA fund selection. In Panel A, B, and D, most of the t-statistics of α_d reject the null hypothesis that there is no significant difference. However, when looking at the result of Panel C, all the alphas for EN and EVA based on the five model and the alpha for EN for the seven factor model are negative, which seems to say that the random CTA fund section is better performing but all the t-statistics for those negative alphas provide the insignificance of those alphas.

Table 8: Alphas from Fung and Hsieh Five Factor Model and Seven Factor Model for the Random and Restricted CTA Fund Selection

Allocation	Fung-Hsieh Five Factor Model			Fung-Hsieh Seven Factor Model		
	α_d	$se(\alpha_d)$	$t(d)$	α_d	$se(\alpha_d)$	$t(d)$
Panel A. Subperiod1 (Jan. 1999 - Sep. 2002)						
EN^a	0.33	0.24	1.37	0.06	0.02	3.12
EVA^b	0.42	0.08	5.56	0.13	0.01	12.18
Panel B. Subperiod2 (Oct. 2002 - Jun. 2006)						
EN^a	0.06	0.06	0.95	0.07	0.02	3.49
EVA^b	0.01	0.02	0.47	0.14	0.01	13.85
Panel C. Subperiod3 (Jul. 2006 - Mar. 2010)						
EN^a	-0.07	0.07	-0.87	-0.03	0.09	-0.35
EVA^b	-0.01	0.03	-0.27	0.14	0.05	2.96
Panel D. Subperiod4 (Apr. 2010 - Dec. 2013)						
EN^a	0.15	0.08	1.89	0.07	0.02	3.49
EVA^b	0.11	0.04	2.66	0.14	0.01	13.85

a. Equal notional

b. Equal volatility adjusted

This table shows the Fung-Hsieh five and seven factor based performance measure for the significance difference of the alphas for the random CTA selection and the restrictive CTA selection employing four subsample data, which is equally divided to 45 months, for the period January 1999 through December 2013. All the coefficients for each selection method for EN and EVA are based on the bootstrapping regression method to obtain robust estimate. Panel A, B, C, and D show the respective alphas from the Fung-Hsieh five and seven factor models for the difference between two selection method in their expose to alphas by four subperiods: January 1999 to September 2002; October 2002 to June 2006; July 2006 to March 2010; April 2010 to December 2013, respectively. $\alpha_d = \alpha_{RES} - \alpha_{RND}$ denotes the difference between the random selection based alpha and the restrictive selection based alpha. i.e. α_{RND} and α_{RES} restrictively. $se(d)$ represents the standard error and $t(d)$ refers to the t-statistics of α_d.

5. CONCLUDING REMARKS

In this paper we have discussed some of the key issues with standard tests for anomalies in hedge fund returns that follow methodologies from other asset classes. Standard momentum techniques are relevant to institutional investors when applied to underlying assets because investors can relatively easily build large long-short portfolios of winners-losers and rebalance them monthly appropriately accounting for transaction costs (see Korajczyk and Sadka ,2004). However, the same techniques, used to evaluate performance persistence in hedge funds in the literature, i) ignore the delay in hedge fund reporting, thus requiring information not available at the time of investment decision, ii) consider funds that have assets under management that are too small for institutional investors, iii) have very short track (sometimes as low as 12 months) and iv) involve portfolios with the number of funds that are too large to be practical.

We have introduced a set of tests for anomalies in hedge fund performance based on a large scale simulation framework designed to test portfolio management approaches consistently with requirements of large institutional investors. We suggest using second order stochastic dominance methodology to evaluate out-of-sample results and a bootstrap procedure to approximate the sampling properties of the test results and allow for statistical inference. We apply the new approach to test for performance persistence in hedge funds in the managed futures industry over the out-of-sample period between January of 1999 and December of 2013 and we find that two simple rules for selecting CTA funds for portfolios of institutional investors first excluding funds in the bottom 30% of the CTAs with at least 60 months of data in terms of assets under management and second selecting funds that rank in the top quintile based on the t-statistic of alpha with respect to a CTA benchmark result in a significant improvement of performance. We evaluate robustness of results across time period and find that our screening procedure consistently adds value with the exception of a relatively short data period. Our set of tests based on the simulation framework has practical importance for institutional investors because it helps discover easily implemented rules that can result in statistically significant improvements in investment performance as demonstrated in the case of momentum-based rules for hedge funds in the managed futures industry.

Appendix A. Comparison of the Coefficients of the Alphas for the Random Fund Selection and the Restrictive Fund Selection

Let α_{RES} denote the alpha for the restrictive fund selection and let α_{RND} denote the alpha for the random fund selection. Next, let $d = \alpha_{RES} - \alpha_{RND}$ denote that the difference between the restrictive selection based alpha and the random selection based alpha. In large sample case, under the assumption of equal variance we can test the significance of alphas between two fund selections as follows

$$Z = (\alpha_{RES} - \alpha_{RND})/[s^2(\alpha_{RES}) + s^2(\alpha_{RND})]^{1/2} \tag{A.1}$$

where $s^2(\alpha_{RES})$ represents the variance of the alpha for the restrictive selection, $s^2(\alpha_{RND})$ represents the variance of the alpha for the random selection, Z follows a standard normal distribution. However, this equal variance assumption is undesirable and practical for the comparison for the alpha estimates between two fund selections. Instead, we simply run bootstrapping linear regression of the spread between the restrictive selection and the random selection in risk adjusted return difference, i.e. the risk adjusted return for the restrictive fund selection less the risk adjusted return for the random fund selection, on the Fung and Hsieh factors. Let $R_{RND} = \alpha_{RND} + \beta_{RND}X$ be the linear regression model for the random selection and $R_{RES} = \alpha_{RES} + \beta_{RES}X$ be the linear regression model for the restrictive selection. Then the difference of two regression model is written as

$$R_{RES} - R_{RND} = (\alpha_{RES} - \alpha_{RND}) + (\beta_{RES} - \beta_{RND})X \tag{A.2}$$

and then

$$R_d = \alpha_d + \beta_d X \tag{A.3}$$

where $R_d = R_{RES} - R_{RND}$, $\alpha_d = \alpha_{RES} - \alpha_{RND}$, and $\beta_d = \beta_{RES} - \beta_{RND}$. Then from this we can easily test the null hypothesis that

$$\alpha_d = \alpha_{RES} - \alpha_{RND} = 0 \tag{A.4}$$

and use t-statistics

$$t(\alpha_d) = \alpha_d/s(\alpha_d) \tag{A.5}$$

for the test of the significance of alphas between two fund selections.

Appendix B. Alpha Estimations from the Bootstrapping Regression

The t-statistics of OLS may mislead if errors are non-normally distributed and violate i.i.d condition. As Kosowski et al. (2007) and Fung et al. (2008) suggest this article estimate the alphas of two group of funds based on the bootstrapping regression method to avoid type-I error in estimating alphas and t-statistics of alphas to examine the validity of the alphas. We describe the bootstrapping procedure as follows.

Step 1. Regress the risk-adjusted return on the Fung-Hsieh risk factors for each fund i as

$$R_{i,t} = \alpha_i + \beta_i X_t + \epsilon_{i,t} \tag{B.1}$$

and estimate the residual as

$$\hat{\epsilon}_{i,t} = R_{i,t} - \alpha_i - \hat{\beta}_i X_t \tag{B.2}$$

Step 2. Draw T periods from $t = 1, \ldots, T$ and produce a bootstrap sample by sampling $\epsilon_{i,t}$. Denote X^b as a bootstrap sample where b is the number of bootstrapping. Denote the resample periods as $t = s_1^b, s_2^b, \ldots, s_T^b$.

Step 3. Construct the resampled observations

$$R_{i,t}^b = \hat{\alpha}_i + \hat{\beta}_i X_t^b + \hat{\epsilon}_{i,t} \text{ for } t = s_1^b, s_2^b, \ldots, s_T^b \tag{B.3}$$

Step 4. Run the regression as

$$R_{i,t}^b = \hat{\alpha}_i^b + \hat{\beta}_i^b X_t^b + \hat{\epsilon}_{i,t} \text{ for } t = s_1^b, s_2^b, \ldots, s_T^b \tag{B.4}$$

Step 5. Repeat step 2 for $b = 1, \ldots, B$ and compute $t(\alpha_i)$ using the distribution of the standard bootstrap standard error of the alpha.

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