



When Active Management Matters

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August 10, 2010

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Financial planners have eagerly awaited any research that could finally, definitively prove – or disprove – the pesky notion that active management is effective. Though no one has yet risen to that challenge, past academic studies have been improperly interpreted to show that portfolio policy, or asset allocation affects portfolio returns far more than active management.

The most recent study to tackle the active management debate, by Yale professor Roger Ibbotson, shares two weaknesses with previous research. As a result, its methodology masks the value of actively managing a fund's asset allocation and consequently it fails to reward "eclectic," unconstrained active managers.

Before exploring those weaknesses, we will summarize the previous research on this topic, and once we've finished we will conclude with the lessons this research holds for financial advisors.

Background: Research on the roles of active management and asset allocation

Perhaps the best known study on the subject of active management and asset allocation was by *The Determinants of Portfolio Performance*, by Brinson, Hood, and Beebower (BHB), published in 1986. That study was misinterpreted by the industry to say that 93.6% of portfolio *return* is determined by asset allocation policy when the study actually concluded that 93.6% of portfolio *volatility* is determined by portfolio policy. Many key conclusions of the study were subsequently refuted by William Jahnke in his paper, *The Asset Allocation Hoax*, which Jahnke published in February of 1997.

Most recently, Roger Ibbotson has published two papers on the subject in the March/April issue of *Financial Analysts Journal*: *The Importance of Active Management* and *The Equal Importance of Asset Allocation and Active Management*. The latter article was coauthored by James Xiong, Thomas Idzorek, and Peng Chen. The papers attempt to take a fresh look at the impact of active management (and other factors) on fund performance, and in the process it addresses criticisms of the original BHB study.

Unfortunately, we believe that Ibbotson's conclusions have limited application, because the study is based on a universe of managers who are severely constrained in their capacity to be "active." In addition, Ibbotson's methodology may minimize the apparent impact of



active management when managers are “significantly” active in their portfolio allocations. Accordingly, it implies an erroneous conclusion that all forms of active management have a relatively small impact on portfolio returns.

Ibbotson and his coauthors assume three factors explain the variation in portfolio returns: market returns, the excess returns from asset allocation policy, and the excess returns from active management. The authors study the performance of 4,641 U.S. equity funds, 587 balanced funds, and 400 international equity mutual funds in the Morningstar database from May of 1999 to April of 2009. They initially conclude that market returns have a disproportionately large impact on portfolio returns – more than 80% of returns are attributable to market movements alone, they found. They then removed the impact of market returns (a change from prior studies) and determined that portfolio policy and active management have a roughly equal impact on determining any remaining excess returns over and above the market’s returns.

The title of their paper, *The Equal Importance of Asset Allocation and Active Management*, refutes the erroneous notion that asset allocation (portfolio policy) determines 93.6% of portfolio returns. Instead, Ibbotson and his coauthors conclude that raw market returns drive the majority of portfolio returns (approximately 80%). Portfolio policy only determines half of the remaining 20%, which is the excess portfolio returns above market returns. Active management determines the other 50% of excess performance above market returns. Although the authors present their findings as precipitating a pronounced shift in how we understand the “importance” of a portfolio’s asset allocation, they still conclude that the market and portfolio policy determine approximately 90% of portfolio returns.

Two reasons to be cautious

Financial planners should consider two weaknesses in the methodology of this study before agreeing fully with Ibbotson’s conclusions about the relative importance of market returns, portfolio policy, and active management.

First, planners should question the methodology used by Ibbotson and his coauthors to measure the importance of changes in a fund’s asset allocation. While their approach may be an effective way to examine the typical asset-class-constrained fund manager, problems arise if a fund manager actually makes active tactical changes to the asset allocation of his fund (a behavior that is admittedly stigmatized by MPT adherents as “market timing”). The study attempts to determine what produces the excess returns (over and above the market) by using a returns-based style analysis to determine the amount of excess performance attributable to the asset allocation policy decision of the fund.

Returns-based style analysis, originally developed by William Sharpe (1992), uses regression to determine an estimate of the passive asset allocation of the fund. In the Ibbotson study, he and his coauthors assign the difference between the returns of *that* asset allocation and the general market to the performance attributable to portfolio policy.



After determining how much of the fund's return is due to a combination of portfolio policy and the market returns of that asset allocation, the remaining excess return of the fund is presumed to stem from active management. As mentioned earlier, the study concludes that active management and portfolio policy are roughly equal contributors to excess returns over and above the returns of the market universe.

Unfortunately, a returns-based style analysis like Ibbotson's may mask the benefits of active management when there are significant changes in exposure to various asset classes. Let's use the Hussman Growth Fund, an eclectic fund where the manager insists his benchmark is the S&P 500 Index, as an example. Depending on market valuation and Hussman's evaluation of current market conditions, he uses hedging strategies to vary his equity exposure from zero to 150% long. Presently, Hussman has significantly reduced his equity exposure, and a regression-based style analysis of his recent performance (like that used in the study) would say Hussman's portfolio policy is 100% cash. Over a ten-year period, let's assume Hussman continues to remain fully hedged in an ongoing secular bear market. The study's methodology would conclude that his portfolio policy (asset allocation benchmark) was 100% cash, even though his actual benchmark was the S&P 500 Index.

Comparing Hussman's actual returns (which, in a "market neutral" position we will assume equal cash returns even though his actual performance may significantly vary due to underlying stock selection) to his policy return (100% cash) could reveal little to no outperformance. The authors would erroneously conclude that his performance – which, by avoiding stocks, may have significantly outstripped that of the S&P 500 in a bear market – was due to the *fund's* portfolio policy instead of the fund *manager's* active management decision, when in reality the entire asset allocation decision itself *was* an active management decision!

Or perhaps even worse, the study would conclude Hussman was a terrible manager, as his fully-hedged returns may lag a cash benchmark (thanks to the impact of fees). Mr. Hussman would receive absolutely no credit for his active management decision to completely avoid a bear market and remain fully hedged. We argue that Hussman's excess returns versus the S&P 500 Index in this example would be entirely a result of his active management, since he used his skill and judgment to vary his actual asset allocation from his stated benchmark.

Style analysis along the lines described above –because it is measured on a retrospective basis – unfairly changes a manager's benchmark itself, after the fact, based on his or her skillful changes to the fund's asset allocation over time. In other words, returns-based style analysis implicitly assumes that the manager's *benchmark* is the same as the manager's regression-based asset allocation. In an asset-class-constrained world, this may turn out to be true; but for managers who have greater investment flexibility, the two may be materially different, and the measured value of active management can vary significantly if the "wrong" benchmark is used.



The study's other major weaknesses is a shortcoming that has been common in research on managed portfolios through the years. The problem is to properly identify what constitutes "market returns" and to specifically identify what constitutes "the market" for unconstrained, "eclectic" active managers.

Using the old paradigm of strategic asset allocation, the "market" is usually considered to be the aggregate performance of an asset class (e.g., "stocks"), and can be even more refined as a style-constrained allocation within a particular asset class (e.g., just "large cap value stocks"). By parsing the investment universe into easy-to-define asset classes and asset class styles, consultants and planners can readily identify a reasonable benchmark for active managers (within that asset class or asset class style), and researchers can use all manner of statistical techniques to determine whether active managers are delivering alpha. The asset class and its performance become the benchmark, and the resulting skewed conclusions about the importance of the "market" in determining returns are reflected in papers, like Ibbotson's recent effort.

In today's world, however, fund managers are evaluated relative to their style box, and accordingly they tend to remain "style-constrained" within that box. U.S. equity fund managers are highly likely to own U.S. equities and little else if they want to remain evaluated in the U.S. equity realm (and, in fact, they are likely to stay within a particular style box *within* U.S. equities, such as large-cap growth or small-cap value). As a result, fund managers are not "eclectic" in their choice of asset classes, but instead confine themselves to the asset classes that meet the Morningstar criteria. Managers who fail to meet those criteria are guilty of "style drift" and will not be recommended by consultants who work with asset allocators.

Therefore, it is hardly surprising that regressing the returns of any particular style-constrained international fund manager against the equal-weighted returns of the universe of equally style-constrained international fund managers (the market) would reveal anything other than a conclusion that 80% of fund returns can be explained by the "market." The same could be said of the universe of U.S. equity managers and U.S. balanced managers. There is an incredibly powerful self-selection bias for most of those managers to predominantly own that exact shared view of "the market" in the first place.

Instead, imagine a universe of non-style-constrained fund managers with the freedom and flexibility to invest in virtually any acceptable institutional-quality asset class, including cash, bonds, U.S. and international equities, real estate, commodities, managed futures, etc. Imagine that these fund managers are also free to pursue strategies such as market neutral, arbitrage, timing, momentum, and others that are specifically designed to minimize the impact of market returns. In this world, there is no clear way to define "the market" when it includes so many different asset classes, manager types and strategies.



The standard approach is to measure the equal-weighted returns of all funds or managers in this universe. But what does that tell us about the determinants of portfolio returns for the eclectic managers in our hypothetical universe, where their asset allocations can significantly change over time? We suspect that such benchmarking numbers would not be particularly useful, and few planners would agree that they represent “market” returns.

Conclusions

For advisors looking for proof that active management really works, both of these problems will provide continuing frustration. Ultimately, both relate to the challenge that if and when investment managers can own multiple asset classes – as they are increasingly doing, thanks to the rising popularity of tactical asset allocation investment approaches – it is increasingly difficult to determine what constitutes “the market” and in turn identify what returns are attributable to a manager’s investment policy as opposed to active management decisions about various asset classes.

To our knowledge, there is no publicly-traded “universe” of managers that is sufficiently non-style-constrained. There are, perhaps, a few select managers that engage in such behavior, but in point of fact they do *not* fit in the general universe of funds; hence, why they are called “eclectic” managers? Perhaps the hedge fund universe is the only one that meets our “active” criteria, and it has problems in terms of survivorship and backfill bias. And either way, as noted earlier, there will be little agreement on what is the proper benchmark to use as a proxy for “market returns” in order to compare hedge funds’ performance to the “market.”

Financial planners who read these latest studies and conclude that 80% of returns are attributable to the market, and that any excess returns over market returns are equally attributable to portfolio policy and active management, should consider the limitations of this study. Insofar as Ibbotson, Xiong, Idzorek, and Chen set out to prove that if you limit your universe to narrowly style constrained managers, you can get insights into the impact of active management *amongst that particular peer group of funds*, we wholeheartedly agree with them. Nonetheless, as the authors themselves point out, when you consider “eclectic” active fund managers, the results will likely be dramatically different than the average results of those style-constrained managers. If tactical asset allocation is truly an emerging trend of investing, studies like the ones discussed here tell us little about the expected or potential value and relative impact of active management returns on a prospective basis – especially when the active management includes ongoing changes to a fund’s asset allocation.

The original BHB paper has been cited as the most misunderstood and misinterpreted paper in popular finance. Its widely and wrongly accepted conclusion that 93% of portfolio returns are attributable to portfolio policy have been the cause of great mischief over the years as financial planners have interpreted the result to mean that strategic and passive asset allocation has been proven to be the most effective way to manage portfolios.



The Ibbotson paper is a useful addition to the literature on the subject, but financial planners interested in active management should not over-generalize its findings. For eclectic managers who implement management strategies involving actively and tactically changing portfolio asset allocations – which these studies cannot properly measure using their methodology – active management will be a *dramatically* more powerful determinant of portfolio returns than these studies suggest.

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