Seeking Alpha Managers

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Past performance puzzle

When I was a graduate student at the University of Chicago in the mid-90s, one of the biggest puzzles in investment management was the lack of performance persistence amongst managers.

Research showed that selecting managers based on their returns over the previous several years did not result in similar performance in subsequent years. Part of the reason the phrase "Past Performance is Not Indicative of Future Results" just might be the truest statement in investment management is because there is a lot of academic evidence supporting it!

Interestingly, the research showed that while it was difficult picking future top performing managers based on previous returns, there was somewhat higher persistence that the very worst performing managers stayed that way going forward in many instances. However, it's not that these managers were such horrible stock pickers perpetually; rather, it was more the case that many of them extracted a lot of returns for themselves in the forms of fees and in some instances via trading schemes.

It is important to note that while most of my comments refer to equities, the same can be applied to bonds markets, commodities, etc. It's just that the data available for stocks is so prevalent that it lends itself well to academic research.

Is it possible that this lack of performance persistence means there is no skill in stock picking or investing in general? The recent wave into passive investing would certainly have you believe so. However, many people would agree that there is skill in investing as in many other things in life.

Tennis, poker and stock picking

Take tennis for instance – if everyone at your work participated in an office tennis tournament, chances are high that some of the employees would consistently fare well. It could be due to the fact that they played competitive tennis in college for instance. In other words, there would certainly be performance persistence amongst the better tennis players in the office tournament.

What about poker? It could be argued that poker is a game with a similar mix of skill and luck as picking stocks. Why is it that a lot of the same people end up at the final table in poker tournaments? It might be due to the fact that they are more skilled than most others.

Given all of this, how can we unravel the puzzle of lack of performance persistence amongst active managers? Before we even attempt to delve into this, let's first ask ourselves what we even really know about a fund manager.

Broadly speaking, we know two things about each fund manager:

1. Past performance (i.e. their track record)
2. The strategy and people at the fund

Let's dig into each one separately.

Past performance: Do we really know who did well last year?

Before we can even figure out how to use past performance winners to determine whether any future performance persistence exists, we would need to be certain that we can even determine who did well in the past. It sounds obvious but as we will see shortly, it is actually quite challenging to even agree on who did well last year.

Will the real winner please stand up?

Let us assume that we have two private equity managers – Amy and Kerry – and that they have managed their respective funds for 10 years.

- Amy boasts that her performance was terrific at 16% annualized over the past 10 years
- Kerry retorts that his fund delivered 18% a year and therefore, he did better than Amy
- Amy responds by arguing that her fund averaged two-fold leverage over the life of her fund, whereas Kerry had three-fold leverage. Therefore, after you adjust for leverage, she beat Kerry.
- Kerry comes back by pointing out that Amy clearly didn't understand that the market was so bullish and mistakenly didn't lever more, so he should get credit for his leverage level. Moreover, Kerry argues that even though he averaged three-fold leverage, he leveraged earlier in the period, and Amy did so later in the period and the real outperformance was in the later part of the period. As a result, Kerry argues that you have to perform a beta adjustment year-by-year or ideally month-by-month and in doing so, one would observe that Kerry won.
- Amy responds by saying that she was smart enough to lever at the right time. Moreover, she argues that her fund was mostly in energy whereas Kerry was largely in healthcare. Given that energy had a tough run, Amy argues she is the more impressive manager.
- Kerry responds by saying that he is smarter for having focused on healthcare…
- Etc. etc. etc. You see that this could go on forever (and it sometimes does in our investment committee meetings at Alignvest!).

The point of this exercise is that there are a series of choices that a manager makes, and as an investor or fund researcher you have to decide whether it is a headwind or a tailwind that we should adjust for and whether it is a part of the manager's skill or not.

It is difficult if not virtually impossible to dissect thousands of managers in the world in this way to form an academic study. It is also quite challenging to do it as an investor unless you have the knowledge, resources, skill and time to do so properly. However, it can certainly be done and we do so as part of our role here at Alignvest. We do it by accessing managers directly as part of our due diligence process and by considering the dozens of different decisions that each manager made and grouping them into good or bad skills and headwinds or tailwinds all in an effort to seek outperforming managers.

What about the strategy and the people?

Decisions, decisions…

As we just demonstrated, it is very challenging to determine who even did better the previous year in a straight-up comparison between two similar managers. Now let us apply the same logic as above to a single manager in an effort to determine what real skill – if any – that manager may have.

Imagine that we run an endowment together and that we must select an equity manager focused on U.S. equities. We award the manager a fairly open mandate, which in this case means the manager can buy any U.S. stock. This manager then decides to buy 100 small cap stocks and proceeds to a) significantly outperform the U.S. small cap index and b) underperform the broader U.S. equity market (as small caps trailed the overall market for the year).

Is this a good manager? What evidence do we have that the manager shows skill? One could argue that we observe a medium amount of evidence that the manager has stock selection ability given the clear outperformance versus the small cap benchmark. We also have a small amount of evidence on the macro side as the manager made a wrong choice by deciding to go into small caps as opposed to say larger cap securities – but one single macro call in one single year is a lot less information than the 100 small cap selections provide.

Now, if we *consistently* observed that a manager shows immense stock selection skill and poor or neutral sector selection ability, we could fund our investment in the manager in a way to neutralize the sector decision but profit from the alpha stemming from stock selection (portable alpha). As long as we didn't pay too much for this skill, the manager may add value to our portfolio.

Lot little, little lot world

In the world of liquid securities, there is a lot mispriced by a little and a little mispriced by a lot. I discussed this at length with my colleagues in the paper, "The Price Is (Almost) Right[1]."

On any given day, there are hundreds of liquid stocks in the U.S. that are mispriced by a couple percent even after factoring in known information. On any given day, there are dozens of U.S. stocks mispriced by a lot – perhaps 5-10%. Finally, there could be a handful of stocks mispriced by 30-50%, but this is very rare and hard to prove academically (due to such a small sample size).

In an effort to show that stocks being mispriced by 5-10% is a lot, let us use activist managers to make our point. Activist investors only have to pick a few stocks per year – which represent their very best ideas. Studies show that once a stock is publicly announced as an activist pick, it jumps around 7% on the news. Interestingly, the data shows that after this initial pop, there is little continued outperformance, or underperformance – the stock performs in line with the market.

It turns out that activists really do show evidence of stock selection skill as well as an ability to beneficially influence companies and a wide variety of stock metrics support the 7% price jump.

Finally, given that activists may turn over their portfolio more than once a year, historical average alpha could actually be better than 7% on an annualized basis.

For the majority of investment professionals, being able to allocate to only several securities is not within their mandate. If you are a traditional mutual fund manager, you might pick 100 stocks within your fund. Let us assume that you bought 45 of them 2% cheap (a lot mispriced by a little) and that you purchased another 45 that you thought were 2% cheap but were actually fair value. Moreover, on five picks, you

[1] "Randolph B. Cohen, Christopher Polk, and Tuomo Vuolteenaho. (December 2009). The Price Is (Almost) Right. The Journal of Finance • Vol. 64, No. 6.

overpaid by 2% and on the remaining five stocks you did really well as they turned out to be underpriced by 5-10%. If you take these numbers, you are going to get approximately 100 or 200 basis points a year of pre-fee, pre-trading costs outperformance, and this is supported if we look at research led by Russell Wermers[2] from The University of Maryland.

Dr. Wermers painstakingly gathered all of the stock holdings information for a huge swath of mutual funds in order to determine what performance would have been without any fees or trading costs. In essence, he wanted to know why the average manager could not beat the market. However, his research showed that the average U.S. mutual fund manager actually does beat the market – by around 130 basis points (1.3%) a year. Unfortunately, after removing trading costs of approximately 80 basis points and fees of around 80 basis points (which of course would be *much* higher for the average retail investor), the average manager's stock picks delivered *minus* 30 basis points relative to the index. Interestingly, Wermers argued that the market is at an equilibrium with index funds because they also slightly underperform the market due to trading costs and fees (although index fund fees have come down since the Wermers paper).

It is interesting that people often criticize hedge funds for taking a 20% performance fee. Given the above data on long-only mutual funds, it can be argued that they are taking 160% of the alpha they produce (80 basis points in fees on 50 basis points of alpha)!

The things managers do for money…

I have seen four different methods that managers employ to deliver really substantial outperformance (this used to be mean double digit annualized alpha and now high single digit is considered substantial given where interest rates are today). Here are the four things to look for:

Concentration

We have all heard a manager deliver an extremely compelling stock pitch. They appear to have done endless research on the stock, its management, financials, opportunities, etc. You then ask them how many positions are in their portfolio and they tell you 120. It is hard to believe that anyone would have the time and resources and knowledge to beat the combined market wisdom on 120 different stocks simultaneously.

Ideally, investors should want exposure to a manager's very best ideas only. This gives you the best chance of getting all of your names from the "little mispriced by a lot", which is that 5-10% group. However, it is possible that each manager may only have a few to several such ideas.

In a paper I co-authored called "Best Ideas[3]", our research showed that a manager's highest conviction bets significantly outperformed the remainder of their fund – by around 4-5% a year or more in some cases! Highest conviction does not necessarily mean the top five holdings in the fund ranked by size; if you own 2% of the portfolio in a stock representing a tiny percentage of the index, this would be an example of a very high conviction idea.

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[2] "Russ Wermers. (August 2000). Mutual Fund Performance: An Empirical Decomposition into Stock-Picking Talent, Style, Transactions Costs, and Expenses. The Journal of Finance • Vol. 55, No. 4.

[3] "Randolph B. Cohen, Christopher Polk, and Bernhard Silli. (November 2005, revised May 2010). Best Ideas. SSRN.

Now here comes the frustrating part of this whole concentration thing. Managers usually won't give investors access to only their best ideas. They do this for three primary reasons: 1) they are afraid of getting fired when inevitably one of their stocks blows up and it results in a large loss for the fund, 2) a five-stock portfolio is much more limited in terms of capacity than a 100-stock fund and 3) the clients also often insist on diversification from each and every one of their managers!

This all makes little sense conceptually. The conversation that should be happening is a convergence between manager and clients whereby fees go up a little and liquidity goes does somewhat but the end result is a concentrated fund made up only of the highest conviction ideas. Moreover, the client handles the diversification concern by investing in many concentrated managers – each with a different focus or area of expertise.

If concentration means having a fund with only several great ideas at any one time, how do we define focus? Focus is being specialized in a particular area or sector. Ideally, it involves some dark little corner of the market that is less efficient. Examples include South East Asian retail stocks, or one of many Frontier Markets or even mid-cap, non-resource companies in Canada that have little research coverage.

Putting it all together, one should want a diversified portfolio of concentrated and focused managers across many industries and countries around the world.

Leverage

Another way to increase returns is adopting leverage of course. We all know the pros and cons of this strategy as leverage certainly amplifies gains but can also do the same for losses. However, judicious use of leverage can certainly be a healthy part of a fund's strategy. Especially if one is invested across many uncorrelated strategies in their portfolio.

The real problem with leverage is that it can turn temporary losses into permanent ones. Suppose you are invested in a carry trade whereby you are borrowing Japanese bonds to buy New Zealand bonds and that you employ 10:1 leverage back in the old days for hedge funds. Everything is fine on the way up but if a financial crisis hits like it did in 2008, your fund might be down 7% as the spread widens – that translates into a 70% loss factoring in the leverage!

It is certainly a smart adage when Warren Buffet says that he will take the bumpy road on the way to large gains over time (i.e. ignore shorter term volatility) but in this case, you can't simply hold on – despite the fact that those Kiwis will certainly honour their obligations and the bonds will likely come back to full value quickly. The explanation for this is that on the day you are down 70%, your prime broker will cut your leverage from 10:1 to 3:1 if you are lucky – after all, the bank is also hurting in the same financial crisis and money and liquidity become scarce! This means that when your New Zealand bonds come all the way back, you are still way down on your position and it's likely the end of your fund (as people will redeem).

We also see this sort of loss profile with deep out-of-the-money put selling strategies and short volatility strategies in recent times.

Velocity

A third tactic employed by fund managers to deliver outsized returns involves the velocity of trading. The idea here is the capture small spreads but with a high degree of portfolio turnover – e.g. once a month or even more in some cases. The extreme example is high frequency trading of course.

One of my childhood friends, Andy, happens to be one of the best municipal bond traders in America. He is rarely able to buy anything more than 1% or so away from fair value and so the spreads to be made are small. However, Andy turns over his holdings frequently and therefore amplifies his small profits on individual trades into much larger gains on an annualized basis.

Illiquid

This brings us to the fourth and last method, which is trading or investing in illiquid (or less liquid) securities. This is a world where a lot could be mispriced by a lot!

The illiquid world immediately brings up thoughts of private equity and private debt and those are certainly two common categories. However, there are a bunch of dark little corners of the market as well as securities that are rare and perhaps poorly understood – these areas are marked by a significantly higher degree of inefficiency and therefore a lot more opportunity to generate outsized profits for those that do their homework.

If a student came to me and said that Apple (NASDAQ: AAPL) was underpriced by 30-50% based on all known information and the collective wisdom of the millions of investors in the stock, I would very skeptical to say the least. Moreover, it is simply unrealistic to expect a fund manager trading in mega caps to outperform the market over time by any significant margin (anything above very low single digits net of fees).

However, years ago, one of my students at Harvard asserted that Bulgarian farmland was significantly underpriced versus say French farmland. The theory was that Bulgarian farmland was of very similar quality to French farmland, within similar proximity to major European markets and yet trading very cheap despite being on the verge of inclusion in the European Union (where presumably the price gap would narrow). A few years ago, another student of Bulgarian descent told me that the previous student's theory was certainly profitable in the end but that buying apartment buildings in downtown Sofia would have delivered an even higher return!

The lesson here is that trading in illiquid or misunderstood securities as well as dark little corners of the market such as frontier markets can be a very profitable strategy for those that are investing in those areas (assuming that they are good investors of course!).

Last but not least – size does matter!

Investors tend to think of alpha in percentage terms but what if we thought about it in dollar terms? Suppose a manager running $100MM was able to generate $10MM of alpha a year. That would certainly be impressive as it is 10% alpha. For simplicity, let us assume a 1% management fee (although that sounds very low for such high alpha fund!). This means that net of fees, the manager still delivered around 9% alpha.

Given the robust results, the manager will most likely attract more investors and more capital into the fund. The fund might grow to $200MM and let us assume at that point that the manager still only has about $10MM a year of good ideas – still a very strong level of alpha at 5% per annum. Net of fees, 4% alpha was delivered to investors and this of course means more capital will rush into the fund.

At $500MM, if the manager can still only come up with $10MM of alpha, it means that alpha is down to 2% per annum gross of fees and 1% net. Eventually, the fund hits $1 billion and alpha and management are both 1% and so there is no more net alpha.

In the real world, alpha is thought of in percentage terms. Moreover, a fixed level of dollar alpha may not be realistic. If a manager can deliver $10MM of alpha on $100MM, they most likely could do more than $10MM on $200MM – after all, they will have other ideas, hire more analysts to source picks, etc.

Reality is somewhere in the middle. In other words, a smaller manager consistently generating alpha of 5% will not be able to continue that level of outperformance if assets under management climb continuously. As long as the manager optimally manages capacity, there may be some persistent alpha.

However, when you look at the academic research, it is mixed – while most studies find that small funds do a bit better than larger funds, there are other studies that claim smaller funds do about the same as larger funds. This is puzzling as it should be intuitive that smaller funds are more nimble and can therefore take advantage of smaller opportunities, have less price impact on the securities they trade and have an easier time getting in and out of positions. Lastly, one great idea can have a significant impact on a smaller portfolio.

Alignvest Partner Kerry Stirton and I decided to do something to solve this riddle. A number of years ago, we went out and interviewed hundreds of hedge fund managers. A very familiar pattern developed with the mega hedge funds – they were almost all run by impressive individuals with great pedigree, people, resources, knowledge, expertise and talent.

With the small funds, half of the mangers were just as impressive as the larger ones across the board (after all – most last managers start off small). The other half of the small managers had no discernable source of edge whatsoever and that's when it hit us: the group that is all geniuses, can't outperform the group of half geniuses, half posers because of these inherent advantages associated with smaller funds.