Efficiency Deficiency

A hard look at how asset allocation is actually being practiced

"If we ignore errors of our past, we are bound to repeat them in the future."

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Over the last twenty years, I've observed a litany of supposed enhancements, improvements, refinements and innovations in how asset allocation is practiced. Some of these have come and gone, some of these have become accepted staples of the practice of asset allocation, and a few have even cycled in and out of popularity (gold and real estate as asset classes come to mind.)

Many of the "enhancements" have been the creation of "new" asset classes. Twenty plus years ago we practiced asset allocation with stocks, bonds and cash...just as the then yet to be published Brinson Beebower and Hood studies later demonstrated to be the primary explanation of variance of portfolio returns.

Some of the "enhancements" came in the form of how we tie the selection of portfolio allocations for clients to the creation of new measures of risk beyond the classic standard deviation, with additional factors like downside risk and semi-variance, among others.

Still other "improvements" came in the form of how risks, returns and correlations are built, with various "forward looking" models and methods to create "fuzzy" efficient frontier areas instead of lines. More recently, new means of manufacturing indices using something other than market cap have resulted in an "improvement" called "fundamental indices."

Why have some of these "expert enhancements" come and gone? Why have some survived and will they continue to survive? Why have some cycled in and out of popularity over the years? How can you be confident the methods you are using are actually delivering the value you implicitly (or even explicitly) convey to your clients? Can you learn from past dead ends? Is what you are doing now at risk of being a future dead end? Are you being truly objective in examining the new theories? How do we get suckered into some of these things?

What follows will perhaps be shocking to some of you. I will expose some things in our nature that cause us to chase follies or fool us into over-weighting the relevance of the data presented supporting plausible, but perhaps weakly founded theories. These should not be viewed as attacks on any of these theories, merely an objective examination that should be considered before we instinctively over-weight the credence we give to such theories.

Let us start by considering the primary value we are attempting to deliver with asset allocation, that is, to deliver the portfolio with the highest return for the client's tolerance for investment risk. Isn't this what many advisors show their clients? Isn't it a value often represented as being delivered? Have you ever considered that perhaps:

1) Clients might prefer a portfolio that does not expose them to the level of risk they can tolerate.

2) Clients might wish to take only the investment risk that makes sense for what they are trying to achieve in terms of achieving tangible lifestyle benefits instead of an abstract expected return number.

3) Clients might not be able to discern the difference in risk between portfolios with significant macro allocation variances (relative to the micro level we tend to focus on).

If we accept that it is plausible that clients put a value on us identifying their tolerance for investment risk so we can position them in a portfolio that is likely to experience it, regardless of whether it is needed for what they are trying to achieve with their wealth, should we not also consider that it might also be plausible the three points above might also be of value to clients?

For example, let's think about item #3 above, the difficulty in discerning the difference between significantly different allocations at a macro level. Assume that there is a client who has a high tolerance for risk and we are dealing with very high-level macro allocations. The table below shows us how difficult it would be for a client to perceive a material difference in performance between these materially different allocations over the last 80 years.

	Aggressive Portfolio	**More Conservative**
Allocation:	60% Large/40% Small	55% Large/25% Small
		18% Bonds/2% Cash

Number of years in the last eighty years that performed:

Less than -30%	3 ('30,'31, '37)	2 ('31 & '37)
Less than -1.55%	20	19
Greater than +15%	38	38
Between +15% & -1.55%	22	23

Look at this data and put yourself in the shoes of the client. While we spend hours fiddling with 1-10% allocations to sub-classes of these macro portfolios, the reality is the client can't perceive the difference between the two materially different macro level allocations, let alone the minor sub-classes. Maybe we should rethink what we are trying to accomplish in asset allocation and spend more time on delivering items numbered 1-3 above?

Letting data fool us creates many dead ends in asset allocation. When I served on the investment advisory committee of the Virginia Retirement System, our consultant suggested we consider Japan as a separate asset class. How many of you today have a specific allocation target to Japan? Why don't you? After all, it seems to have a low correlation to domestic and even EAFE stocks. Shouldn't this increase our portfolio efficiency?

Well, like many things where small sets of data permit our minds to be skewed, back in the early 90's when the Japanese bear market was only a recent blip in the data set of the previous thirty years, Japan appeared to have both a diversifying benefit and return premium. In retrospect, of course, an 18-year cumulative return of minus 60% has made it a bit difficult for us to swallow a Japan return premium. But, in the late 80's & early 90's it was the "enlightened" thing to do. If you have been in the business for long enough, you have probably cycled through more than a few of these small data set biases. When I entered the business, gold and precious metals were all the rage. A twenty-year cumulative return (before inflation) of minus 75% pretty much ended that...until recently.

Some of you may not remember the awful real estate bust when Japan stopped buying our office towers and California homes declined by 30% or more and real estate evaporated from our list of asset classes...of course until recently again. We do tend to permit small sets of data to influence our decisions. We tend to overweight the relevance of more recent data...too small to form the type of conclusions we actually in practice should apply.

Can you imagine, for an example, two unnamed asset classes with the following thirty year results...assume it is all the data we have for these two asset classes named "A" and "B".

Class:	"A"	"B"
30 Year Return (Mean):	13.84%	21.09%
30 Year Compound:	12.13%	17.20%
30 Year Risk (SD):	21.69%	39.10%

If this thirty-year sample were all of the data you have for these asset classes, what would you use as an assumption for modeling them? How many asset classes do you currently model with 30 years of data, or less? How likely do you think it is that any one relatively large sample period like thirty years would happen to represent the true average of the distribution? Aren't we are supposed to be modeling the nature of the class, not a random and potentially highly skewed sample? Asset class "A" and "B" happen to be the actual data from the Center for Research of Securities Prices for large and small cap stocks respectively, for the thirty years ending 12/31/1961. If it were 1962, and this was all of the data you had, you would probably feel comfortable with these assumptions, just as many of us are comfortable now with far smaller samples for things like alternative investments.

Of course, for large and small cap stocks, we have a lot more data than this thirty-year period so we can observe that this data set is not the norm but rather an outlier, and we make better assumptions. But, if you didn't have the data to observe this period was an outlier, does it make it any more likely the data you have now will just happen to be the middle of the distribution? Think about how unlikely that is.

The "value premium" has gained a lot of popularity as well. The evidence for it appeared pretty compelling, so I went to the source of Kenneth French's data library. (http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html)

I've been tearing through these reams of data elements for the last several years and while the premise of the theory was counter-intuitive to me, in looking at things like rolling mean returns, it looked reasonably compelling. The data was broken into deciles, quintiles and various other grouping of styles boxes based on book-to-market and market cap, monthly, back to July, 1926.

The theory supposedly demonstrates there is a "value premium" for which one has greater risk that is rewarded for investing in more "distressed companies." Therefore, so the theory goes, one should overweight small and large value stocks. I don't know how patient your clients are, but imagine you represent this value premium as part of the

value you deliver and we start right at the beginning of the dataset knowing clairvoyantly this value premium will appear. How long will a client keep paying you to deliver this value? Would they stick with the long-term plan for five years? Ten years? Fifteen years? Could you get them to stick with it for twenty years? If so, it still wouldn't be enough.

The first twenty years of the data set have small value underperforming small core by 28 bps, with a whopping additional standard deviation of 8.59% a year. Plot that on your efficient frontier!

What about the large value bet I made at the expense of large growth? Large value underperformed large growth over the first 20 years by 39 bps, with a standard deviation that was 16% higher (42% versus 26% for large growth). Risk premium indeed!

Of course, more recently value stocks have outperformed growth. For example, we know that over the 20 years ending in 2004, the best performing segments (I can't bring myself to call them asset classes) of the domestic three by three style box were, first small value and next, large value.

Say I enter the business in 1985 and begin growing my practice by 20 clients a year. Once I get to two hundred clients over the next ten years, my job turns toward servicing instead of prospecting. Now imagine when I entered the business I clairvoyantly knew that over the next twenty years small value and large value would be the two best performing asset classes, so I overweight them.

Knowing this clairvoyantly in advance, I create a special contract with each of my clients. It says that if they sign in blood that they will stick with me for at least ten years; if I don't outperform a market portfolio (no style or cap bets) I'll refund their prior ten years worth of fees. You might not be willing to make such a contract with your clients, but if you knew for certain the two best performing segments were small and large value, you might be willing to because all you would need to do is index and overweight those segments you know for a fact will out perform for the next twenty years.

Of the 200 clients that you acquired in the first ten years of your career, how many refunds did you have to make by the time the ten-year measurement period unfolded for the last set of them at the end of 2004? Turns out 110 of your 200 clients would have received refunds. How could this be? It is based on timing of when you acquired each client...despite small and large value out performing with certainty over the twenty years of this advisor's contracts with clients, the timing of when they outperformed caused most of them to under perform a simple market weighted portfolio. So much for the "value premium."

Whenever you have data that shows superior results, you can guarantee the industry will find a way to capitalize on marketing it. Despite the two twenty year examples cited above where the bet on value didn't work out for the advisor, or the client (well...at least a majority of the clients in the second example got their fees refunded), the value bet data over much of the history looks pretty compelling. In fact, if value and growth were just opposite sides of a market coin, I'd only have a 28% chance of flipping the value growth coin in a way that would produce such results, or about the same chances as flipping two heads in a row.

We know that value has had a pretty good run over recent history. What if I could figure out a way to sell indexing that produced alpha? Wouldn't that be the best of both worlds? Ignore the contradiction in terms for a moment. Wouldn't it sell if you could assemble a compelling enough spin on it?

Enter "fundamental indexing." The theory here is the available market cap weightings of the opportunity set is an "inferior" way to create an index. So, what would be a better way of creating an index? Weights based on earnings growth? No...that wouldn't work because growth has underperformed value in the available data set that conveniently excludes the first twenty years where the data for value isn't as compelling. But, marketers can spin this. Dividends...that should be a fundamental weighting (sound like a value tilt to you? Why dividends instead of earnings growth? Oh...earnings growth would tilt the wrong way from the kind of stocks I retrospectively know outperformed.) What else could I throw in? Price to book...sure...that will give me more value stocks. Number of employees? That will tilt me towards value too. So I mix and match the arguments for these criteria for creating an index that will clearly tilt the portfolio toward value stocks, and shazam! Indexed Alpha (in retrospect).

There are many more examples of how we find ourselves accepting marketing spin and the data presented without researching some of the contradictions or counter examples. But our clients expect more than that from us. Plausible sales pitches are not the same as confidence when we are talking about helping our clients make the most of the only life they have. We need to think critically on these things. That isn't to say that a critical examination might confirm such theories are all indeed plausible, but should we bet our career on representing such things as our value to clients? More importantly, should we be betting our clients' lives on them when we have the choice to avoid such risks?