Efficient Frontier

An Online Journal of Practical Asset Allocation

Edited by William J. Bernstein

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What's Wrong with this Picture?

(From *Value Line*, 10/17/97)

The Median of Estimated
PRICE-EARNINGS RATIOS
of all stocks with earnings

18.4

26 Weeks Ago*	Market Low 12-23-74*	Market High 9-4-87*
15.6	4.8	16.9

The Median of
ESTIMATED YIELDS
(next 12 months) of all dividend paying stocks under review

1.7%

26 Weeks Ago*	Market Low 12-23-74*	Market High 9-4-87*
2.1%	7.8%	2.3%

The Estimated Median
APPRECIATION POTENTIAL
of all 1700 stocks in the hypothesized economic environment 3 to 5 years hence

30%

26 Weeks Ago*	Market Low 12-23-74*	Market High 9-4-87*
55%	234%	40%

*Estimated medians as published in *The Value Line Investment Survey* on the dates shown.

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Efficient Frontier

William J. Bernstein

Do Fund Managers Exhibit Skill?

Of Money Managers, Major Leaguers, Heavy Hitters and Random Walkers

OK, you've done your homework. You've scoured the Morningstar database, culled out the funds with the best performance over the past several years, read the prospectuses, and more importantly, the annual reports, and figured just how the funds chosen fit your target asset allocation. Is it really worth the effort?

There is a large body of academic finance literature concerning mutual fund persistence, i.e., just what does past performance tell you about future performance? The short answer is "not much." Burton Malkiel, who has extensively researched the problem, concludes in *A Random Walk Down Wall Street* that yes, the funds with the best past returns will outperform their peers by a slight amount, but will not beat an index fund. Unfortunately, the analytic techniques used are abstruse, highly complex, and unverifiable by the average investor.

I decided to investigate the problem myself. Morningstar *Principia* is a commercially available Windows based product aimed at allowing individual investors to sort, search, and rank mutual funds. It is also capable of exporting customized outputs to a spreadsheet; this capability enables even the smallest investors to perform very sophisticated analyses.

I settled on the following technique, the short version of which is: Screen for Aggressive Growth, Growth, Growth&Income, Equity-Income, Small Co., and International Funds with a >10.5 year track record, i.e., inception before 1/87. I know, I know, these are silly categorizations, but they're the best we have going back that far.

For each year, we calculate how much the fund return varies from the objective average. This is why I used the archaic classification system. For each fund, we now have 11 relative returns. (I included the first half of 1997 as a whole year.) A return of +2.0 means that the fund exceeded the objective average by 2%, and -4.5 means that in that year it fell below the objective average by 4.5%.

Using this data we can test "the null hypothesis" that the average return difference = 0.

We calculate the average relative return value and relative return SD of each fund, from which we can calculate a Z value as sqrt(11)*(avg/SD). Using a one tailed t test with10 degrees of freedom we can now calculate a p value. (For the purists among you, I used a population SD instead of the sample SD. This produces slightly lower p values, and thus militates slightly in favor of the funds.)

The "p value" is simply the probability that the result may have occurred by chance. A p value of 1 indicates that the result occurred almost certainly by chance, whereas a p of 0.05 means that there was only a 5% probability of chance occurence.

I ran this procedure by Paul Pudaite, chief statistician at Morningstar. Mr. Pudaite pointed out to me that we're still not done. Since we're looking only at the best funds after the fact, we have to guard against "data mining." We do this by calculating the adjusted p value as (1-(1-p)^n). (where p is the unadjusted p value, and n the number of funds) Whew!

Anyway, here are the results:

Aggressive Growth: 34 funds. The best was AIM Constellation I, with an unadjusted p of 0.007 but an adjusted p of 0.21. In other words, there is only a 21% probability that the good result occurred by chance. Not enough to satisfy a statistician, but good enough for me. Kaufmann? Unadjusted p of 0.13, adjusted p of 0.99! In other words, there was a 99% probability that the good result was due to chance, and only a 1% probability of it being due to skill. Yes, the "Tough Guys" exceeded the average fund by 6.6% annually, but the SD of its relative return was 18.5%. In other words, the fund was so volatile that its excellent performance was most likely the result of random motion.

Growth: 200 funds. The best was AIM Value A, unadjusted p of 0.0009, adjusted p 0.17. Next best, Fidelity Destiny II, unadjusted p 0.003, adjusted p 0.47.

G&I: 118 funds. The best was Fido G&I, unadjusted p 0.003, adjusted 0.31. That's it -- out of 118 funds only one with a better than 50% chance of "skill" with >10.5 years of track record.

Equity Income: 20 funds. The best was United Income A, unadjusted p of 0.003, adjusted 0.075. Again, the only fund with adjusted p < 0.5.

Small Co: 57 funds. Best FPA Capital, unadjusted p of 0.01, adjusted of 0.42. Again, only 1 fund with adjusted p<0.5.

International: 27 funds. The best was EuroPacific Growth, unadjusted p of 0.001, adjusted to 0.035--the only one in the whole study which a statistician would accept as showing genuine outperformance. Also, Ivy International, unadjusted p of 0.009, adjusted to 0.22, and TRP International, unadjusted p of 0.02, adjusted to 0.42. My favorite, Harbor International, wasn't included in the analysis because of it's later inception, but for the 10 return periods beginning 1/88 it has an unadjusted p of 0.00035 and an adjusted p of about 0.01. Not too shabby.

What is really striking is that the evidence of underperformance is much more solid -- 7 funds with an adjusted p of <0.05 for underperformance, versus only 1 for superior performance. For those of you who would like to view the output, it is available in .htm format here, and in .xls format here.

This method is fairly insensitive, and not particularly good at picking out individual funds. It tends to favor conventional funds with low benchmark tracking error, which produces low relative SDs, and thus high z values and low p values. It penalizes unconventional funds, which have high tracking errors, and thus low z values and high p values. For example, Scudder International is not a particularly distinguished fund, but over the past 10.5 years has outperformed its peers by about 2%. Because it is very "conventional" it tracks its peers closely, with a relative SD of only about 2%, which produces a fairly respectable z value of about 1. On the other hand, Mutual Qualified outperforms it's peers by a similar amount, but has a much larger tracking error -- about 8.5%, so it has a much lower z value (0.23). In fact, by any conventional measure of risk, Mutual Qualified is a much less risky fund than Scudder International, and has much better risk adjusted performance.

Nonetheless, data is highly consistent with the academic data; most exceptional fund performance is due to chance, and not skill. To give you an idea of what the statistics of real skill looks like, let's consider major league batters. I examined 11 annual batting averages from some famous, and not so famous, major league stars from the middle of their careers -- avoiding their rookie as well as their declining years. I assume a non-pitcher's mean batting average of .270, which has been remarkably constant over the decades.

Let's look at arguably the greatest batter of modern times: Ted Williams. I picked the 11 years from the middle of his career, from 1946 to 1958. (I left out 1952-3, when he was flying Navy jets for most of the season.) His unadjusted p value was an astonishing .0000001. Data mining? You bet. But correct it for, say, 1000 major league players and the value is still .0001. If we data mine as egregiously for mutual fund managers we come up with the likes of Peter Lynch. If we look at the data for his heyday of 1976-86 (11 years) we find that his annual besting of the growth fund average by an astonishing 15.9% per year results in an unadjusted p of 0.00001. We've mined his fund from approximately 300 diversified domestic stock funds extant during the period, yielding an adjusted p of 0.0034. Very impressive, but still a few orders of magnitude less impressive than Mr. Williams.

Let's take a slight step down to Stan Musial. For 1948-58 his p values are .00000004 unadjusted and .00004 adjusted. His data actually looks slightly better than Williams' because his averages were much more consistent over the years.

Let's take yet another step down. I'm of a certain age, and from Philadelphia, so Richie Ashburn sticks out in my memory. However, I doubt that even the most fanatical baseball buffs under 30 know who he is. From 1950 to 1960 his p values are .0002 unadjusted and 0.17 adjusted.

Let's eliminate the data mining problem entirely with the following construct: Take all of the NL batting champs for 1959-79, and look at the batting averages for those who played 11 more seasons after that. Six players qualify:

Our first example is Hank Aaron, who won the title in 1959. At the end of that season, we say, "Hmm, he just might have skill. Let's see how he does for the next 11 years." We cannot now be accused of data mining. From 1960-1970 his p value is .00001.

Roberto Clemente won the batting title in 1961, and for 1962-72 his p is .0000008.

Tommy Davis won the title in 1962, and even though his 1963-73 average was only .290 that still produces a p of .01.

Pete Rose won the title in 1968, and for 1969-79 his p was .000002.

The last 2 players who fit the criteria were the less memorable Bill Madlock and Dave Parker, but even they managed 0.01 and 0.044, respectively.

So What's the Point of all This?

As one of our readers wrote of another piece, "All the math made my head hurt." Sorry about that -- I suspect that this piece falls into the same category. So here's the Cliff's Notes version: Out of over 400 diversified funds studied during the 1987-97 period, by definition half showed above average performance, but in almost all cases it seemed likely that this was due to random variation, and not skill. In only one case was there unequivocal statistical evidence of skill. When the same tests were applied to major league batters, abundant evidence of skill was found.

By way of comparison, consider the best performing mutual fund for any given year. Such funds tend to do somewhat better than average the next year, but no better than average in following years. In contrast, in every case the National League batting champions demonstrated strong statistical evidence of skill in the 11 year period following their batting crowns. Put another way, batting performance persists, mutual fund performance does not.

Successful money managers occasionally are tagged as "heavy hitters." The above analysis suggests that they are much more likely random walkers. Is the selection of active money managers worth the effort? I doubt it.

Don't blame me, I'm only the messenger.

Efficient Frontier

William J. Bernstein

Mean Reversion and You

Does Tactical Allocation Based On Prior Returns Pay Off?

There is little question that the financial markets can be profoundly irrational. Over long time periods, asset valuations and returns can gyrate wildly. At times stocks may become absurdly overvalued, and at other times they cannot be given away. It would seem axiomatic that the alert and disciplined investor could take advantage of this phenomenon with some ease.

Consider, for example, the behavior of large US stocks over the past several decades. There is fairly good data on the price to book ratio, dividend yield, and price to earnings ratio of the Dow Jones Average going back 80 years. Until recently, anyway, the P/B ratio of the Dow has correlated fairly well with the 5 year future return of large US stocks. For almost all of the 1926-94 period the DJIA has been priced at between 1 and 3 times book value. Had one simply bought and held large stocks (proxied as the S&P) for the 1926-94 period one would have garnered a return of 10.19% with a standard deviation of 20.30%.

Now, consider a strategy where equity exposure is allotted according to P/B -- e.g., 100% stock at a PB of 1, 50% at 2, 25% at 2.5, and 0% at 3. This determination is made at the beginning of each year; whatever not allotted to stocks is invested in 5 year treasuries. This strategy yields an annualized return of 10.96% with an SD of 15.34% -- a slightly higher return with much less risk.

It can be argued that we are cheating by employing an allocation rule using P/Bs of 1 and 3 as our border values; in 1926 we had no way of knowing that these would be the "correct" values. Fine -- let's say we guessed wrong and used P/Bs of 0.5 and 6 as our border values (100% stock at 0.5, zero stock at 6.0). As this is being written the P/B of the DJIA/S&P is actually approaching this upper limit. This rule yields a return of 10.39% and an SD of 16.36% -- still better than buy and hold 100% stock. The point is this; any strategy that slightly increases the portfolio exposure of an asset as it gets cheaper, and slightly decreases it as it gets more expensive seems quite likely to both increase return while reducing risk.

One ought to be able to expand this sort of strategy to international investing. Unfortunately, it is difficult, if not impossible, to compare P/B, P/E, and dividend yields across borders.

Some other kind of approach is needed. There is ample evidence that a wide range of assets mean revert -- i.e., a period of outperformance by any one asset is likely to be followed by a period of underperformance.

Anthony Richards, from the IMF, in an article to be published in December's ***Journal of Finance***, looked at the 16 MSCI countries from 1970 to 1995, and examined the performance of portfolios made up of the 4 "winners" and "losers" over varying time periods. Here are the results over different horizons, relative to the average of all of the countries studied:

	Winner Portfolio (annualized relative return)	Loser Portfolio (annualized relative return)
3 months	+6.4%	+3.3%
6 months	+1.5%	-1.9%
12 months	-0.4%	-2.7%
24 months	-4.0%	+1.2%
36 months	-3.2%	+3.2%
48 months	-3.0%	+2.7%
60 months	-1.6%	+1.8%

(The time periods are both forward and backward looking. I.e., "12 months" refers to the forward 12 month performance of the winners/losers over the previous 12 months.)

As you can see, there is a strong tendency for past winners to produce above average short term performance, and below average longer term performance. The opposite is true of past losers. (Be careful with the 3 month data -- remember that this is annualized.)

For those of you who want to look at the piece yourselves, it can be found at http://www.cob.ohio-state.edu/~fin/journal/archive_papers/issdec97/ms5314.pdf

Unfortunately, switching back and forth between different national index portfolios is not practical for small investors. As an alternative, I've calculated the "autocorrelations" for 5 year returns for a range of global regional assets. An "autocorrelation" is simply the correlation of a time series with itself, lagged by one period. So, an autocorrelation of +1 means that an above average result in one period always predicts the same above average result in the next period. (This is mathematically impossible.) An autocorrelation of -1 predicts the opposite result, and a value of zero is seen when the return for a given period has no predictive value. Put another way, a high positive autocorrelation favors momentum strategies, a high negative correlation favors contrarian strategies, and a zero autocorrelation defines a "random walk," in which a fixed, mechanically rebalanced policy is most effective.

Here are the autocorrelations for 5 year periods for some regional global assets:

Asset	1970-94	1972-96
S&P 500	-0.12	+.14
US small stocks	-0.39	-0.45
Japanese stocks	-0.50	-0.12
Pacific Rim stocks	-0.73	-0.62
European stocks	-0.45	-0.39
UK stocks	-0.66	-0.26
Precious Metals stocks	+0.44	+0.07

I've used the two overlapping periods to judge the reproducibility of the data. Note that for both periods US small stocks and all foreign stock groups fairly strongly mean revert. Large US stocks exhibit a random walk, and precious metals stocks may actually have nonregressing behavior -- at least over 5 years.

Over shorter periods of time, this negative autocorrelation tends to disappear, and at very short periods, tends to be positive. For example, the average autocorrelation for the above assets over 1 month periods is about +0.1. Extensive studies have tended to confirm the short term positive and long term negative autocorrelation of US equity prices. Lakonishok et. al. have recently demonstrated positive excess returns generated by short term momentum strategies, and discuss why this is not necessarily inconsistent with longer term contrarian strategies.

The overwhelming acceptance of the "random walk" behavior of stock prices is seen to stem from the fact that most of the data is derived from large US securities. Look at almost any other equity class, however, and there is fairly strong evidence for mean reversion over long time periods.

I've looked at the data yet another way. Let's use the database of global equity assets for the 1970-94 period (the above, except that the EAFE-E is used to combine UK and European stocks.) The "relative 5 year return" for each asset is calculated as the difference between for the return for the asset and for the average of all 6. This "relative return" is then compared with the "relative" return for the same asset for the next 5 years. It turns out that if you aggregate all of the nonoverlapping 5 year autocorrelations (24 in all, there are 4 data points for each of 6 assets) the average autocorrelations is -0.44. In other words, if Japanese stocks have underperformed global equities for the past 5 years, the odds are that they will outperform global equities in the next 5 years.

In summary, then, the performance of a wide group of global assets seems to mean revert, with a mean 5 year autocorrelation of about -0.4 to -0.5, either in absolute or relative terms.

Can the individual investor make this pay?

For some time I've experimented with models which examine different allocation strategies for overweighting/underweighting global asset categories which underperform/outperform. The results have been disappointing. Those which work best involve "all or none" paradigms, similar to the technique used by Richards. . Consider the 1970-96 allocation model discussed in last month's issue, consisting of 6 assets:

- S&P500
- US Small
- EAFE-E
- EAFE-PXJ
- EAFE-Japan
- Gold Stocks

The 5 year returns are calculated every 5 years, and the best performing asset is dropped for the next 5 years, resulting in an equally weighted portfolio of the remaining 5 assets. Thus, this portfolio covers the 22 year period 1975-96, with portfolio revisions being made in 1975, 1980, 1985, 1990, and 1995. This portfolio returned 19.20% annually, with an SD of 17.44%. The "coward's portfolio" consisting of equal amounts of all 6 assets yielded a return of 17.57% with an SD of 15.68%. The Sharpe ratios, assuming a 7.11% t bill yield for the period, are 0.697 for the dynamically allocated portfolio, versus 0.667 for the coward's statically allocated portfolio.

What if you rebalance every year based on the previous 5 year's performance, dropping the winner each year ? The results are worse -- a 22 year return/SD of 17.64/18.44, with a Sharpe of 0.559.

What to make of all this? Although I've identified a few methods of increasing risk adjusted yield slightly, I've also identified many more which have failed. I would bet that the successful techniques (using P/B and dividend yield with the S&P, eliminating the best global performer every 5 years) were more the result of data snooping than of a genuine efficacy.

The lesson? Don't get too cute with your allocations. Keep them fairly constant over the long haul, and don't count on reversion to the mean to increase your returns by very much. Even Richard's method, which uses the white-knuckle strategy of picking the 4 nations with the worst preceding 3 year returns, produces an excess return of only 3.2%. It's a good bet that a fair chunk of this advantage was extracted from the old data mine.

If you must change your allocations, do so very slowly and infrequently, by very little, and always in a contrarian manner.

Even the Best Don't Get it Quite Right

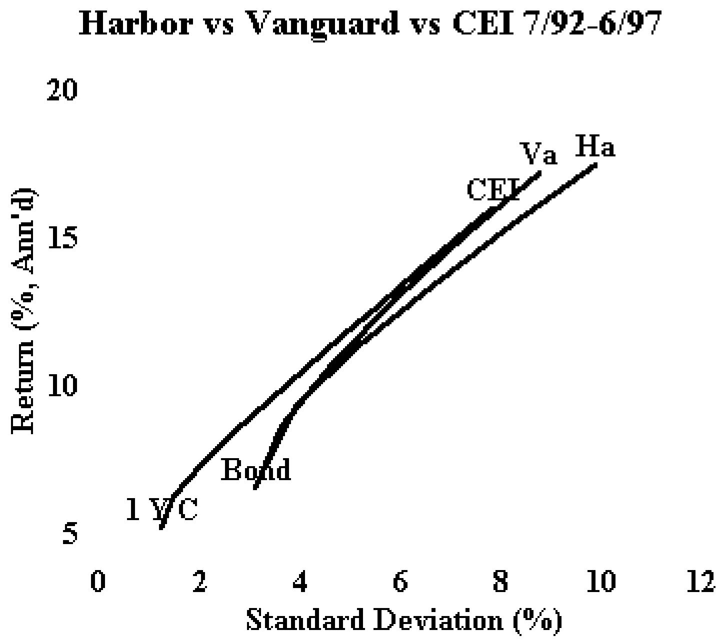
A Superb Portfolio Needs More Than Just Superb Funds

I genuinely like the Harbor Funds, and admire its President, Ronald Boller. They may not be the best company in mutual fundom, but you can sure see it from there. Consider that their two flagship funds are Harbor International and Harbor Capital Appreciation. Managed since inception by Hakan Castegren, International has beaten its peers by an astonishing annualized 9% since 1988, and the EAFE by even more. In fact, it is one of the few funds whose performance actually shows statistical evidence of skill, rather than random variation. International closed to new investors in 1993, and even Castegren's sales loaded Ivy International closed this year. Their newer International Growth and International II Funds perform almost as well.

Harbor Capital Appreication has bested the S&P by 2% annually over the same period, and leaves its peers in the dust.They also have two excellent bond funds, and a domestic value fund which keeps up fairly well with its peers. Only Harbor Growth lags the averages by a few percent.

Boller is no slouch, either. Well versed in portfolio theory and justifiably proud of his brood, he puts out a quarterly report featuring the returns and SDs of a model portfolio of his funds, graded from 100% stock to 100% bond, and compares it to a passively managed portfolio of Vanguard Index Funds. The stock portion of each portfolio is 70% domestic and 30% foreign, and the bond end is a short/intermediate duration mix. Obviously Mr. Boller expects his model portfolios to be more efficient than the index portfolios.

But wait, something's definitely wrong here in mutual fund paradise. I've plotted the 5 year returns and SDs for the Harbor Funds ("Ha" on the graph), the Vanguard Index Funds ("Va" on the graph) and the CEI for the 5 year period 7/92-6/97.



With such superb fund returns, you'd expect a portfolio made up of the Harbor Funds to be a world beater. Wrong. In fact, the Harbor Fund portfolios get edged out by both the Vanguard Index portfolio and the CEI. For those of you new to the site, the CEI is a non cap weighted global equity index devised on these pages, constituted as follows: (The 5 year returns for 7/92-6/97 are listed in parentheses after each asset.)

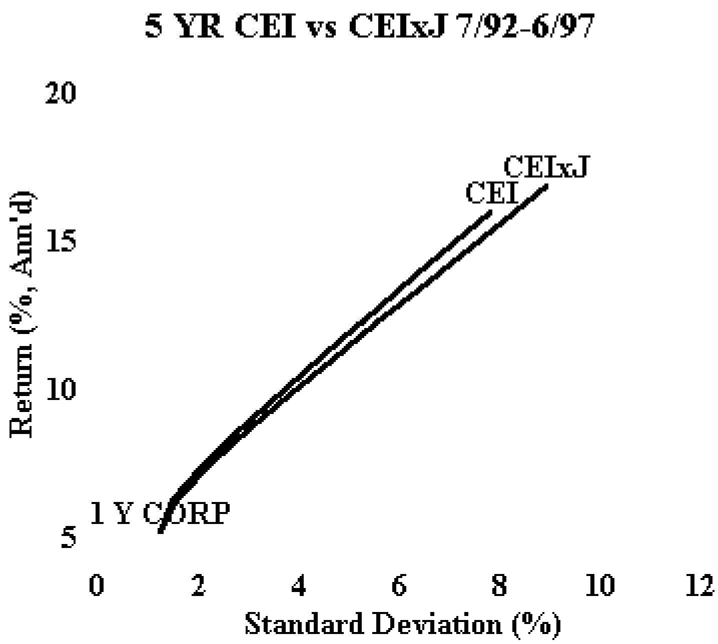
Coward's Equity Index (CEI)

- 20% S&P 500 (19.76%)
- 20% US small stocks (DFA US 9-10 Portfolio) (20.85%)
- 15% EAFE-Europe (14.87%)
- 5%EAFE Pac. Ex Japan (14.42%)
- 5% Japan Large (MSCI Japan) (9.13%)
- 10% Continental Small (DFA Cont. Sm. Co. Portfolio) (7.15%)
- 5%UK small (DFA UK Sm. Co. Portfolio (9.24%)
- 5% Japan Small (DFA Jap. Sm. Co. Portfolio) (0.73%)
- 5% Pac. EX Japan small (DFA Pac. Rim Sm. Co. Port., before 1/93 EAFE Pac. X J) (15.47%)
- 10% Latin American (MSCI Lat. Am.) (15.17%)

So if Harbor has such great funds, how come its fund portfolios don't beat the benchmarks? Well, I'm not completely sure, but I've got a hunch or two. Firstly, I suspect that the Harbor equity portfolio isn't diversified enough. For example, they've largely avoided Japanese equity until recently in their foreign funds. *Wait a minute, I hear you say -- avoiding the Nikkei for the past several years wasn't a mistake -- it was a stroke of genius!* In fact, what first drew me to the Harbor International Fund in 1988 was its low Japanese exposure -- valuations on the

Tokyo market scared the bejabbers out of me. They still do. Surely avoiding Japanese equity would have made any global portfolio more efficient. But in the looking glass world of portfolio theory, things are not always what they seem.

A small portfolio experiment is in order. Consider the composition of the CEI. As you can see above, it is 10% Japanese, split between large and small cap components. The Japanese small cap component by far underperformed every other major sector of the major global equity markets over the past 5 years, and the large cap component was the third worst. Surely eliminating them would have improved portfolio performance. Well, yes and no. In order to investigate portfolio behavior with and sans a Japanese component, the return/SD curve for the CEI with and without Japanese equity was calculated. As always, the CEI is combined with 1 year corporate bonds to produce a spectrum of portfolios according to risk, as was done above.



As can be seen above, removing Japanese equity does enhance return, but it also increases risk. In fact, it can be clearly seen from the plot that *even with its miserable returns and very high risk over the past 5 years, a 10% Japanese equity component benefitted risk adjusted portfolio behavior.* Now it becomes clearer what the folks at Harbor did wrong. If avoiding doggy Japanese equity was a mistake, then it was a much bigger mistake avoiding domestic small cap stocks , which actually slightly outperformed the highflying S&P over the past 5 years.(Harbor has no small cap fund.) And as long as we're at it, the average 1% expense of their funds, while more than reasonable by industry standards, can't stand up to Vanguard's 0.26% average expense. Lastly, it can be seen from the first graph that the Harbor and Vanguard Bond portfolios, which have a duration of about 2.2 years, engender higher risk than the 1 year corporate strategy used with the CEI, with an adverse effect on portfolio efficiency. This will be discussed in the following article.

I've brought up this issue with Mr. Boller. His response cuts to the heart of the matter:

> This is all active management has ever done, in reality. People brag about doing better but usually do not provide a proof statement. We are very proud to be able to return more value to shareholders that they pay us in fees. *Remember, the value added over indexes is a zero sum game before fees are subtracted.* (Italics mine)

Concise, honest, and not terribly charitable to the industry of which he is one of the best specimens. Remember, this guy is no John Bogle type indexing imam -- his rice bowl is the active management of securities.

One further point needs to be made: The fact that I chose to discuss the Harbor Funds in the first place is the result of their superb long term performance; it's data mining of the the worst sort. The message to the small investor here is clear -- *the odds that you will be able to prospectively pick a group of actively managed mutual funds which prove more efficient than an indexed global strategy in the long run are slim to none.* If Harbor could't get it quite right over the past 5 years, how likely is it that your favorite funds will do so in the next 5?

Well, I think I feel better now about having owned Japanese equity these past few years. I'll probaly still invest with Harbor, too. Now, if I can just get rid of those ole' S&P tracking error blues . . . .

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Efficient Frontier

William J. Bernstein

Roll Your Own

Finally, an Inexpensive, Easy to Use Windows-based MVO

EF was originally inspired by the lack of portfolio tools available to small investors. In the July issue we discussed a mean variance optimizer (MVO) available from Wagner Associates for $99. This MVO dealt with two of the major problems of previous optimizers: cost and ease of data entry. Unfortunately, this application requires Excel 7, which only a minority of readers posess.

EF is pleased to announce that this last hurdle has finally been surmounted. David Wilkinson, a Connecticuit physicist, has written a freestanding Windows based application named ***VisualMvo***. The program allows fast and easy data entry, modification, and portfolio file saving operations. The efficient frontier of portfolio risk/return behavior is rapidly displayed, and by scrolling through the frontier the portfolio compositions at each point on it are displayed in real time.

VisualMvo is currently being offered for beta testing to serious evaluators. The software will be provided free of charge, but evaluators will be asked to spend time putting the program through its paces, and to report back to the programmer. If you are interested contact David Wilkinson via email. He may also be reached via snailmail or telephone at:

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Efficient Frontier

William J. Bernstein

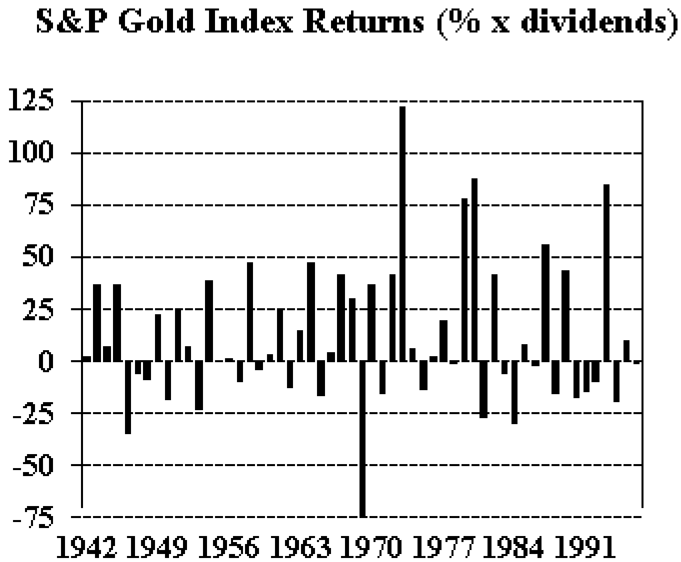
What is the Expected Return of Precious Metals Equity? Part II

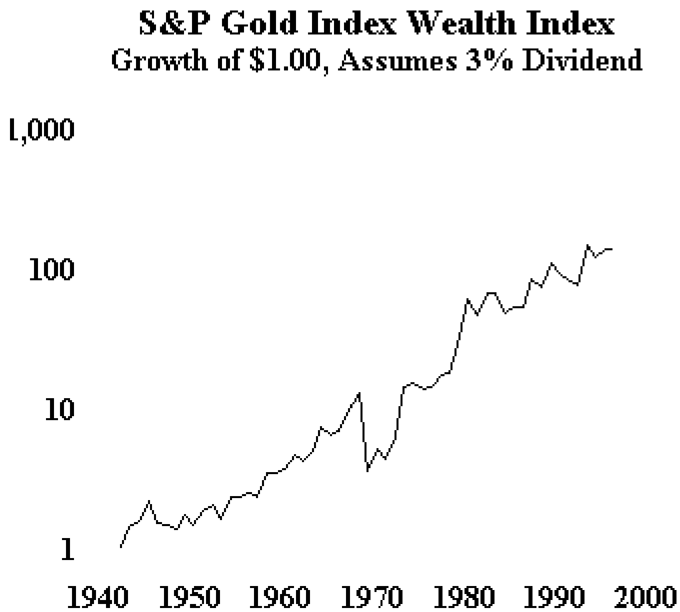
In a previous issue of ***EF*** we attempted to cobble together an index of precious metals equity from the Morningstar and VanEck data, and came up with an annualized return of 12.81% from 1/69 to 9/96.

While we thought this was a useful estimate, we were concerned that it seemed "too high," and were worried that it encompassed only 27.75 years.

After a great deal searching, it turns out that the obscure S&P Gold Mining Index actually goes back to 1942. Courtesy of Brian Taylor at Global Financial Data we now have this data. The price we pay for this much more reliable index is that it does not include dividends. For the 55 year 1942-96 period the principal only return was 6.45%. If one assumes an average dividend of 3%, then the long term return over the period was 9.45%. This compares with 16.53% for small stocks, 13.96% for the S&P 500, 5.20% for the 20 year treasury, and 4.47% for t-bills over the same period.

The correlations with the other assets in the Ibbotson data base are as follows: US Small Stocks 0.23, S&P 0.24, Long Corporates 0.02, 20 year treasuries 0.00, 5 year treasuries 0.07, and T-Bills 0.09. I've plotted the annual returns and wealth index for the period below:





As you can see, this asset is not for the faint of heart -- in 1969 the index lost 75% of its value. Nonetheless, optimizations of the 1942-96 historical data for the 6 Ibbotson assets and the S&P Gold Index (assuming the 9.45% dividend inclusive return) yields an optimal precious metals equity allocation of about 10% over the mid range of risk (10%-15% of SD).

In 1942 the price of gold buillion itself was fixed at $35, and at year end 1996 was $340, for a 55 year return of 4.22%. It should not surprise that the return of gold stocks was higher. Over the long haul you do much better investing in United Fruit than in a load of banannas.

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William J. Bernstein

What's the Proper Bond Duration for Your Portfolio?

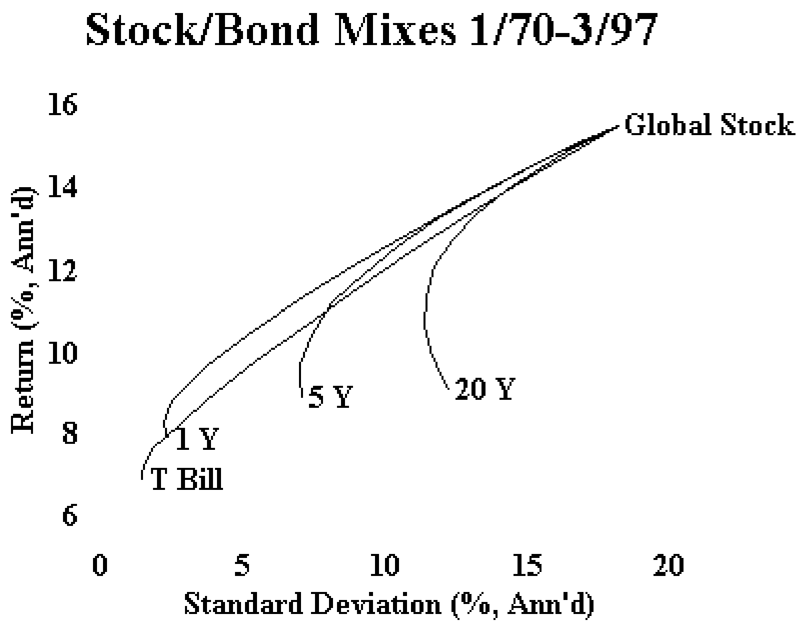
A Dull but Important Question

Ever stare at bond yield curve and wonder what maturities to buy? Or even worse, listen to some bozo analyst explaining to Uncle Lou with a grave but sagacious expression that "in the current interest rate environment our viewers would be well advised to purchase intermediate bonds of high quality."

Well, folks, you can't get there from here. Attempting to evaluate the risk/return characteristics of a single asset isolated from the portfolio it will be harmonizing in concert with is wasted effort. One cannot simply taste the butter to determine its effect on the finished cake.

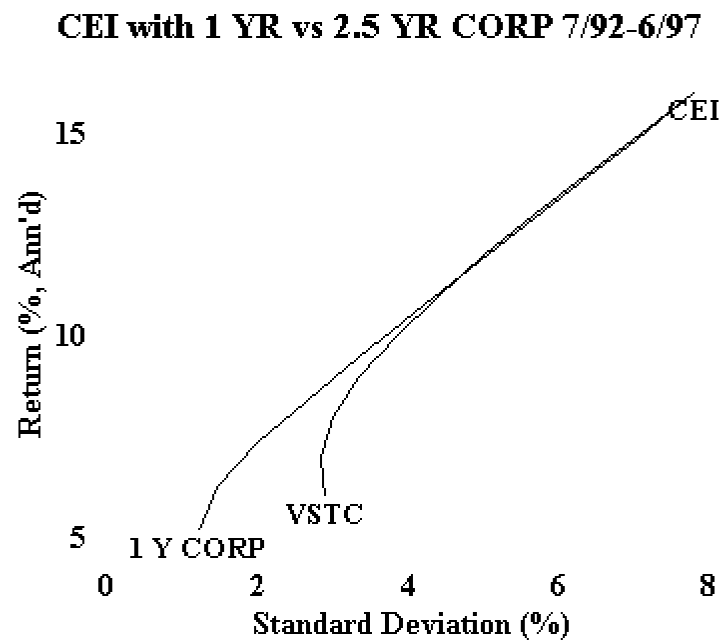
I'll make this one short and sweet. Bonds are the underwear in your portfolio -- unexciting and not much thought about, but select the wrong pair and you'll be surprised at just how uncomfortable you are.

Let's start with data supplied by Ibbotson and DFA. We've constructed a global stock portfolio for the 1/70-3/97 period consisting of one quarter each large and small foreign and domestic equity. Next, we mixed it with 30 day, 1 year, 5 year, and 20 year treasuries. The return/risk curves for each bond duration is then plotted.



As you can see, unless you are at the very high end of risk tolerance, the long bond is a terrible idea. It's not an investment -- it's a wager on interest rates. Likewise, unless you are at the very low end of risk tolerance t-bills are a bad idea too. One and 5 year treasuries seem to work the best over the vast middle range of stock/bond mixes that make up most of our portfolios.

The above plot seems to show that the optimal maturity for the long haul is somewhere in the 1 to 3 year area. In order to pin this down a bit further, I compared the CEI mixed with either the DFA 1 year fixed income fund and the Vanguard Short Term Fixed Income Fund for the past 5 years. These 2 funds are virtually identical in makeup and expenses, except that the DFA fund has a duration of 1 year, and the Vanguard fund an average duration of 2.2 years (with an average maturity of 2.6 years).



As you can see, it really doesn't make much difference, except at very low risk levels, where the 1 year duration seems to work the best.

The short version? Use bonds/bond funds of less than 5 year average duration or maturity. Expenses are everything, and unless you are already a DFA customer the Vanguard short term funds are tough to beat. One attractive alternative is to construct your own treasury ladder, which will yield about the same as even the DFA and Vanguard short term corporate funds, since the fund expenses are not much less than the currently razor thin spread between treasuries and high grade corporates.

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The Intelligent Asset Allocator

William J. Bernstein

The Brinson 93.6% Hoohah, or,

The Fable of the Blind CFAs and the Portfolio

The worlds of academic and practical finance are a wonder to behold. A decade ago Gary Brinson and 3 of his colleagues published a pair of articles in ***Financial Analysts Journal*** analyzing the variation of quarterly returns of pension fund managers. They came to the conclusion that greater than 90% of the variability of returns of a given fund could be explained on the basis of the allocation to cash, bonds, and stocks. Given the radically different behavior and long term returns of these three assets, this is not exactly front page news. It should not surprise that in the fourth quarter of 1987 an overweighting in stocks resulted in below average returns, while an overweighting in bonds mitigated the damage.

Over the next several years, financial planners of all stripes took up the battle cry -- "Your investment returns are almost entirely the result of asset allocation." Forget stock picking, forget security selection, give us your money to manage and we'll find you a profitable allocation. The FA business boomed.

The backlash was inevitable, although remarkably tardy. This spring William Jahnke published an article in the ***Journal of Financial Planning*** with the incendiary title "The Asset Allocation Hoax." He reanalyzed the Brinson data and came to the conclusion that in fact asset allocation was of minor importance. More specifically, the range of expected returns based on asset allocation alone fell within a 1.0% range (9.47% to 10.47%) , whereas there actually was a 7.55% range (5.85% to 13.4%) of fund performance for the study period. Compare the two numbers and you get a 14.6% contribution of asset allocation, far less than Brinson's 93.6%.

Others have come to the same conclusion. Paul Pudaite, chief statistician at Morningstar Products, came to a very similar estimate of a 16.5% contribution of asset allocation to the performance of a group of mutual funds.

So, who's right? Probably everyone. Remember the story of the blind men and the elephant? Feel only the tusk, trunk, or tail, and you will come to a very different conclusion about the nature of the beast.

By way of illustration, consider the 1970-96 database of national/regional/fixed income assets referred to before on these pages. I've listed both the returns for the full period as well as the last 7 years for each asset:

	1970-96 Return	1990-96 Return
S&P 500	12.27%	14.37%
US Small Stocks	14.15%	15.62%
EAFE-E	13.05%	11.97%
EAFE-PXJ	12.26%	15.67%
EAFE-Japan	14.54%	-4.73%
Gold Equity	13.70%	1.09%

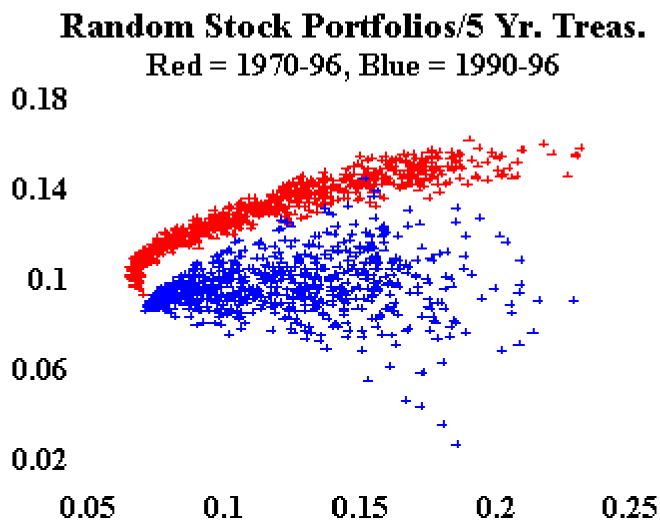
Note how similar the returns are for the 6 equity assets for the 27 year period, and how dissimilar they are for the 7 year period. This database suggests that the allocation of global stock assets makes a much bigger difference over shorter time periods than for longer ones. Looking at asset returns for individual years the average spread of returns between the assets, measured as the SD, was 20.8%; for the whole 27 year period the spread was only 0.88%. If the data for the 27 year period was due to random walk behavior, then one would have expected a 4% spread (20.8% divided by the square root of 27) instead of the 0.88% actually observed. In other words, the stock assets exhibited a strong tendency towards mean reversion.

What does this all mean? Let's consider the returns for each stock asset for the years 1987, 1990, 1993, and 1995:

	1987	1990	1993	1995
S+P 500	5.23	-3.17	9.90	37.53
US Small Stocks	-9.30	-21.56	21.00	34.48
EAFE-E	4.10	-3.37	29.79	22.13
EAFE-PXJ	4.20	-10.15	80.35	12.95
EAFE-Japan	41.87	-36.18	25.05	0.69
Gold Equity	37.53	-23.73	82.87	1.91

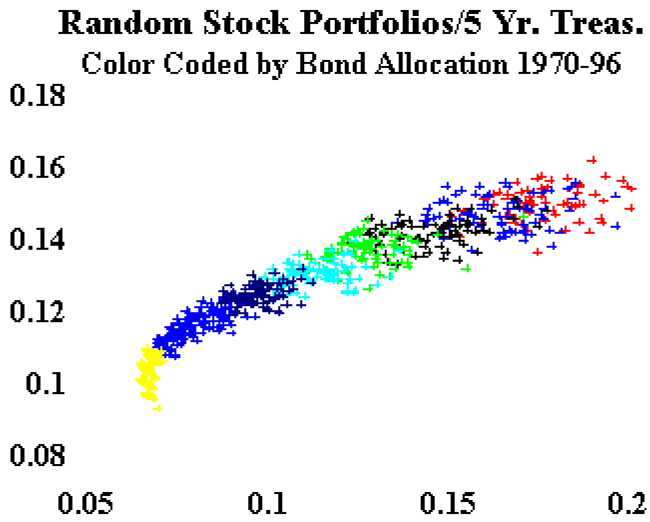
In 1987 a badly managed fund heavy in the Nikkei would have probably outperformed the best managed fund with a high weighting in US microcaps. In 1990 the same Japanese weighting would have killed you. In 1993 the worst manager with a liking for Pacific Rim or gold stocks would have bested the smartest US equity manager, whereas the situation was reversed in 1995. And so it goes. Obviously, then, on a year to year (or even a decade to decade) basis, allocation among global assets matters a great deal, but in the long run, it's a wash.

To further illustrate this point I've used the 1970-96 database to generate 800 random stock portfolios in the following manner: We started with 5 year bond allocations of 0%, 5%, 10%, etc., in 5% increments to 95%. At each 5% bond increment the remaining portfolio was filled out by 40 random allocations of the above 6 stock assets. The returns and SDs for the 27 year 1970-96 as well as the 7 year 1990-96 period were calculated. I'm indebited to colleague David Wilkinson for generating the random portfolios and returns. The results are presented below:



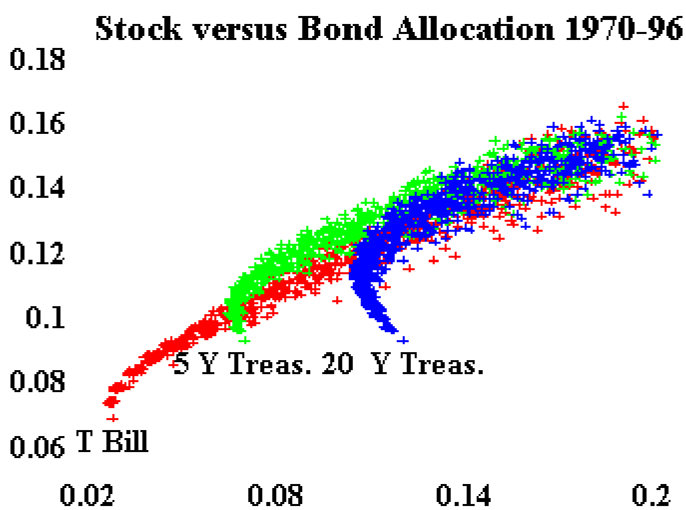
Note how narrow the range of returns is for the longer period at any given level of risk, versus the rather wide variation of returns for the more recent 7 year period. Conclusion number one: Over very long time periods, your allocation among different global assets matters very little. Contrariwise, over shorter periods, it is much more important. Unfortunately, if you're a fund manager and bet on the wrong global pony, you'll be out of a job long before mean reversion can save you.

Next, I plotted the same data for the 27 year period, but this time color coded the data points by bond composition, changing color with every 10% change (2 steps) in bond composition:



Conclusion number 2: Your overall commitment to stocks versus bonds is extremely important, even over very long time periods. This is of course precisely the point Brinson et. al. were trying to make -- stocks versus bonds versus cash matters. It matters a great deal.

Finally, just for fun I combined the plots for the 3 different fixed income categories: 20 year, 5 year, and 30 day treasuries:



Conclusion number 3: Over the long haul long versus short bonds matters only with portfolios of low volatility.

To summarize, then, over the very long term your overall percentage exposure to equity is of primary importance. Your precise global stock allocation is not. Your choice of bond duration is important only if bonds make up the largest part of your portfolio.

Unfortunately, we are all investing in the here and now of the short term. As the above tables and graphs so clearly illustrate, over periods of less than 10 years all aspects of allocation are critical. The short term, if you will, is the unforgiving tusk of the beast -- hard, sharp, and dangerous if approached from the wrong angle. The long term is the trunk -- soft, muscular, and a not uncomfortable way to the top of the animal.

The sorts of return spreads encountered above from the stock-bond paradigm above are in the range of about 5%. The short term spread in global portfolio returns are about 5% also. Over 10 year time periods, the same sorts of differences are seen in fully invested domestic equity funds. So, let's call it a draw: asset allocation and security selection are of about equal importance. In the long run, stock/bond allocation and security selection are also about equally important, but global stock allocation probably drops out of the equation.

Let George Do It

The punch line here is that the answers don't do us much good if we're asking the wrong questions. The real question is not asset allocation versus security selection, but whether active management in any of these areas is worthwhile. In another piece in this month's ***EF*** we show that even the best managed funds do not necessarily produce an efficient portfolio, and the long term success of the Coward's Portfolio suggests that active global managers are simply not capable of besting a mechanically rebalanced arbitrary mixture of global equity indexes (the "Coward's Equity Index," or "CEI"). In fact, one might entitle the portfolio plots shown above "portfolio coward goes on a random walk."

It turns out that one of the few global mutual funds to match the efficiency of the CEI is Mr. Brinson's flagship Global Fund. The fund's allocation is interesting: 22% US equity, 17% foreign equity, 36% dollar denominated bonds, 18% non dollar bonds, and a smattering (6%) of emerging markets securities. The 3 year Sharpe ratio of the fund is 2.18 versus 2.21 for the CEI and 1.08 for the fund's multiasset/global (Morningstar) peers. I guess the moral here is watch the man's hands, not his lips.

Pilots casually refer to their autopilots as "George." George is not very bright, but he does some things very, very, well. For example, if you've ever landed in dense fog, the chances are George did it -- he can fly the runway's instrument landing system (ILS) more precisely and smoothly than any human pilot. In fact, the Category III ILS procedures used to land in the poorest weather conditions are strictly hands-off affairs for the humans watching the gauges -- no fingering allowed. In the same way, efficient global portfolio management is best done on autopilot. Try to outguess the markets and you're liable to establish ground contact considerably off the runway centerline, and maybe even completely off the airport.

So how important is "asset allocation?" Wrong question. More relevant to the investor is the question of how worthwhile professional efforts at both asset allocation and security analysis really are. The film "Less Than Zero" comes to mind.

For small investors the answer seems to be:

1. Decide how much equity you can stomach, and adjust your stock/bond allocation accordingly.
2. Allocate your stock assets among a wide variety of global regions in a prudent manner.
3. Let George do the rest while you get on with life's more salient matters.

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Efficient Frontier

*Editors Note: Bill Jones is a math professor and frequent contributor to the Mutual Fund Interactive main board. I've seen many analyses of dollar cost averaging versus lump sum investing, but Bill's presentation of the conundrum as an insurance problem was both novel and elegant. With his kind permission **EF** reproduces it below.*

Do Not Dollar-Cost-Average for More than Twelve Months

Bill Jones

I am a strong believer in the advantages of DCA (dollar-cost-averaging). I recommend it to anyone making a substantial move from cash to mutual stock funds (unless they fancy themselves to be market-timers). In particular, if you have recently received an inheritance that increases your worth by over 50%, you should use DCA instead of putting it all in at once. If you have a large amount of money that you know you really ought to put into the stock market but you can't bring yourself to do it because you are afraid the market is too high, you should use DCA for 12 months instead of waiting.

However, I find that using DCA for any period longer than 12 months is a bad strategy. It may even be a good idea to DCA over a period as short as 6 months; the choice depends on an individual's balance between risk and return (6 months is a little more risky and a little more profitable in the long run).

I compare two alternative strategies: LUMP-SUM, where you move a substantial amount of money from your money market account to a mutual stock fund today, and DCA-N, where you move one Nth of that substantial amount at the beginning of each of N months starting today. I give historical data to support my conclusions, and I present a full analysis for DCA-6, DCA-12, DCA-18, DCA-24, and DCA-36 in the appendix.

The Logic

DCA is not for people who consider themselves competent market-timers. Market-timers lump-sum in at whatever time they judge that the market is very likely to go up in the near future. And they get back out of the market when they judge that the market is very likely to go down in the near future. The DCA choice is for people who fear that the market may drop drastically at any time, but do not feel competent to judge whether that is more or less likely now than at some other time.

I feel that the purpose of DCA is to avoid the damage from a substantial drop happening in the first few months after a lump-sum investment. 6-month DCA works fine if the drop happens in the first two or three months, because half of the money is invested at a lower cost. But DCA-6 is not effective if the drop happens in the fifth or sixth month. 12-month DCA gives good results for drops that happen in the first 7 or 8 months. My feeling is that, beyond that point, there is generally no substantial problem anyway, because when a drop does not happen for say 8 months, the market usually rises enough in those first 8 months that the net result is a gain.

An additional point is that, if you use say a 24-month DCA period in a particular instance and it is successful, i.e., the stock market falls significantly in the first 6 months, then the loss is more often than not made up in the following 6 months. But that means that the last half of the money is being DCAd into the market at ever high prices. The operation is a success but the patient dies. Your caution is vindicated but you lose anyway. Logically, then, DCA should not be used over periods of 2 or 3 years, not even 18 months. A DCA period between 6 and 12 months is probably best.

Historical Support for DCA-6 Versus DCA-36

But all of the above is theoretical, a subjective opinion based on vague concepts of how stock markets behave. It is helpful to look at some concrete historical data to see how various periods would have turned out. For this, I need a well-defined process for investing. I implement e.g. DCA-12 as follows: Move 1/12 of the assets into the stock market on the first day of the first month and let the rest stay in the money market. One month later, move 1/11 of the remaining money market balance into the stock market, etc., for 12 months.

Equivalently: Divide the initial amount into N equal parts. Move the first part in immediately. Let the second part sit in the money-market for one month and then move it plus its interest into the stock market. Let the third part sit in the money-market for two months and then move it plus its interest into the stock market; etc. Thus the amounts moved in are steadily increasing in nominal dollars, but they are equal in time-value-adjusted terms.

Question: How do we measure the RISK against which we are insuring by DCA? I figure that the main thing to avoid is lump-summing an amount into the stock market and finding out some months later that its value is LESS than we started with. Normally, if we move $10,000 into the stock market in a lump-sum, then we hope to have $11,000 or $12,000 one year later. If we have $10,500, that isn't bad; we would have had the same in a money market. If we have just $10,000, that is disappointing and irritating, but we knew that stock investing wasn't a sure thing. Panic, depression, anger, and regret set in only if we actually have significantly less in nominal dollar terms than we started with.

For DCA-36, I looked at the 493 rolling 36-month periods within the 44 years 1953-1996. I calculated that each use of DCA-36 cost on average 7.40% of total assets compared with lump-summing (computed using the geometric mean, as is normal for computing returns). That is not 7.40% per year for 3 years, but 7.40% for the one decision to use DCA-36 instead of lump-summing.

But the cost is not the only consideration. We have to consider the RISK of severe loss. Note: Stock market returns in the following are based on total returns of the S&P500 including reinvested dividends. Money market returns are estimated using 3-month treasuries, to adjust for interest earned on the money not yet moved into the stock market using DCA. The stock market earned slightly over 5% more per year on the average in this 44-year period; the average dollar in DCA-36 is kept out of the market for 17.5 months, almost 1.5 years, so of course lump-summing beats DCA-36 by roughly 1.5 times 5%.

Unfortunately, there is no really good single number to measure the risk. We all know how unsatisfactory the standard deviation is as a measure of the risk involved in investing in stocks. So the following provides enough statistics to let you make your own decision as to the risks and rewards and costs involved in DCA.

There were only 30 of those 493 DCA-36 cases where a lump-sum had less at the end of 36 months than at the beginning. We want to ameliorate those 30 cases. But it doesn't help to DCA in those disastrous cases if DCA doesn't make it BETTER. So, I set the criterion of effectiveness that DCA should produce results at least 5% better than lump-summing in cases where lump-summing loses money, otherwise it does not offer much protection.

In the 30 cases where lump-summing lost money over 36 months, DCA-36 came out more than 5% better than lump-summing in only 16. Those 30 are easy to categorize: 7 began on 5/1/67 through 11/1/67; 1 of the 30 began on 12/1/68; The other 22 began on 5/1/71 through 2/1/73 (all periods start on the first-of-the-month). Note that the S&P500 lost 25.2% in the 3 months July-Sept 1974.

In the 22 cases involving 1974 where lump-summing lost money, DCA-36 didn't help much for the first 8 of those periods (it was WORSE in 6 cases, 0.2% and 2.1% better in 2 cases), came out 7.7% to 18.4% better for the next 8 periods, and came out 20.8% to 27.5% better than lump-summing for the last 6 of the 22 periods.

That is not a very effective insurance policy, considering the premium: an average loss of 7.40% of total assets for each use of DCA-36 instead of lump-summing. 14 of the 16 "effective" cases included the summer of 1974; the other two began 10/67 when lump-summing lost only 3.33% and 12/68 when lump-summing lost only 3.97%. In other words, DCA-36 is a moderately effective way of guarding against a re-occurrence of the summer of 1974 and is pretty much useless otherwise.

But the main problem is that most of the time DCA-36 doesn't even do what people expect of a DCA strategy: If you DCA-36 and the market tanks in the first 3 or 6 months, sure, you get to gloat that the lump-summer took a 15% loss and you didn't. Then for the next 30 months, you gradually feed piddling little amounts into the market while the lump-summer is making money hand-over-fist. Then when you are completely in, the market tanks again. Is that insurance???

Matters are quite different for DCA-6. I looked at the 523 rolling 6-month periods within the 44 years 1953-1996. Lump-summing came out only 1.11% better on average than DCA-6 (per use of DCA-6, not per year). DCA-6 came out better than lump-summing in 199 of the 523 cases. But that is not the point. We have to consider the RISK of severe loss.

There were 143 cases where a lump-summer had less at the end of 6 months than at the beginning. We want to ameliorate those 143 cases. In 122 of them, DCA-6 came out better than lump-summing. There were only 56 of the 523 cases in which DCA-6 came out more than 5% better than lump-summing, but 55 of those were cases where lump-summing lost money. So DCA-6 helps most where it is most needed.

Of the 32 cases where lump-summing lost over 10%, DCA-6 did better in 30 of them; in fact, in 26 of them, DCA-6 did more than 5% better. And in each of the 3 cases where lump-summing lost over 20%, DCA-6 did 6 to 11% better.

Conclusions

DCA-6 offers some significant protection against lump-summing into the stock market and losing more than 10% in the next 6 months, and it is at a small cost (1.11%). However, since there were only 15 cases in which DCA-6 gave at least 10% more than lump-summing, and in the best case gave just 19.9% more, you are in effect paying a 1.11% premium for an insurance policy that will only pay off well (10 to 20%) 3% of the time. Those are not really great odds.

The payoff is higher for a 12-month period of DCA. Lump-summing lost money in 114 12-month periods, and DCA-12 beat lump-summing in 100 of them, 64 by more than 5%. DCA-12 sometimes beat lump-summing by over 20%. There were 40 12-month periods where lump-summing lost over 10%; DCA-12 beat lump-summing in 39 of those 40. The only drawback is that each use of DCA-12 costs you an average of 2.50% of what you would have if you lump-summed; in general, you come out ahead when lump-summing loses, but gain less when lump-summing wins. But that is what you expect when you dollar-cost-average anyway.

Since I defined effectiveness as meaning that DCA beats lump-summing by more than 5% in those instances where lump-summing loses money, we have: DCA-6 is effective in 55 of 143 cases; DCA-12 is effective in 64 of 114 cases; DCA-18 is effective in 46 of 77 cases; DCA-24 is effective in 31 of 45 cases; and DCA-36 is effective in 16 of 30 cases. Thus DCA-12 clearly produces the highest number of effective cases, but DCA-6 costs half as much and produces the second-highest number of effective cases.

NOTE 1: These results imply that the best timing for people who DCA quarterly is that the last of N equal quarterly payments should be made 6 to 12 months after the first. Thus there should be 3 to 5 equal quarterly payments. NOTE 2: If you DCA into a stock fund with a few hundred dollars a month from your paycheck over a period of many years, because that is all you can afford to save, that is DCA perforce and not something that the above analysis contra-indicates. Personally, I prefer to save it up in a short-term bond fund that I empty once each 6 months, because it simplifies my book-keeping; but I know that slightly reduces my returns vis-a-vis DCA. NOTE 3: DCA is only valuable for moving from cash or bonds into stocks. If you are moving from one stock investment to another, DCA is pointless, because the chances of moving in to a market top are the same as the chances of moving out from a market top, so the risks balance out. On the other hand, it doesn't hurt either.

My conclusion is that DCA for 6 to 12 months is the most that one should use, and then only if moving more than 5% of your total assets (since even the worst case would cut your overall total returns by only 1% or so). If you are shifting 30% or more of your total assets from cash to stock, you could take up to but no more than 18 months; this once-in-a-lifetime sort of situation merits overly-excessive caution. But I provide the data below on which you can base your own opinion.

Appendix (uniform presentation for 5 different time periods)

DCA-6 beat lump-summing 199 of the 523 instances; the lump-summer gained 1.11% on average. Of the 143 instances where lump-summing lost money, DCA-6 beat lump-summing 122 times, 55 of them by at least 5%, and 15 of them by at least 10%. Of the 32 instances where lump-summing lost more than 10%, DCA-6 beat lump-summing 30 times, 26 of them by at least 5%, and 11 of them by at least 10%. Of the 3 instances where lump-summing lost over 20%, DCA-6 did 6.0% (1/62), 9.0% (3/74), and 11.2% (4/74) better. The 6 biggest relative gains for DCA-6 were 13.6% (5/74), 13.6% (6/74), 15.1% (7/74), 13.5% (8/87), 19.3% (9/87), and 19.7% (10/87) more than lump-summing.

DCA-12 beat lump-summing 175 of the 517 instances; the lump-summer gained 2.50% on average. Of the 114 instances where lump-summing lost money, DCA-12 beat lump-summing 100 times, 64 of them by at least 5%, and 30 of them by at least 10%. Of the 40 instances where lump-summing lost more than 10%, DCA-12 beat lump-summing 39 times, 34 of them by at least 5%, and 20 of them by at least 10%. Of the 8 instances where lump-summing lost more than 20%, DCA-12 beat lump-summing every time, all of them by more than 5%, and

7 of them by more than 10%. The 6 biggest relative gains for DCA-12 were 20.9% (11/73), 18.6% (2/74), 19.9% (3/74), 18.7% (4/74), 20.1% (9/87), and 19.4% (10/87) more than lump-summing.

DCA-18 beat lump-summing 170 of the 511 instances; the lump-summer gained 3.84% on average. Of the 77 instances where lump-summing lost money, DCA-18 beat lump-summing 69 times, 46 of them by at least 5%, and 33 of them by at least 10%. Of the 27 instances where lump-summing lost more than 10%, DCA-18 beat lump-summing all 27 times, 26 of them by at least 5%, and 22 of them by at least 10%. Of the 12 instances where lump-summing lost more than 20%, DCA-18 beat lump-summing every time, all of them by more than 12%. The 6 biggest relative gains for DCA-18 were 22.5% (8/73), 21.0% (9/73), 24.7% (10/73), 25.4% (11/73), 18.2% (9/87), and 19.7% (10/87) more than lump-summing. DCA-24 beat lump-summing 158 of the 505 instances; the lump-summer gained 5.06% on average. Of the 45 instances where lump-summing lost money, DCA-24 beat lump-summing 39 times, 31 of them by at least 5%, and 21 of them by at least 10%. Of the 24 instances where lump-summing lost more than 10%, DCA-24 beat lump-summing every time, 21 of them by at least 5%, and 15 of them by at least 10%. Of the 9 instances where lump-summing lost more than 20%, DCA-24 beat lump-summing every time, all of them by at least 5%, and 7 of them by at least 10%. The 6 biggest relative gains for DCA-24 were 24.0% (1/73), 24.4% (2/73), 23.7% (4/73), 23.2% (8/73), 24.1% (10/73), and 24.5% (11/73) more than lump-summing. 9/87 had a gain of 17.6% and 10/87 had a gain of 15.4% more than lump-summing.

DCA-36 beat lump-summing 127 of the 493 instances; the lump-summer gained 7.40% on average. Of the 30 instances where lump-summing lost money, DCA-36 beat lump-summing 22 times, 16 of them by at least 5%, and 14 of them by at least 10%. Of the 14 instances where lump-summing lost more than 10%, DCA-36 beat lump-summing 10 times, 9 of them by at least 5%, and 8 of them by at least 10%. Of the 2 instances where lump-summing lost more than 20%, DCA-36 beat lump-summing only once, by 7.7%. The 6 biggest relative gains for DCA-36 were 22.2%, 26.2%, 27.5%, 26.6%, 24.1%, and 24.1% more than lump-summing for the 6 periods beginning 11/72 through 4/73, respectively. 9/87 had a gain of 13.7% and 10/87 had a gain of 11.6% more than lump-summing.

Alternative Criterion

We could use an alternative definition of effectiveness based on the amount of protection provided in the worst 50 cases (out of the 500 or so, thus roughly 10% of cases). Here are the results:

DCA-06 beat lump-sum 46 times, 34 by 5%, 11 by 10%. Lump-sum lost over 7.3% 50 times.

DCA-12 beat lump-sum 48 times, 40 by 5%, 22 by 10%. Lump-sum lost over 8.2% 50 times. DCA-18 beat lump-sum 47 times, 34 by 5%, 26 by 10%. Lump-sum lost over 3.6% 50 times.

DCA-24 beat lump-sum 41 times, 33 by 5%, 22 by 10%. Lump-sum gained under 1.2% 50 times.

DCA-36 beat lump-sum 36 times, 30 by 5%, 25 by 10%. Lump-sum gained under 9.1% 50 times.

By this criterion, the maximum "insurance protection" against the worst losses is offered by DCA for 12 to 18 months. But since the "insurance premium" is hefty and increases from 1.11% to 3.84% over this period, the best balance occurs around 6 to 12 months.

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