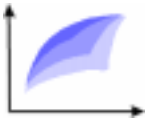
Efficient Frontier



An Online Journal of Practical Asset Allocation

Edited by William J. Bernstein
and Susan F. Sharin

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Efficient Frontier

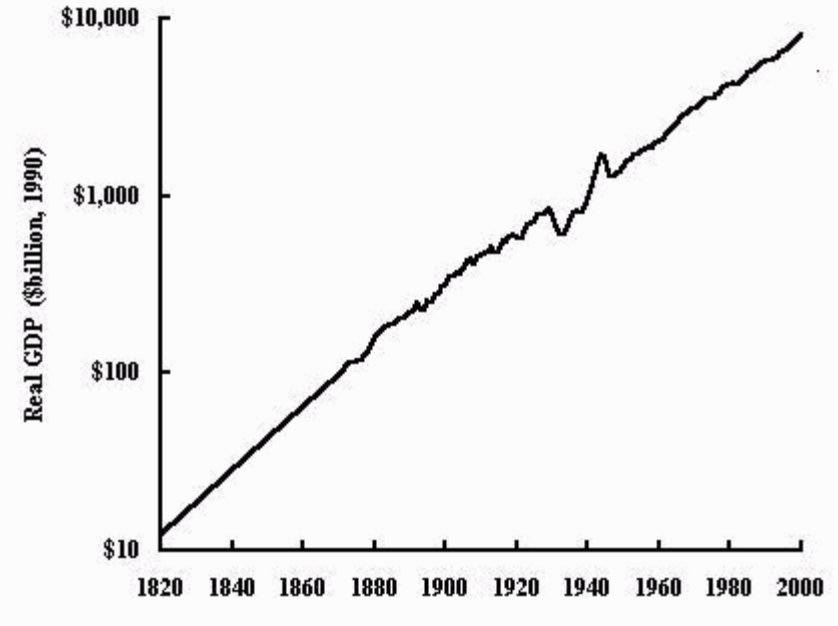
William J. Bernstein

The Two-Percent Dilution

Over the past two centuries, common stocks have provided a sizeable risk premium to U.S. investors. For the 200 years from 1802 until 2001, inclusive, the returns for stocks, bonds, and bills were 8.42%, 4.88%, and 4.21%, respectively. In the most simplistic terms, the reason is obvious: a bill or a bond is a promise to pay interest and principal and, as such, its upside is sharply limited. Shares of common stock, on the other hand, are claims on the future dividend streams of the nation's businesses. Instead of a fixed, paltry trickle from low-risk fixed-income securities, the ever-increasing fruits of technologically-driven economic growth fall to the shareholders.

Viewed over the decades, the national economy grows with remarkable uniformity. Figure 1 plots the real GDP of the U.S since 1820:

Figure 1
U.S. ($Billion, 1990)

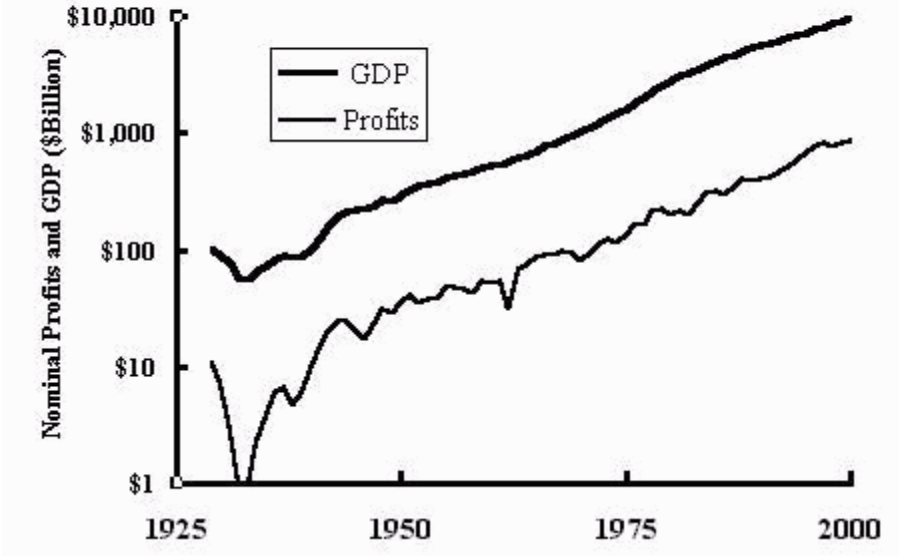


During this period, real GDP has grown fairly evenly at about 3.6% per year. The

long-term uniformity of economic growth is both a blessing and a curse. It is reassuring to know that real U.S. GDP doubles once every 20 years (and real per-capita GDP once per generation). But it is also a dire warning to those predicting a rapid acceleration of economic growth from the computer and Internet revolutions. Such extrapolations of technologically-driven increased growth are painfully oblivious to the broad sweep of scientific and financial history. The impact of recent advances in computer science pales in comparison to the technological explosion that occurred between 1820 and 1850. This earlier era saw the most deep, far-reaching technology-driven changes in everyday existence throughout human history. They profoundly affected the lives of those from the top to the bottom of the social fabric in ways that can hardly be imagined today. At a stroke, the speed of transportation increased tenfold, and communications became almost instantaneous. Before 1820, people, goods, and information could not move faster than the speed of the horse. Within a generation, journeys that had previously taken weeks and months now involved an order of magnitude less time, expense, danger, and discomfort. Important information could be instantaneously transmitted. Put another way, the average inhabitant of 1800 would have found the world of 50 years later incomprehensible, whereas a person transported from 1950 to 2000 would have little trouble understanding the relatively small intervening changes in everyday life.

The comparatively uniform increase in GDP also implies a similar uniformity in the growth of corporate profits, which is, in fact, the case. Figure 2 demonstrates that, except for the Great Depression when net corporate profits disappeared, aggregate company earnings have remained fairly constant at about 10% of nominal GDP:

Figure 2
U.S. Nominal Corporate Profits and GDP



Should it not follow that stock prices also grow at the same rate? After all, there has been a direct relationship between corporate profits and GDP since 1929. Unfortunately for the shareholder, earnings and dividends will keep up with GDP *only if no new shares are created*. Since 1871, real stock prices have grown at 2.48% per annum versus 3.45% for the GDP (the slightly slower growth rate for the more recent period reflecting the slowdown in population growth). There has thus been about 1% per year of "slippage" between stock prices and GDP. Further, as we shall see, the true degree of slippage is quite a bit higher, since much of the 2.48% rise in real stock prices after 1871 was due to an upward revaluation, as the highly illiquid industrial stocks of the post-Civil War period, selling at three to four times earnings, gave way to instantly and cheaply tradable common shares selling that much more dearly.

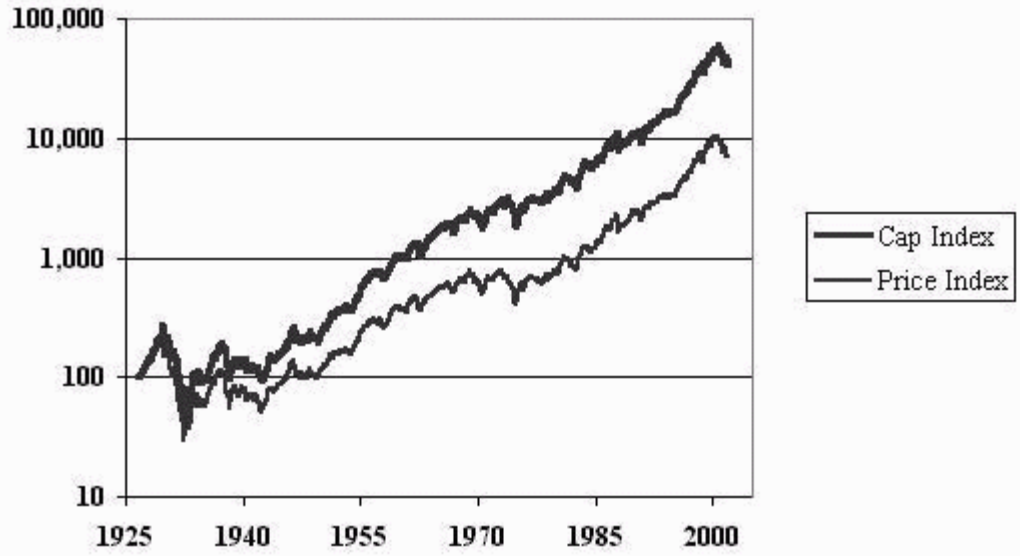
This slippage is the result of the net creation of shares, as existing and new companies capitalize their businesses with equity. It suggests a very simple paradigm for measuring the degree of slippage: the ratio of the proportionate increase in market capitalization to the proportionate increase in price. For example, if over a given period, the market cap increases by a factor of ten, and the cap-weighted price index increases by a factor of five, then there has been 100% net share issuance in the interim. More formally,

$$\text{Net Dilution} = \{(1+c)/(1+r)\} - 1$$

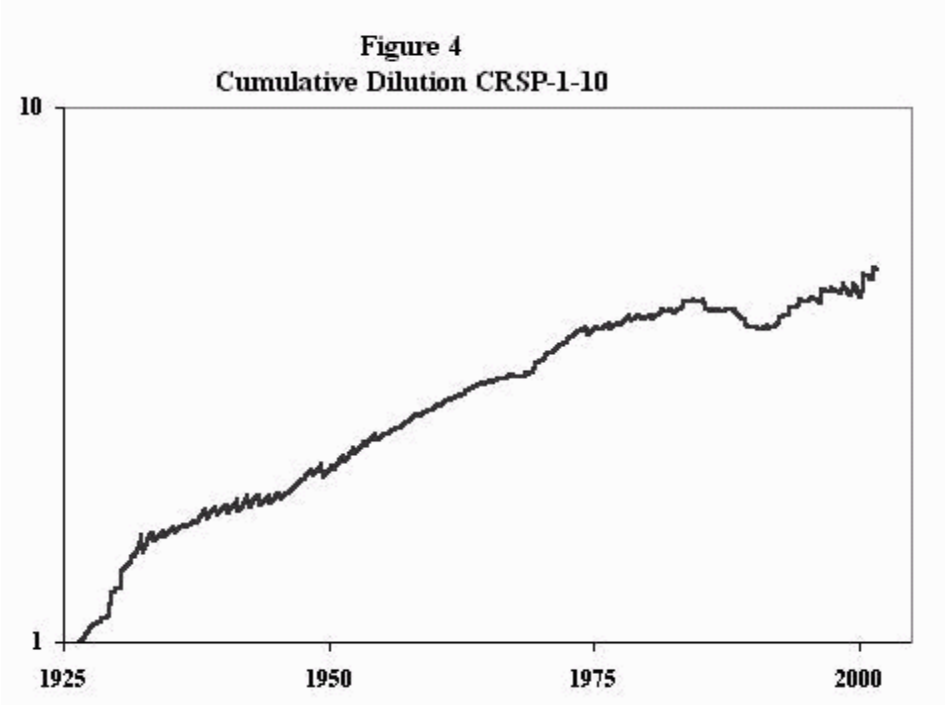
where c = capitalization increase, and r = price return

This relationship has the advantage of factoring out valuation changes, as they are embedded in both the numerator and denominator. Further, it holds only for universal market indexes such as the CRSP 1-10 or the Wilshire 5000, since less inclusive indexes can vary the above ratio simply by adding or dropping securities. In Figure 3, we plot the total market cap and price index of the CRSP 1-10, with 1926 equal to 100:

Figure 3
Price and Cap Indexes, CRSP 1-10
1926 = 100



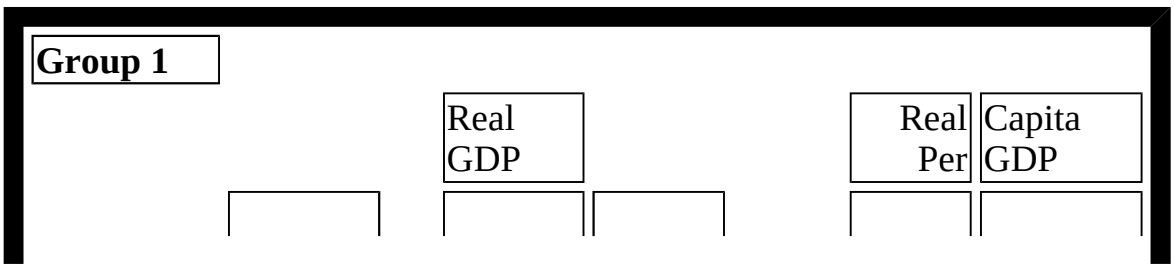
Note how market cap slowly and gradually pulls away from market price. By the end of 2001, the cap index has grown 4.97 times larger than the price index, suggesting that for every share of stock extant in 1926, there are now 4.97 shares! To give a better idea of how this has proceeded over the past 75 years, in Figure 4 we plot this dilution index, defined as the cumulative net creation of new shares:



These data are consistent with a nearly continuous net dilution of common shares. The process is seen to have been more rapid during the late 1920s, quickly decelerating after the crash of 1929. As capital costs rose in the 1970s, it slowed yet further, and during the late 1980s, there was even a brief net contraction as companies responded to peak capital costs with stock buybacks. However, in the 1990s, shares again began to dilute. The overall rate of dilution since 1926 is 2.15% per year.

The slippage between aggregate economic data and per-share performance can be independently examined by comparing the rise in per-share corporate dividends versus GDP growth around the globe. Recently Dimson, Marsh, and Staunton, in their wonderful monograph, *The Triumph of the Optimists*, have examined the real dividend-growth rates in 16 nations over the entire 20th century. These can then be compared to the growth of real GDP and real per-capita GDP growth rates.

We divide these nations into two categories according to the degree of devastation visited upon them by the calamities of the 20th century: Group 1, which suffered no substantial destruction of their productive physical capital during World Wars I and II and the Spanish Civil War, and Group 2, which did.



	Div Growth		Growth	Dilution		Growth	Dilution
Ireland	-0.80%		2.21%	3.01%		2.05%	2.85%
Switzerland	0.10%		2.66%	2.56%		1.85%	1.75%
Canada	0.30%		3.87%	3.57%		2.07%	1.77%
UK	0.40%		1.89%	1.49%		1.44%	1.04%
US	0.60%		3.28%	2.68%		1.96%	1.36%
Australia	0.90%		3.29%	2.39%		1.60%	0.70%
S. Africa	1.50%		3.49%	1.99%		1.16%	-0.34%
Sweden	2.30%		2.62%	0.32%		2.05%	-0.25%
Average	**0.66%**		**2.91%**	**2.25%**		**1.77%**	**1.11%**

Group 2

	Div Growth	Real GDP Growth	Dilution	Real Per Capita GDP Growth	Dilution
Japan	-3.30%	4.11%	7.41%	2.99%	6.29%
Italy	-2.20%	2.96%	5.16%	2.40%	4.60%
Denmark	-1.90%	2.86%	4.76%	2.09%	3.99%
Belgium	-1.70%	2.15%	3.85%	1.72%	3.42%
Germany	-1.30%	2.78%	4.08%	1.79%	3.09%
France	-1.10%	2.37%	3.47%	1.99%	3.09%
Spain	-0.80%	2.79%	3.59%	2.00%	2.80%
Netherlands	-0.50%	2.96%	3.46%	1.80%	2.30%
Average	**-1.60%**	**2.87%**	**4.47%**	**2.10%**	**3.70%**

The first column in each table tabulates the growth of real per-share dividends in each nation between 1900 and 1998, and the second, the growth of real aggregate GDP for the full century. The third column tabulates the difference between the two. It is noted that in all nations per-share dividends grow more slowly than aggregate GDP. The gap is lowest in Sweden at 0.32% per year, and more than 2% per year in five of the eight Group 1 nations, including the U.S, where it was

2.68%. This is close to the 2.15% value obtained by the market-cap/market-price model. It is even closer to the 2.25% average for the Group 1 nations. The fourth and fifth columns do the same for per-capita GDP, where gaps of 1.11% and 3.70% are found for Groups 1 and 2, respectively.

The data for Group 2 are striking: Amazingly their economies repaired the devastation wrought by the 20th century, with overall GDP and per-capita GDP growth rates equivalent to Group 1. The bad news is that the same cannot be said for per-share equity performance; there was almost 4.5% slippage between the growth of their economies and per-share corporate payouts.

It thus seems that in "normal nations" of Group 1—those untroubled by war, political instability, and government confiscation of the economic commanding heights—the ongoing capitalization of new technologies produces a net dilution of outstanding shares of about 2% per year. (Did I hear anybody say "stock buybacks?" Ah, then I've some wonderful vacation plots in the Everglades to show you.) The Group 2 nations represent a more fascinating phenomenon. These can be thought of as experiments of nature in which physical capital is devastated and must be rebuilt. Fortunately, it is much harder to destroy a nation's intellectual, cultural, and human capital; within little more than a generation, GDP and per capita GDP catches up with, and in some cases surpasses, the Group 1 averages. Unfortunately, this requires a high rate of equity recapitalization, reflected in the large dilutions seen in columns 3 and 5, and which mulcts existing shareholders.

This analysis has disturbing implications for paradigmistas convinced of the revolutionary nature of biotech, the Internet, and personal computers. It may very well be that a rapid rate of technological change could, in effect, turn a Group 1 nation into a Group 2 nation, as an increased rate of obsolescence destroys the economic value of plant and equipment as surely as bombs and bullets. The resultant recapitalization would then dilute per-share payouts much faster than the technology-driven acceleration of economic growth, if such acceleration exists at all.

But whatever the true nature of the interaction of technologic progress and per-share earnings, dividends, and prices, it will come as an unpleasant surprise to many that even in the Group 1 nations, average real per-share dividend growth was only 0.66% per year; for the Group 2 nations, it was strongly negative.

Thus, at the dawn of the new millennium, the equity investor cannot expect a real return greatly in excess of a generally derisory dividend yield. Nor will he be rescued by more rapid economic growth, which is unlikely to occur. But even if it does, its benefits will undoubtedly be more than offset by the dilution of his ownership interest necessitated by technologically-driven increased capital needs.

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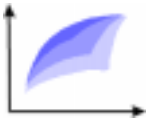

William J. Bernstein

The Big Lie

As Joseph Goebbels knew when he drafted the Nazi campaign blaming the Jews for Germany's failure to rise out of the ashes of the Great War, if you tell a lie often enough, it eventually becomes accepted as the truth.

Year after year, investment pundits repeat a fundamental falsehood about indexing that has somehow acquired the ring of truth—that indexing works for large-cap stocks, but not for foreign or small-cap stocks. The origins of this monstrosity are lost to time, but Charles Schwab deserves a major portion of the credit for popularizing this notion in its "core-and-explore" strategy, where one indexes more of a portfolio's large-cap segment than its small and foreign.

I had hoped that core-and-explore's manifest failure over the past three years would put this old chestnut to rest, but sadly, it hasn't. In the Mutual Funds supplement to the May 6, 2002 *Wall Street Journal*, former Fidelity chief Robert Pozen, now firmly ensconced at Harvard, opined: "Active managers beat the relevant indexes on a regular basis for things like international funds, small-cap funds, etc."

Well, I really didn't expect a balanced view of indexing from a Fido bigwig, but I had hoped that Harvard profs were a bit more data-driven. I've written about this one until my fingers ache, but no one seems to listen. So, Professor Pozen, this one's for you.

Small Cap

Let's start by dividing small cap into value, growth, and blend categories. The oldest small-cap-value index fund is the DFA U.S. Small Cap Value, which ranks 7th out of 58 funds at five years. It incepted in 1993, but it's not much of a stretch to extend its record backwards by tacking on the performance of the Fama-French small value index, which it tracks closely. (Actually, almost all DFA funds *beat* their underlying indexes, so this is a more-than-fair approximation.) At 10 years, this fund would rank *first* of 14 funds, and at 15 years, *first* of nine funds.

For small blend, it all depends on which index you use. The Vanguard Small-Cap Index fund ranks only 26th of 36 funds at 10 years, and the underlying Russell 2000 Index would have ranked 12th of 16 funds at 15 years. But DFA U.S. Micro Cap ranks 6th of 36 at 10 years and 7th of 17 at 15 years, and DFA U.S. Small Cap, exactly in the middle of the pack at 10 and 15 years. It turns out that there's a problem with the Russell 2000 Index—it is rebalanced every June 30. Since it is defined as the 1001st through 3000th stocks ranked by market cap, and since it is the most widely used small-cap index, savvy traders can easily predict which stocks will be added and dropped from the index, bidding these stocks up or down before June 30, adversely impacting the indexers who must buy or sell these stocks *after* June 30, lest they incur increased tracking error.

The DFA funds do not suffer from these disadvantages. Neither does the S&P 600, as it is committee-chosen and thus impossible to predict which stocks will be added and dropped; were it a fund, it would rank 60th of 160 funds at five years, and 13th of 36 at 10 years. In any case, the mediocre performance of small-blend indexing is largely an illusion—adjust for survivorship bias, and even the Vanguard Small-Cap Index fund beats the average actively managed fund by a significant margin.

Finally, small growth, as we've noted before, is a real problem for indexers. The Fama-French small growth index would have ranked 34th out of 53 funds at 10 years and 22nd out of 25 funds at 15 years. We've been down this road before—momentum effects are strongest in the small growth arena. An index fund that kicks out its strongest performers, which a small growth and to a lesser extent a small blend fund does, suffers accordingly. So, yes, don't buy a small-cap-growth index fund. But the larger point is *simply not to buy small growth funds at all*—this is a miserable asset class, with long-term historical returns lower than all other market segments.

Finally, REITs. Surely, this a specialized, inefficient area where savvy analysts should be able to pick out underpriced securities. Well, no. DFA Real Estate Securities ranks 13th of 60 at five years; the Vanguard REIT Index offering, 28th. Again, add in survivorship bias, and the Vanguard fund too outperforms by a handy margin.

Foreign

At first blush, indexing foreign stocks also appears to be a loser: the MSCI-EAFE, were it a fund, would have ranked 45th of 77 foreign entries at 10 years, and nearly dead last at 15 years.

The problem, of course, can be explained in one word—Japan. Indexing the

foreign market in 1989-1990 would have resulted in a portfolio consisting of nearly two-thirds Japanese equity, something that even the most ardent indexer would not likely do, and which killed the EAFE index for over a decade and a half.

So, let's break things down by region. At 10 years, the Vanguard European Index fund ranked 7th out of 19; at 15 years, the index it's based on would have ranked first of seven. Pacific Rim? There's no fund with a 15-year track record, but the MSCI Pacific-ex-Japan Index would rank second of four at 10 years and 20th of 46 at five years.

Emerging Markets? Now, if you buy the argument that active management adds the most value in inefficient markets, it should do so in this arena. The best index data are for the DFA Emerging Markets and Emerging Markets Value funds. At five years, they rank 30th and 5th of 106 funds, and at 10 years, adding on their strategies before their 1994 incepts, they would rank second and first of seven funds. (The Vanguard Emerging Markets fund ranks 46th of 106 at five years; since they don't precisely track the MSCI Emerging Markets Index, it's difficult to judge just how well they would have done at ten years.)

Lastly, I can't help myself from adding in a value twist: At five years, DFA International Value ranked 122nd of 423 diversified foreign funds, even with its heavy Japanese market weighting. Adding on its strategy before the 1994 incept, it would rank 15th of 79 at ten years, and 4th of 29 at 15 years.

No one really expects the truth from anyone at Fidelity. But, Mr. Pozen, you're in the big leagues now. People expect that when a Harvard professor opens his mouth he's at least had an ever-so-brief look at the *data*. Better luck next time.

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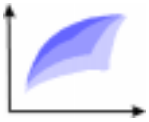

William J. Bernstein

S.W.I.N.E.

When I reflect on how much we can be certain of in finance, I'm reminded of Li'l Abner's famous 1960s protest group: Students Wildly Indignant about Nearly Everything (which most readers of a certain age will remember).

Change "Indignant" to "Ignorant" and you pretty well sum up the state of knowledge in financial economics. Let's face it, finance is a science in the same way that home economics is economics. The essence of a science is the presence of reasonably reproducible phenomena, and nothing (and I do mean *nothing*), is reproducible in finance. In the hall-of-mirrors world of investing, the fact that something was true in the past strongly suggests that it will not be true going forward. You say that stock returns are lowest on Mondays? Perhaps this was so, but as it became well known, enough investors saved their buying for the blue day (including myself), and this peculiar calendar quirk disappeared.

And that's even *before* we consider September 11th-type shocks. As we found out all too clearly, geopolitical events beyond our control can take hold of the financial markets and shake them like beans in a castanet. This is nothing new. Consider these words of wisdom from Keynes' *General Theory*, discussing economic "uncertainty":

> The sense in which I am using the term is that in which the prospect of a European war is uncertain, or the price of copper and the rate of interest twenty years hence, or the obsolescence of a new invention . . . About these matters, there is no scientific basis on which to form any calculable probability whatever. We simply do not know!

Too bad Meriwether, Scholes, and their merry band hadn't read this before they nearly blew up the world economy in 1998.

I find myself increasingly the recipient of a wide variety of questions regarding the behavior of asset classes and portfolios, and in almost all cases end up saying, "I simply do not know!" (Then perhaps adding, with a soupçon of rationalization, "And I don't feel bad about it, because I don't think anyone else does either.")

So I've decided to divide the Big Questions in investing into three categories: those we definitely know the answer to, those we definitely don't, and those reasonable people can argue about.

What We Do Know

1. Risk and return are strongly related. This, in fact, is the prime directive of finance. Although not formalized by Sharpe and Markowitz until decades ago, it was well known to the ancients, who charged good credits lower rates of interest than bad credits or upped rates in time of war and disorder. It's backed by an enormous body of empirical data, both here and abroad: safe assets, such as short bonds and time deposits, over long enough periods always have lower returns than riskier assets, such as stocks. And, like all concepts with airtight empirical backing, it is also intuitively obvious: if two assets throw off the same amount of income, the safer one will sell for the higher price and thus a lower yield.

2. The net return of speculation is zero. Bachelier's famous dictum is axiomatic; there is no net benefit to the two individuals on opposite sides of a transaction. Taking this one step further, it is only possible to profit from a trade when you know more than the person on the other side. Over 70 years of empirical data show us that trading superiority, for all practical purposes, does not exist—that is, the markets are efficient.

3. Costs matter. Your net investment return must of necessity be the aggregate return of the securities you own minus your expenses, including those caused by the market impact of your trades.

4. Of the three major relative stock-asset-class characteristics—return, correlation, and volatility—only the latter seems to have any predictive value. That is to say, over the next five years, we can be reasonably certain that Turkish stocks will be riskier than U.S. stocks. On the other hand, we cannot say with certainty whose returns will be higher or lower. Correlation is an intermediate case: relative correlations—the higher value of U.S./France versus Turkey/France—will likely persist as well, but not with as much certainty as volatility.

That's it. Four things.

What We Do Not Know, And Never Will

1. Where is the market going tomorrow? We have more than 70 years of data

on this one too. No one knows. And if someone does, she ain't talkin'.

2. Which securities should I own? The market can be thought of as an enormous computer whose job is to amalgamate all of the available information, both public and private, into extremely accurate security prices. The only way you can beat it, is if you are privy to non-public information or smarter than all of the other market participants. If you believe that either of these is true, then either your name is Ken Lay or you are an overconfident buffoon (or both).

3. What's the optimal future portfolio composition? This is the big kahuna of the portfolio analyst and it is absolutely unobtainable, since it is basically a variant of points one and two above. Yes, it is theoretically possible to calculate the optimal portfolio at any degree of risk if you know return, risk, and correlation. Unfortunately, the least predictable of these—return—is also the most critical to the calculation. The net effect on your wealth of feeding historical data into any portfolio black box, whether you're simply backtesting, optimizing, or (drum roll, please) resampling, is about what you would get by tossing raw lumber into the intake of a jet engine.

In this list, there's a paradox associated with every item. With the first and second cases, if you actually knew the answer, you would shortly become one of the world's wealthiest people and surely wouldn't be squinting into your monitor reading *me*. In the last case, if you knew the future returns of all assets, you wouldn't need optimization or any other technique. You'd simply buy the highest-return asset and go to the beach.

Finally, I include here the most common specific question I get asked: Is there a role for a mid-cap allocation in an efficient portfolio? I don't know; no one does.

What Reasonable People Can Argue About

These are what physicists call Deep Questions; if you know the answer to any of the following, please drop me a line:

1. Is there a value effect? Personally, I think the answer is yes, as you can see from an accompanying piece. But there are some very smart people who disagree, one of whom is named Bogle. You quickly learn that when you disagree with the Sage of Valley Forge, you usually wind up cleaning egg off your face.

2. Do small stocks have higher returns than large stocks? See point one above, then erase 80% of the empirical support.

3. Do retained earnings increase earnings growth? You'd think that each percent of retained earnings produces a percent of extra return. But it very well may not. In this regard, REITs can be considered an experiment of nature in which Congress mandated that 90% of a whole sector's earnings wind up in investor's pockets. From January 1975 through April 2002, REITs have bested the S&P by 1.34% per year, truly remarkable when you realize that the latter have tripled in valuation in the interim, whereas the former have not budged their PEs or yields in the intervening 27 years. But it's hard making judgments on just one sector. This is a testable hypothesis, of course—one could sort the CRSP by payout ratio and adjust for size and value. But that smacks of real work, which my doctor tells me to rigorously avoid.

4. Is the historical equity risk premium useful in predicting the forward risk premium? I think not; there are numerous instances where very long periods of high/low risk premia reversed for equally long subsequent periods, such as bonds in the 19th and 20th centuries. More to the point, very high/low risk premia imply very high/low prices, which certainly do *not* imply subsequent high/low returns. But what do I know? I don't have an endowed chair at Yale.

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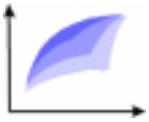


William J. Bernstein

The Cross-Section of Expected Stock Returns: A Tenth Anniversary Reflection

Ten years ago this month, Eugene Fama and Kenneth French fired the shot heard ’round the world. Its echoes still plainly reverberate today in boardrooms and trading floors. And although most investors are unaware, these effects also appear regularly in their mailboxes under the guise of investment-account statements.

The projectile in question was a 39-page piece bearing the above title, published in the June 1992 edition of *Journal of Finance*. It was no walk in the park; even among the *Journal*’s rarefied readership, I doubt many grasped the full meaning without multiple readings and hours of peer discussion.

Its import lay on three levels:

- The month-to-month performance of a diversified portfolio of U.S. stocks can be explained by only three factors: the portfolio’s exposure to the market itself, to small-cap stocks, and to value orientation (the latter defined by price-to-book ratio). In plain English, "Show me the returns series of any U.S. diversified portfolio and, in almost every case, I can explain nearly all its performance based on these three factors; the precise securities are irrelevant." And, "Oh, by the way, without knowing exactly what’s in this portfolio, I can tell you the median market cap and price-to-book ratio just by looking at its returns."

- The corollary of their work was that once one considered the size and value factor "loadings" of a diversified U.S. all-stock portfolio, the market loading—Sharpe’s famous "beta"—did not explain return. In other words, portfolios of high-beta stocks did not have higher returns than portfolios of low-beta stocks. Beta was dead.

- Most importantly, the size and value factors, because they were surrogates for risk, had positive returns. Therefore, value stocks should have higher returns than growth stocks, and small stocks, higher returns than large stocks; small value stocks should have the highest returns of all. The one

> place where the model "didn't work" was with small growth stocks, which empirically had much lower returns than expected, having the worst performance of the four "style corners" (large growth, large value, small value, and small growth).

Heeding these findings, investors (myself included) began to accumulate small- and value-weighted portfolios and promptly had their heads handed to them. Suppose it took several months to read the piece, confer with your savviest colleagues, and assemble a portfolio loaded down with small value stocks on January 1, 1993. Here's how the "four corners" of the equity world, as defined by Fama and French, would have fared over the next seven years.

(But first, let me explain the Fama and French definition of the four corner portfolios. In the simplest case, stocks are split into two halves by size: "large" constitutes size deciles one through five of the CRSP (Center for Research in Security Prices) database, and "small," deciles six through ten. "Value" and "growth" are defined as the bottom and top 30% of stocks sorted by price/book.)

Annualized Returns, January 1993 to December 1999
Small Value: 13.90%
Small Growth: 16.92%
Large Value: 15.72%
Large Growth: 21.64%
Wilshire 5000: 20.47%

Mind you, you'd still have done fine, thank you. But not nearly as well as your uncouth, beer-swilling, Janus fund-buying neighbors. (And not as well, for that matter, as the folks at Vanguard, who have never bought into the model and still insist that the optimal core equity holding is their Total Stock Market Fund, which tracks the Wilshire 5000 and is heavily weighted towards large growth stocks.)

It didn't help that the biggest proponent of the model was Dimensional Fund Advisors, an institutional fund company that eschews the mass market and does not go out of its way to cultivate journalists. The latter proceeded to have a field day at the expense of multifactor investing, turning back on its creators the old efficient-marketeer observation that market-beating strategies have a nasty habit of disappearing the moment they are described.

Academicians raised a more serious objection—Fama and French were guilty of data-mining; their results were an artifact of the U.S. market during the article's study period from 1963 to 1990. And finally, practitioners raised the most serious objection of all: small- and value-oriented strategies could not be implemented. Yes, in a frictionless world, excess returns could be earned. But in the real world, you'd be eaten alive with commissions and transactional costs.

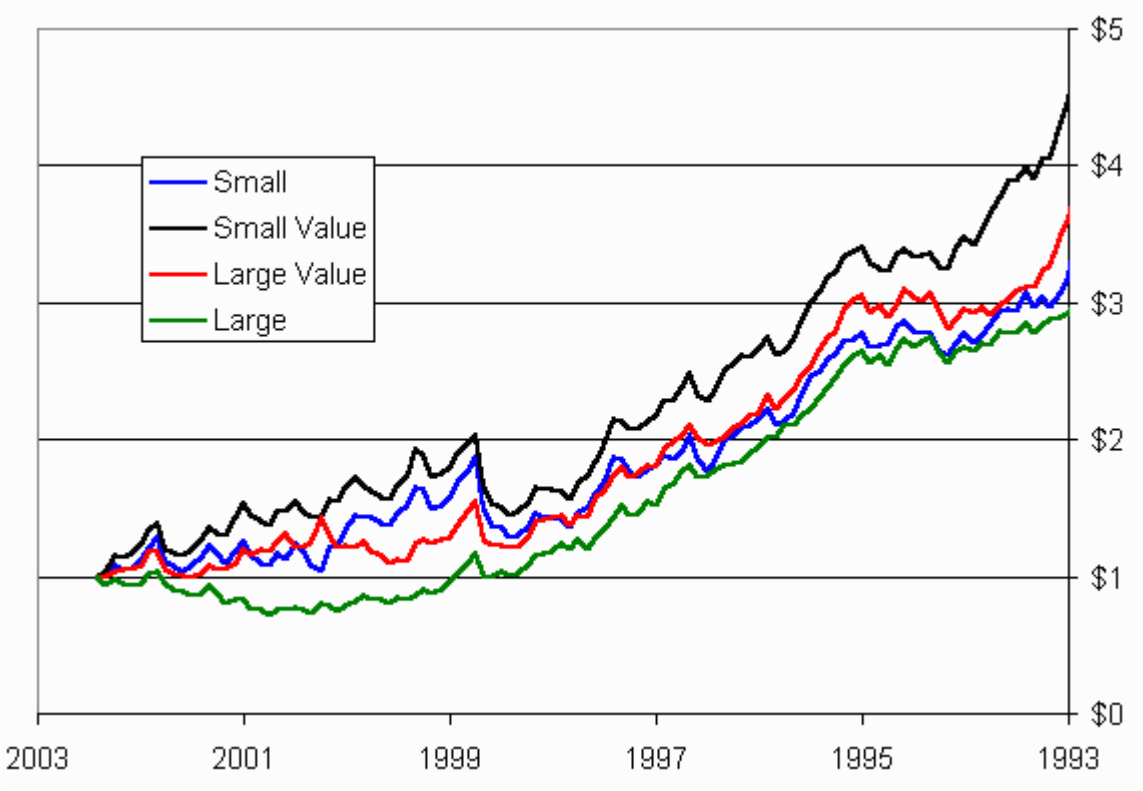
In their quiet way, Fama and French disposed of the data-mining *j'accuse*. They examined stock markets abroad, then those in the U.S. before 1963. Value and size premia were found on every hill and under every rock.

Then at last, the markets themselves came to their rescue. In 2000, the tech-led large-growth dominance began to violently unwind. By early 2002, all the damage to a multifactor strategy was more than undone. So let's extend the returns of the above hypothetical investor to April 30, 2002.

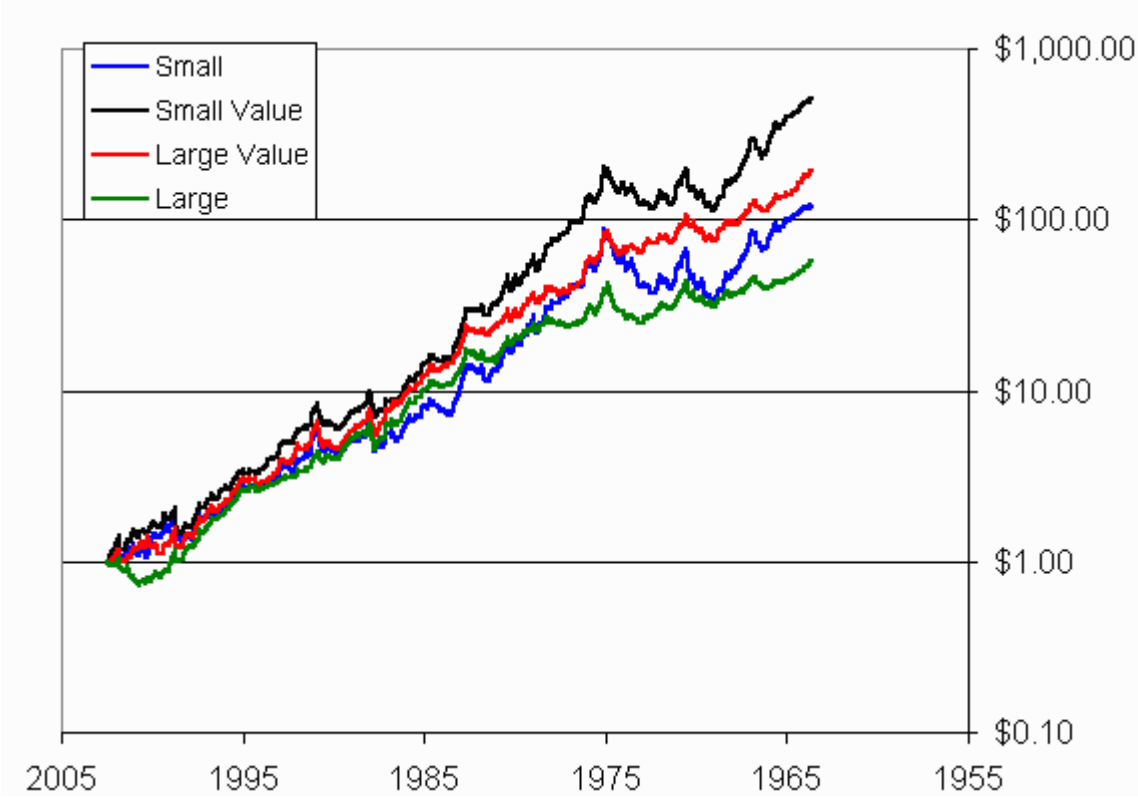
Annualized Returns, January 1993 to April 2002
Small Value: 15.23%
Small Growth: 8.20%
Large Value: 10.43%
Large Growth: 11.08%
Wilshire 5000: 11.69%

Suddenly, the world according to Fama and French is a much happier place, where savvy, patient investors favoring small value stocks reap their due rewards.

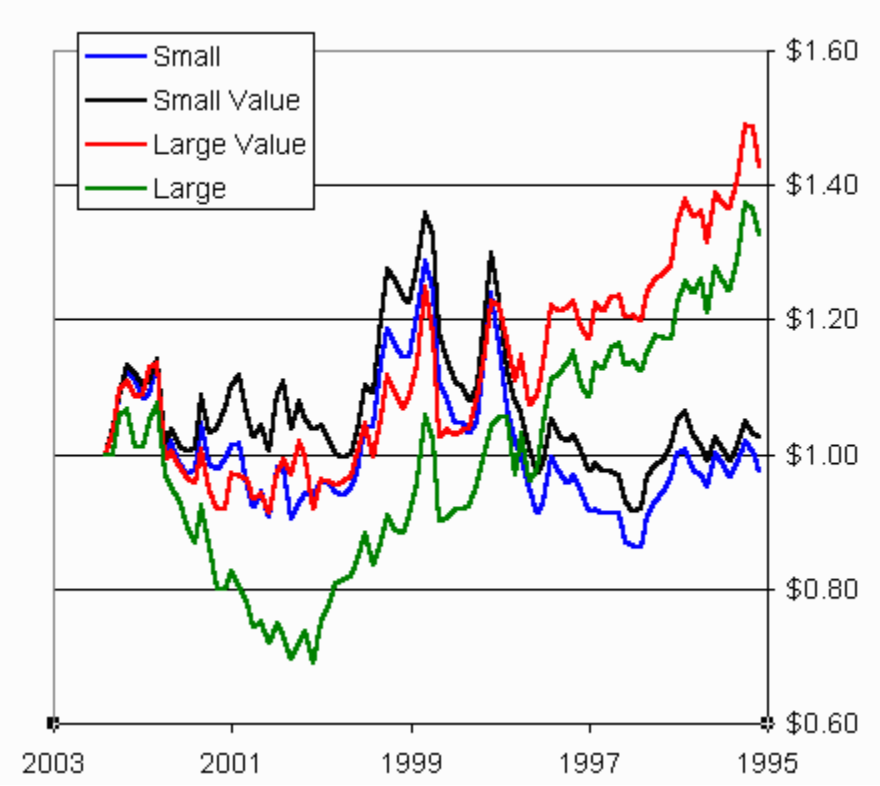
But what about the implementation objection: Can these factors be captured in real life? Yes, indeed. Looking back from the vantage point of April 30, 2002, there is no time period when the DFA U.S. Small Cap Value strategy has not been king of the hill. The below graph plots "backward-looking" returns from this date. The graph takes a bit of getting used to, but basically plots your return *to* April 30, 2002 from any given starting point. All of the four strategies listed (DFA does not run growth funds) are commercially available from them.



Extend the graph back before the fund inceptions in 1993 using theoretical data and the picture is even prettier:

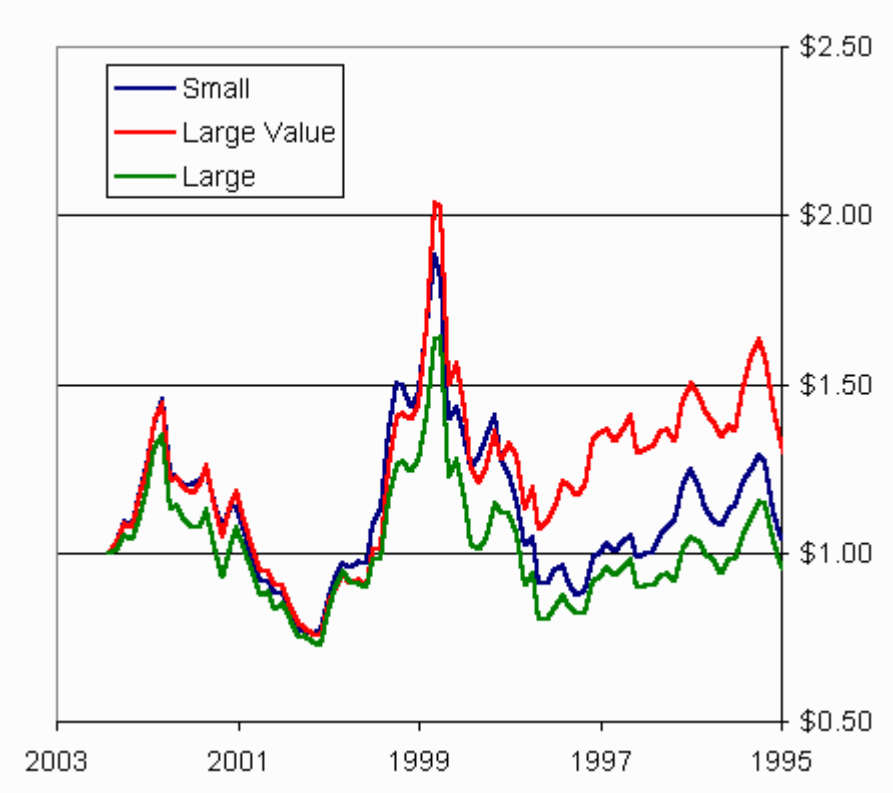


For foreign stocks, things aren't as agreeable. DFA has had all four international corners running since 1995. Value seems to hold up, but size does not:



If you use theoretical data and go back far enough, eventually small does provide a positive return, but you have to go back 15 years, to 1987, for this to occur.

Finally, in the emerging markets arena, with astronomical transactional costs, particularly for value and small stocks, the three-factor model seems to be defending its turf. DFA's plain-vanilla Emerging Markets fund incepted in 1994, their Emerging Markets Value (large-cap) fund began as an institutional portfolio in 1995, and the Emerging Markets Small Cap started in 1998. Here's how things look back to 1995 (with theoretical data used for the small-cap portfolio before 1998):



What's a small investor without access to DFA to do? For U.S. small-cap value, Vanguard Small-Cap Value Index Fund, although it includes a fair slug of "blend" in addition to "value," seems to track the DFA fund nicely and has nearly identical small and value loadings.

The same cannot be said for the Vanguard Value Index Fund (for U.S. large-cap value), which does not seem to capture the value factor very well and whose median market size is considerably greater than that of the DFA U.S. Large Value fund, which, truth be told, is really a midcap value fund. The lack of a retail U.S. large-cap value index fund has a relatively quick fix: the iShares Midcap 400 Value offering (an ETF, so be careful about dollar-cost or value averaging with this one).

The Vanguard International Value Fund also does a pretty good job of capturing the value factor return, though it is not an index fund. Since the 1994 inception of DFA International Value, the Vanguard offering (which is far older) tracks the DFA fund very closely and lags it by only 0.4% per year.

But if you want international or emerging-markets small or value exposure, you're out of luck. Fortunately, these make up a relatively small part of most investors' portfolios. I'd stay away from active funds in these areas for the usual reasons—tracking error and very high expenses.

So, after ten years, the three-factor approach, which overweights small size and particularly value, seems to be alive, kicking, and eminently doable, even for the small Vanguard-only investor. But, as the 1990s demonstrated, no small amount of patience may be required, and many fools may have to be suffered.

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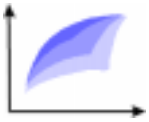
Efficient Frontier



William J. Bernstein

Link of the Month: When a Buyback Isn't a Buyback

So you thought that stock buybacks were aimed at enhancing shareholder value? Silly you. In this understated working paper, Kathleen Kahle turns over the buyback rock; paradigmistas won't like what she's found crawling underneath.

Note: You'll need the Acrobat Reader plugin to access this piece.

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