This book presents evidence that risk, in general, is not related to return. Assets on the extreme ends of the risk spectrum, however, those with hope and certainty, have lower-than-average returns. These results are a consequence of our benchmarking our wealth against our peers, of valuing opportunities that provide hope, and needing some amount of certainty as insurance. This implies that investors should focus on the expected returns, which includes anticipating the infrequent lean times, and ignore any nuance to the discount rate—which is pretty much what most investors do in practice. Furthermore, alpha is fundamentally private information, and so the main issues in finding alpha relate to the various issues that arise in negotiating with someone whose incentives are not directly aligned with yours, and of ascertaining the value of something most other people have dismissed.

There are two general areas one can apply these insights to that generate strategies and tactics that are unconventional. The first is avoiding assets with a large hope premium added into their price. To the extent one is benchmarking against the market, going long only assets without huge upside means that you are selling hope relative to the market. If you are long only boring assets that have little chance of doubling, your relative performance is enhanced to the extent the hope assets underperform, as they historically have. These are generally strategies that presume no alpha on one level, but like buying index funds versus active funds, they generate superior returns, with less risk however measured. A second set of recommendations comes from useful rules once we recognize the incentives and information asymmetries in alpha. As our intuition about risk and return comes from a rule that applies mainly to people finding their comparative advantage in life, our understanding of alpha informs our efforts to find our comparative advantage. Indeed, one could say a synonym for alpha is comparative advantage, because what you can do, better than anyone else, is where you have alpha.
MINIMUM VOLATILITY PORTFOLIO

The first, most obvious investing implication is to arbitrage the lack of a positive risk and return relation. Risk may be impossible to define academically, but it seems like less volatility should be preferred to more, as was supposed in the Dark Ages before we found out the CAPM did not work. If you can trade your envy for greed, you should prefer a higher Sharpe ratio, and targeting low volatility stocks is the easiest way to do this.

After finishing my dissertation in 1994 documenting the slight negative relation between volatility and returns, I was eager to apply this in an investment vehicle. The basic idea was simple: Buy low volatility assets, make the same return if not slightly better than the indexes, and generate a higher Sharpe ratio. I figured the attractiveness of this idea was like sliced bread. I went to work at KeyCorp, primarily because of the opportunity to work with the asset management group there, which I was certain would be very excited about this great idea. In short order, I found no enthusiasm, because it was a little too unconventional. People were not interested in pitching a strategy that promised not so much higher returns within an asset class, but mainly lower risk. In the context of relative risk, and how people in general define themselves relative to a benchmark, this makes sense. That is, as described in Chapter 9, investors tend to think of risk as tracking error, deviating from the S&P500 or any relevant index is risk, regardless of whether the total volatility, or covariances with the market, or the business cycle. An equity investor with less volatility, but a tracking error with the benchmarks, is riskier to most investors.

I think a strong case can be made that while people measure risk relative to benchmarks, they should measure it more objectively, relative to volatility. The origin of the CAPM is incorrect as a description of reality, but it is a good guide as to how one ought to invest. In the long run, a higher Sharpe ratio implies that one can take on equivalent volatility and generate higher returns, and all that benchmark risk is idiosyncratic: over time it washes away through time diversification. Thus, over a long period, the Sharpe ratio should be the primary metric for developed country equity investing because in general these portfolios have relatively symmetric distributions (unlike debt or options).

While I managed my personal situation by becoming a risk manager, I also set up a fund with a couple of hundred thousand dollars, primarily my parents' money, and set up an S-Corp called the Falken Fund to invest the money. I would invest the money, generate audited returns, and this I thought would help my efforts to someday land a job implementing the strategy. Like so many, I added to the basic idea, in my case by targeting low volatility stocks another factor, taking low volatility stocks that had high
momentum, which in the 1990s was a relatively new anomaly that seemed promising. As I had a rather limited amount of funds, I could buy only about 30 stocks, which implied considerable idiosyncratic risk remained in the portfolio, but I was hoping the momentum and low volatility focus would generate higher returns at low risk. I rebalanced this every 6 to 12 months, implementing a very simple filter rule that took little time, and did this on the side while working on risk management issues for KeyCorp and Moody's (I got okays from senior management at those institutions, noting it involved no real outside money and none of my daily time at work). I stopped doing it once I got into a situation to be a real portfolio manager at Deephaven, because I wanted to focus then on my directly competing day job.

The strategy performed very well, basically by outperforming in the tech bubble period of 2000 to 2002. From December 1996 through July 2002, it generated a 16 percent annualized return, with a beta of 0.8, versus a 3.8 percent return for the S&P500 over that same period.

I mention this not to brag, but because it highlights that the idea that focusing on low volatility is truly an out-of-sample result. In addition to my dissertation, I actively implemented a strategy fundamentally based on this insight. I am not looking at the past 10 years of data with hindsight, the recent literature on the latest anomaly, and making a grand statement. It is based on a pattern in the historical data, going back to 1926, which I discovered in 1992, and emphasized continually since. Furthermore, I was involved in litigation for almost two years, where one of the demands by my former employer was that I not use volatility as a factor in evaluating stocks, as this was asserted as being necessarily derived from my work at this hedge fund. I discovered, wrote about, and implemented this idea well prior to any work I did after 2003. None of this originated, nor was derived from any ex-employer's trade secrets or confidential information. While I currently work in equities implementing long and short equity strategies, the basic idea of my 1990s Falken Fund was an edge based on the superior Sharpe to low volatility equities. This insight, however, is not suited toward hedge funds, where the desired Sharpe ratio is above a 1.0. Thus, it is not like pairs, but rather more like index investing, a modest improvement but highly scalable, long-only approach.

Researchers have investigated minimum variance funds since Haugen and Baker in 1991. All document that a straightforward variance minimization algorithm applied to a large set of stocks, including a no-short-sales constraint, generates about a 30 percent reduction in volatility compared to common U.S. indexes without diminishing returns. These studies had issues, in that only an obscure unpublished piece by Tal Schwartz used a set of stocks that underlay the indexes, which complicates the analysis because it is possible that using a broader set of data to compare to an index create a
low volatility portfolio spanning a different set of risk factors, making it an apples-and-oranges comparison.

The long-only, minimum-variance portfolio is an attractive target because it makes no assumptions about returns, merely portfolio volatility. If traditional measures of risk are unrelated to returns, this should be a straightforward way to increase a Sharpe ratio. Curiously, all the papers that examined minimizing index volatility also found that returns were actually higher than the benchmarks. In each case, the higher return was unremarked, as if it had to be some sort of mistake, merely emphasizing the lower volatility feasible in a minimum variance focus.

I denote these long-only, low volatility portfolios MVPs, for Minimum Variance Portfolios. New index weights are calculated for each on January 1 and July 1 based on the prior year’s daily returns, using a minimum variance minimization algorithm on the factors and factor loadings from Chris Jones’s heteroskedasticity-consistent version of Connor and Korajczyk’s principal components procedure. Index weights are then constant for the next six months using a total return index, so there is no rebalancing bias due to equal-weighting daily returns of portfolio constituents. There is no survivorship bias because I use stocks in the indexes at the beginning of the performance period, transaction costs are low, and liquidity issues should be minor because we are only going long stocks within the major equity indexes. Thus, unlike the Fama-French size (SMB) portfolio, with its many illiquid stocks and shorted securities, the January returns for the MVPs are actually slightly less than their sample average, suggesting there are no significant institutional issues due to end-of-year tax strategies that show up in illiquid securities.

Minimum Variance Portfolios are prominent in advanced textbooks on portfolio theory, because they form the leftmost point in the convex hull representing the set of feasible returns in risk-return space. Theoretically, they include long and short positions in stocks. The long-only approach of an MVP is attractive because many obvious stocks we all wish to short (for example, Palm in the 3M spin-off of 1999) in practice cannot be shorted, or have a negative rebate (that is, instead of earning interest on the short sale proceed; you pay a fee on the money you generate). Rebate schedules and impossible-to-borrow lists are difficult to accurately recreate because these are over-the-counter markets. Furthermore, in many developed markets, one can short only 40 percent of the stocks, and proportionately more of the weak stocks everyone wishes to short (for example, in January 2009 highly liquid General Motors was basically impossible to short for retail investors). This makes results difficult to interpret, because one can never be certain it was feasible to implement the short side of a portfolio-minimizing algorithm. Jagannathan and Ma (2003) show that in constructing a global minimum
variance portfolio, a no-short-sales constraint actually helps out-of-sample performance because in an unconstrained approach, recommended shorts usually have very high covariances with other stocks. Stocks that have extremely high covariances with other stocks tend to receive negative portfolio weights, and the no-short-sales constraint is equivalent to capping the sample covariances at reasonable level (alternatively, one can think of it as applying a Bayesian base rate to a covariance estimate that mitigates extreme correlations in sample). Hence, to the extent that high estimated covariances are more likely to be caused by upward-biased estimation error, imposing the nonnegativity constraint on position weights reduces the sampling error, and so a no-short-sales restriction is in practice a modest constraint in constructing a minimum variance portfolio, and makes the result something eminently feasible.

Looking at the past 10 years of data for the S&P500, eight years of data for the Nikkei and FTSE, and seven years for the MSCI-Euro Index, Table 13.1 shows that annualized volatility can be reduced by 30 to 45 percent within these indexes by simply reweighting the constituents in a way that minimizes the historical volatility, but is then applied to the out-of-sample returns. Furthermore, in each case, the numerator of the Sharpe ratio was also significantly higher. The FTSE, Nikkei, S&P500, and MSCI-Euro index would need annualized return increases of several percentage points to

<table>
<thead>
<tr>
<th></th>
<th>FTSE-MVP</th>
<th>FTSE</th>
<th>Nikkei-MVP</th>
<th>Nikkei</th>
<th>SP500-MVP</th>
<th>SP500</th>
<th>MSCI-Euro-MVP</th>
<th>MSCI-Euro</th>
</tr>
</thead>
<tbody>
<tr>
<td>AnnRet</td>
<td>5.28%</td>
<td>-3.82%</td>
<td>1.53%</td>
<td>-4.35%</td>
<td>6.58%</td>
<td>-0.58%</td>
<td>-0.30%</td>
<td>-5.39%</td>
</tr>
<tr>
<td>AnnStdv</td>
<td>15.45%</td>
<td>21.11%</td>
<td>16.90%</td>
<td>25.37%</td>
<td>14.47%</td>
<td>20.91%</td>
<td>14.09%</td>
<td>23.38%</td>
</tr>
<tr>
<td>Beta</td>
<td>0.67</td>
<td>1.00</td>
<td>0.57</td>
<td>1.00</td>
<td>0.55</td>
<td>1.00</td>
<td>0.52</td>
<td>1.00</td>
</tr>
<tr>
<td>Sharpe</td>
<td>0.04</td>
<td>-0.40</td>
<td>0.07</td>
<td>-0.18</td>
<td>0.19</td>
<td>-0.21</td>
<td>-0.19</td>
<td>-0.36</td>
</tr>
</tbody>
</table>

SP500-MVP, Nikkei-MVP, MSCI-MVP, and FTSE-MVP are the Minimum Variance Portfolios of the S&P500, Nikkei 225, MSCI-Euro, and FTSE 100 indexes. Each index is formed at the end of June and December using a long-only subset of the corresponding index, where portfolio weights minimize the portfolio volatility over the prior year's daily returns. Beta is from regressing the daily returns of the MVP on its respective index. The Nikkei and Nikkei-MVP, FTSE, and FTSE-MVP use daily data from January 1 to December 31, 2008, while the SP500-MVP and S&P500 use daily data from January 1, 1998, to December 31, 2008, and the MSCI-related indexes are from January 2, 2002, to December 31, 2008. The Sharpe ratio subtracts the average one-month LIBOR rate for each currency from the annual return, divided by the annualized volatility of returns.
equalize the Sharpe ratios of their relevant MVPs. For example, the S&P500 has an annualized geometric return of almost zero, –0.6% percent, over the past 10 years, whereas the minimum variance subset portfolio generated a 6.58 percent annualized return over that same period. As the volatility of the S&P is about 21 percent versus 14 percent for the MVP, in a Sharpe ratio perspective, the S&P return would have to be about 10 percent higher, annually, to be comparable to the minimum variance portfolio for your mean-variance maximizing investor. Betas of the MVPs range from 0.52 to 0.67. Thus, as index funds dominate actively managed funds primarily because of their 1 percent cost advantage, the relevant advantage here is several orders of magnitudes higher.

The returns in these samples are for geometric returns, and about 2 percent lower than the returns generated from an arithmetic averaging of the monthly returns. This is appropriate, I would argue, because this kind of strategy is a long-run strategy, not a market timing one, and so, the idea is over a long period of time, say 10 years, what were the returns. But the results are qualitatively similar using arithmetic returns. Looking at Figure 13.1, we see that the total return to the average of all the MVPs since

![Figure 13.1](image)

**FIGURE 13.1** WorldWide Equity Index versus Minimum Variance Subsets of Indexes

*Note: Total return data are averages of index, and minimum variance portfolios described in Table 13.1.*
2001 versus the average of the indexes shows the obvious domination of this approach: higher return, at lower volatility, lower cyclicality.

This type of return dominance is especially obvious if one uses the harsh criterion of Second Order Stochastic Domination (SOSD). This criterion was highlighted in the early 1970s, when high-brow theorists like Rothschild and Stiglitz were defining just what, exactly, was risk. One idea was that it was broader than mere standard deviation, as obviously returns with fatter tails, or more negative skew, might be perceived differently from assets with less. Standard deviation is often criticized for being incomplete, as if finance is ignorant that tail risk is important. Yet economists have known of this for decades, and indeed the Rothschild and Stiglitz work in this area tried to come up with better, broader risk measures (that they did not really take off highlights that these more accurate measures have problems, mainly they have too many dimensions and are unintuitive). But all you need to know is that the SOSD metric requires a strictly increasing utility function and global risk aversion, that is, utility is everywhere increasing and concave, two standard assumptions that underlie most risk models. Every risk-averse agent prefers an asset that SOSD another, in this framework. To put this idea in more concrete terms, see Table 13.2. Note the first, fifth, tenth percentiles, and the worst drawdown, for the MVPs are all higher than for their respective indexes. This means that the extreme bad days tend to be much worse for the indexes than for the MVPs. This is tail risk, something that is occluded by a metric like standard deviation that is sufficient only for normal distributions.

If the most widely used equity indexes can be significantly dominated by simply applying the idea that risk is not rewarded using its very constituents, this suggests the failure of the CAPM and its extensions is not a mere academic finding in an abstruse statistical test, but something tangible to regular investors. For those demanding concrete proof that their standard approach works, in general, they should explain the Minimum Volatility Portfolio anomalies, because forming a portfolio based on the most conspicuous, and earliest, metrics of risk, is not data mining the way book-market

<table>
<thead>
<tr>
<th>TABLE 13.2</th>
<th>Extreme Drawdown for Minimum Variance Portfolios and Their Indexes</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>FTSE</td>
</tr>
<tr>
<td>0.01</td>
<td>-4.08%</td>
</tr>
<tr>
<td>0.05</td>
<td>-1.94%</td>
</tr>
<tr>
<td>0.1</td>
<td>-1.36%</td>
</tr>
<tr>
<td>MaxDraw</td>
<td>-48.11%</td>
</tr>
</tbody>
</table>
or momentum might be—these are stock characteristics that should sit at the top of one’s list of things to measure against returns. The returns to these seemingly low risk portfolios is above the indexes, and for those who are skeptical of alpha and appreciative of relatively small edges, this move should be most attractive to index investors. In Europe, Robeco and Unigestion have recently created funds explicitly targeting low volatility equities, and the only reason for them to not grow, would be if investors truly cannot trade their envy for greed. That is, those who can appreciate index funds relative to active mutual funds, should be able to appreciate MVPs.

**BETA ARBITRAGE**

It was mentioned in Chapter 4 that stock returns are negatively related to betas against the market index, and perhaps the best evidence of this is that no academic, who must withstand the barbs of conventional wisdom, presents beta-sorted portfolios for benchmarking because the cognitive dissonance would be too great. This is because beta at some level is positively correlated with risk, and the higher return to the low beta portfolio over the high beta portfolio would seem to imply some kind of error in the data. This is wishful thinking. In addition to the total absence of a positive beta-return relationship after controlling for size, consider all the things that would artificially increase the returns to beta that were generally unremarked, because no one likes to pick on a weak theory: the effect of variance on geometric returns, which biases high beta returns upward, or the biased effect of delisting return omissions on highly volatile stocks. Were the bias obstructing the CAPM, they would certainly be first-order adjustments, but as they merely make matters worse, these adjustments are rarely made. Another incidental confirmation of the negative return to beta, is that most anomalies in the literature—accruals, momentum, value—have lower betas on the higher returning stocks. Supposedly, to the extent the higher returning stocks have some risk, such risk is inversely correlated to beta. Again, there is little made of this, because “no guts, no glory—on average” is a pillar of finance that is seemingly impervious to empirical rejection for those currently in academia who made their career writing papers infused with this basic principle.

The investment implications of the failure of beta are fairly straightforward. Assume there exists an equity return above the risk-free rate. For a trader without alpha, this is presumably a necessary condition for investing in equity markets, with their higher volatility and covariance with the business cycle. Assume also that the traditional CAPM, with betas formed against the market return, does not work. Given these assumptions, you can generate the same return as the market, with zero beta.
Consider:

- Expected equity risk premium (above the risk-free rate): 3 percent
- Beta for longs: 0.5
- Beta for shorts: 1.5
- $R_p = R_m$ for all $\beta$

With these assumptions for what is available in the market, we can generate the exact same return as the market, yet without any beta risk. Just go long 1.5 units of the low beta portfolio (for example, beta 0.5), and go short 0.5 units of the high beta portfolio (beta 1.5). The net result is zero beta (dollar beta of the long and short side are both 0.75). Note that this employs 2.0 units of stock, thus nicely fitting into Reg T, an U.S. regulatory rule that prevents retail investors from having the gross amount of longs and shorts more than twice one’s capital. Perhaps someone, someday, will invent a high beta and low beta ETF (as opposed to leveraging the S&P500), and this trade will be made much easier.

What makes this trade even better, is that on average the low volatility longs have a higher-than-average return, and the high volatility shorts have a lower-than-average return, which means that one should expect a higher-than-market return, with zero beta.

I implemented a similar strategy in the United States over the past 10 years, investing in low beta stocks, hedging with high beta stocks such that one is long one unit of stock at zero beta:

\[
p_l - p_s = 1 \quad \text{net long 1 unit}
\]

\[
p_l \beta_l - p_s \beta_s = 0 \quad \text{zero beta}
\]

\[
\Rightarrow p_s = \frac{\beta_l}{\beta_s - \beta_l}, \quad p_l = 1 + p_s
\]

$p_l =$ position in long portfolio

$p_s =$ position in short portfolio

$\beta_l =$ beta of long portfolio

$\beta_s =$ beta of short portfolio

With these mathematical conditions, we estimate the beta of the portfolio using a rolling estimate of the past year’s returns on the low and high beta portfolio returns (that is, using only data prior to date $t$, for implementation on date $t$). The beta portfolios are constructed every six months, looking for the lowest and highest beta equities (100 stocks) in the United
States that had over $1 billion market cap, where beta was estimated over the prior year’s daily returns. Thus, this is an out-of-sample performance of the basic strategy outlined: long a unit of stock (for example, a dollar), at zero beta, by going short a sufficient amount of the high beta stock to make the portfolio’s total beta zero, while having one unit of stock long. The results are shown in Figure 13.2 and Table 13.3.

We can see that if you want $1 million exposure to the market, but with a zero beta, you actually do generate a higher return by about 4 percent, with a lower standard deviation than the market. Plus, you have zero beta, which most investors consider a good thing (back in the 1970s, some would call this riskless investing). The total return chart for the S&P 500 versus this alternative is shown, and the relative outperformance seems to be when it is appreciated most, when the market is tanking.

**TABLE 13.3** Summary Statistics for Beta Arbitrage Strategy, April 1998–December 2008

<table>
<thead>
<tr>
<th></th>
<th>S&amp;P500</th>
<th>Zero-Beta/Long-Market</th>
</tr>
</thead>
<tbody>
<tr>
<td>Annual Return</td>
<td>1.10%</td>
<td>4.30%</td>
</tr>
<tr>
<td>Annual Standard Deviation</td>
<td>21.27%</td>
<td>17.40%</td>
</tr>
<tr>
<td>GeoRet</td>
<td>−1.19%</td>
<td>2.92%</td>
</tr>
<tr>
<td>Sharpe</td>
<td>−0.21%</td>
<td>−0.02%</td>
</tr>
<tr>
<td>Beta</td>
<td>1.0</td>
<td>0.0</td>
</tr>
</tbody>
</table>
The beta arbitrage portfolio seems like a no-brainer for anyone who appreciates the statistics of mean-variance optimization. It takes no special insight, just recognizing that the CAPM should work, but doesn't.

There are many ways to take advantage of the absence of a beta relation with returns, because for a rational, nonenvious investor, the fact that the CAPM does not work is more interesting than if it did. An alternative is a strategy that directly targets a 1.0 beta, but avoids the high-beta assets that tend to have low returns. This also closely matches the beta of the overall market, and so plays into investor preferences to match the market.

Using data on all stocks above the median market capitalization since July 1962 to ensure an investable sample, I calculated betas to form a portfolio with a target 1.0 beta, by using those 100 stocks with betas most near 1.0. I call this the Trimmed Beta 1.0 portfolio, because it trims off those stocks with very high and very low betas. The mean return to this approach, from 1962–2007, is 14.0 percent, 2.8 percent above the value-weighted market return over that period (see Table 13.4). In comparison, the well-known value portfolio constructed and maintained by Kenneth French, applied to stocks with above-the-median market cap, generated a 14.6 percent annual return.

From an institutional perspective, a Trimmed Beta 1.0 portfolio is attractive if we put it into context. Many institutions have targeted index funds because they generate a cost savings relative to actively managed funds, and this cost savings is slightly less than 1.0 percent in annualized return, yet still approximating the market. Many funds tilt toward things like size and value to achieve an extra 2 to 4 percent lift while being consistent with the market return. In contrast, this strategy takes no bets on value or size, and thus has very little tracking error, and merely plays on the fact that hope and safety stocks generate lower-than-average returns. Clearly, I consider this an attractive investment because of the simultaneous higher return, but target on the market (low benchmark risk), which is something investors really like. Looking at Figure 13.4, we see the outperformance of the value portfolio and the Trimmed Beta 1.0 portfolio, and note that the Trimmed

<table>
<thead>
<tr>
<th>TABLE 13.4</th>
<th>Returns from 1962 to 2007</th>
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<tbody>
<tr>
<td></td>
<td>Large-Cap Value</td>
</tr>
<tr>
<td>Annual Return</td>
<td>14.6%</td>
</tr>
<tr>
<td>Annual Standard Deviation</td>
<td>15.1%</td>
</tr>
<tr>
<td>Beta</td>
<td>0.86</td>
</tr>
</tbody>
</table>
Beta portfolio has fewer extreme periods of underperformance. Further, it tends to underperform when the market is booming, such as in 1998 and 1999, while outperforming in periods of market declines, like 1981 and 2000. Thus, it is somewhat countercyclical. I have a patent pending.

**INVESTING IN ANOMALIES**

It may be thought that if risk is not, in general, positively associated with returns, then all the prominent anomalies are free lunches: higher return with no more risk. The problem, however, is that most anomalies are artifacts of the publication process, and not really there. Remember that when Bill Schwert wrote on the new size effect back in 1983, he mentioned the other prominent anomalies of his day, the calendar anomalies, specifically the weekend effect and the January effect. These have disappeared, as the findings were primarily a result of low-priced stocks that are large in number, and generate really large returns (for example, moving from one-half to three-fourths in price is a huge percentage move). In practice, these effects cannot be captured, because one actually pays the same large spread that underlies the apparent returns. That is, the bid-ask bounce in these
low-priced stocks is a big part of their apparently large returns, but also, their cost to investors. In the 1980s, low-priced stock funds were introduced with the same vigor as small-sized stock funds, but these have disappeared as it became apparent that low price does not reflect a factor or investor bias so much as measurement error.

Consider that for almost a decade, a prominent anomaly was documented by De Bondt and Thaler in 1985, showing that for each year since 1933, going long a portfolio of extreme losers over the previous three years, and short extreme winners over the previous three years, would generate an 8 percent annualized return over the next three years. This was published in the *Journal of Finance*, the leading finance journal, and followed up in 1987 with more results. It seemed like investors were engaging in a simple case of overextrapolation of recent times. Alas, in 1993, Conrad and Kaul (1993) demonstrated that if you did not arithmetically average the returns, the result disappeared, highlighting that this too was a result of low-priced stocks, and their tendency to bias results by moving from their bid to ask price. Indeed, the more prominent “past return anomaly” is actually the opposite of mean-reversion—momentum, first documented in 1992, so it appears that if anything, this theme would have generated low returns if actually implemented.

So, for an investor not plugged in to the latest research, one could easily imagine buying stocks based on flawed research, in that it usually takes 5 to 10 years for the academy to find the problem, and then another five years for this to become conventional wisdom. Currently, an investment bank distributes a monthly review of quant strategies, and lists about 30 different approaches: dividend yield, percent off 52-week high, recommendation changes, and so on. There is always much discussion about the factor rotation, as the latest hot factor approach is always different, which one would expect if most of these are random groupings. On average this is a total waste. As most anomalies do not stand up to scrutiny, this means, on average, you will be wasting resources in a bad investment.

Now, some anomalies, specifically value and size, are so common in theory and practice, one might exempt them. Indeed, the value effect was initially articulated by famous value investor Benjamin Graham in the 1930s, later discussed often by his most famous disciple, Warren Buffett, and lastly, the academics started referencing this effect, starting with Sanjoy Basu in 1977. It has been known for a long time, and seems to be pretty consistent in the data, generating a higher return with no obviously higher risk. But while some investors have become very famous plying this strategy, value funds have not been more successful than growth funds since records of this distinction were made. Houge and Loughran report that small cap value funds realized insignificantly lower annual returns than small cap growth
funds: 14.1 percent versus 14.5 percent over the 1975-to-2002 period. Value managers, as a group, were identical with growth-oriented funds, on average.10

The original Standard & Poor’s 500–Barra value index (Now Standard & Poor’s 500–Citigroup index) has generated about the same returns as their growth index over the period from January 1975 through September 2008, which has been about the same as for the S&P500 (9.7 percent, 10.0 percent, and 9.8 percent, annualized, respectively).11 Furthermore, this index was created around 1990, and they reached backward to build a historical track record for the data. As this took a lot of work, they decided to go back only to 1975, in part because they knew that the value effect was strong in the late 1970s but not early 1970s, implying a look-back bias to this index. Thus, in practice, the value effect is subject to the selection biases all anomalies face.

I believe there are patterns that are useful for statistical equity investing, but as noted in the discussion of alpha, one must have good reason to suspect one’s own alpha is complementary to these issues. Just as the main problem with taking advice is knowing what advice to take, knowing which anomalies are good requires almost as much skill as those who document them. Even the most prominent anomalies, ex post, are not all that prominent; ex ante your odds are pretty close to 50–50. If you have data and like to analyze it statistically, understanding the various parochial issues involved, it can be rewarding (it is something I do); however, as a general rule, jumping on anomalies in a diversified portfolio is a waste of time.

SAFETY INVESTING

It makes sense for someone to wish that some of their investment portfolio be as close to zero volatility in real terms as possible. Many traders, who are big risk takers, put much of their savings in extremely safe investments, and this makes sense because their careers are so highly correlated with the market that their savings actually constitute a small part of their aggregate wealth. One of the principles of the Kelly Criterion for maximizing the logarithm of one’s long-term wealth is that the probability of going into default should be zero. This can be thought of as the negative infinite utility payoff, or the bankruptcy state. If you continuously have access to your account, this means you bet less and less as the market volatility increases. Alternatively, it means you never bet your entire savings. Thus, if having 10 or 30 percent in riskless assets satisfies this basic need, it seems a reasonable objective.

But it can be overdone. People crave certainty, and risk is evaluated on a log scale, so people generally assume it must be the least risky thing possible.
Yet only a modest deviation from the most risky asset generates a sizable lift of 50–100 basis points. Now, when considering reaching for this lift, it is a matter of perspective. Consider the following moves:

<table>
<thead>
<tr>
<th>Super Safe Investment</th>
<th>Slightly Less Safe Alternative</th>
<th>Average Return Lift</th>
</tr>
</thead>
<tbody>
<tr>
<td>AAA bonds</td>
<td>BBB bonds</td>
<td>1.2%(^{12})</td>
</tr>
<tr>
<td>3-Month U.S. T-bill</td>
<td>1-Year U.S. T-bill</td>
<td>1.04%(^{13})</td>
</tr>
</tbody>
</table>

Are these worth the risk? With assets this safe, their variability is so low that demonstrating assets in these classes is like proving you are very good at evaluating your skills at forecasting earthquakes. As these happen infrequently, it takes many years to accumulate data that would allow one to see if one is truly good, because the mode signal for everyone, good and bad, is the same. Playing the yield curve from 3 months to 12 months in maturity for a cash account, or from an AAA bond to a BBB bond, the signal-to-noise ratio on any market timing is so low, at annual frequencies, as to be nonexistent. The time needed to even make a reasonable argument that one has alpha in picking among A-rated bonds is probably longer than one’s working life.

It is all, the question of whether 100 basis points in annual return is worth the extra uncertainty, quantified as higher volatility, or the size of the negative downside (asymmetric tail risk), classic quantitative risk preferences. The lift in question is not insignificant, but not life changing. I think this choice does fall into the strict mean-variance optimization problem that underlies Modern Portfolio Theory, applicable because there is no alpha here, just a statistical trade-off.

In addition to having some rock-solid investments, and perhaps minor stretches in risk from these (for example, going from overnight to 12-month risk on cash balance), having exposure to broadly diversified, low-cost asset classes also fulfills a fundamental investor desire. By being equally weighted among conventional assets, you mitigate your benchmark risk. Thus, having some money in real estate (their home), and the stock market, is a good idea, because everyone else has their money there too, and broadly speaking, you need to consider your relative risk. Consider if you lived in Southern California in the early 1970s, and decided to rent instead of buy real estate, because the Sharpe ratios on the perceived equity premium puzzle (you identified decades before Mehra and Prescott) suggested equity investing was optimal. You may not be able to retire there because of the massive increase in the value of property over the next 40 years. For those who bought property, this was no big deal, but sadly, not everyone who grew up
there owned real estate. The absolute return lift, if implemented with an eye toward low costs (that is, low trading, low fees) should probably generate a modest return above riskless assets, but more importantly, will keep one in company with one’s peers, as these assets can trend wildly above historical norms over decades.

HOPE INVESTING

It is easy to say, “Do not gamble” because this has a poor average return. But many people gamble anyway, either out of ignorance, or a real preference for taking on risks that promise extraordinary returns. At the very least, understand that investing in an asset hoping for some life-changing largess, you should have some reason to think you know better than everyone else why, say, a certain asset is undervalued. The riskier the asset considered, the lower your return, because on average, such assets have very low returns. So, you need alpha, to see which advice is good. Now, you might think you have a high IQ, or an MBA, which combined with this advice, makes for wise investing. This too is an insufficient reason, because these same professionals, from good schools, with high IQs, are average stock pickers.

As mentioned, I believe hope is a very useful motivation, but I think it should always guide some effort to get one’s hands dirty building a strategy, or choosing a career. Finding alpha passively, by applying statistics to a set of returns, is highly improbable, and should be discouraged or channeled into something else. Thus, I offer the following salve for those who need a little hope in their passive portfolios.

Group all your investing that is done without a general long-term strategy, but has high putative returns, as investing on hope. People have a need for hopeful investments the way we need trace elements called vitamins. So, one might wish to allocate a small portion of one’s portfolio to this. Thus, to the extent one should passively invest in dreams of winning it big, the cheapest way is probably the lottery. You can still dream of the fantastic new life you will have if luck smiles upon you. If you only invest once a week, it’s merely $52 a year. The odds are really irrelevant, you are paying to dream, and your dreams are not impossible, just so improbable, your intuition does not notice the extra zeros. Our lives are sufficiently short that an investment with a probability of 1 in 100,000, when played weekly, is observationally equivalent to one with odds of 1 in 100 million.

The problem with allocating part of a portfolio to passive hope investing in financial markets is that the upside is a mere 50 to 200 percent; you need to allocate 30 to 50 percent of your wealth for this to materially change your spending habits, to play into your dreams, and that is too much. Losing
3 percent annually on the 30 percent of your wealth is much more expensive than a weekly lotto ticket.

That means, understand the odds are highly against you in the following:

- Options, especially out-of-the-money options
- Junk bonds
- Highly volatile, beta, stocks
- Highly leveraged company stocks
- Stocks with junk bond ratings

These are classic hope assets that have lower-than-average returns for their class. You are paying for dreams, and it is too costly relative to lotteries because while the returns to these are better than the lottery, the opportunity cost is much greater. And as all you are buying are dreams, and these are passive dreams, not dreams possibly related to some individual alpha, the actual returns do not matter, just having a positive probability of reaching them.

RELATIVE RISK AND BUBBLES

It is common for people to state that bubbles exist, and financial theorists have good models of them. It has been variously suggested that financial bubbles may be endogenous, yet to date, there is no widely accepted theory to explain their occurrence. Take the famous Hyman Minsky Financial Instability Hypothesis: At first, people invest based on the cash flow of the investment, and are able to pay back their investment with principal and interest. Later, the investors still invest only in positive net present value investments, but need to use, in effect, interest-only loans. Lastly, they are engaged in financing by the “greater fool theory,” and need fresh stupid investors to cash them out, though at any time the scam will come crashing down. The problem is that these later investors seem to be quite irrational. They either cannot calculate a net present value, or are willing to assume that another fool greater than themselves will come in after them (as in classic pyramid or Ponzi schemes).

There is a large literature on these models, which attempt to generate the famous bubbles we observe in history—the tulip bubble, the South Sea bubble, the Internet bubble—but fundamentally contain someone making a dumb investment at the near-end of the cycle. Modeling bubbles is a highly difficult problem, because invariably one sets up a specific information structure so that agents have bounded rationality. If they had rational expectations and infinite horizons, then the minute an asset went above
FINDING ALPHA

its fundamental value, such agents would short the asset. When agents are merely selfish, as opposed to mainly envious, one has to put large blinders on them to generate bubbles.

In the recent subprime boom, many investment banks were buying mortgages, in part because they learned these activities generated large profits for others. If you work for UBS, and tell your boss you have a great way to make money, and that JPMorgan, Citigroup, and Bear Stearns are doing it, you are halfway there. As Keynes noted, “Worldly wisdom teaches that it is better for reputation to fail conventionally than to succeed unconventionally.”

In a world of relative risk, the impetus can be much more straightforward and intuitive. If an asset is becoming popular, it may reach a stage where it creates a feedback loop, as greater popularity makes it desirable for investors who consider deviations from the consensus to be risky. Thus, popularity implies lower risk by itself, and people will demand more of it, which increases its popularity, starting even more people adding it to their portfolio as they try to emulate the benchmarks. Think of the case of the Internet in the late 1990s, when as absurd as the Internet stock’s valuation was, many investors were simply afraid of missing the boat, and got in just before the bubble burst. The later stages of the bubble were filled with investors who were investing merely to avoid missing out, because their relative performance was lagging even as the market in general was doing well on an absolute basis. The mechanism, though irrational for an investor with a classic utility function, with a relative utility function, it is rational.

CAPITAL FINDING ALPHA STRATEGIES

If your job is to find strategies with alpha, the first thing you need to know are the basic properties of assets you are potentially investing in. Most investment pitches can be eliminated because they are hardly feasible given base rate information on the asset in question. For example, you should know what the average returns are among the assets in question, their average annualized volatilities, and the cyclicity of a product. The equity risk premium puzzle is that if the return on equities is 5 percent above the risk-free rate, this implies a return that is too high relative to its volatility of around 17 percent on average, and thus a major theoretical puzzle, and empirical outlier (I think it’s actually a lot of measurement error, but that is a different issue). Therefore, a Sharpe ratio of 0.35 is very good. The average Sharpe ratio in alternative investments in their glory years, like convertible bond arbitrage, generated Sharpes above 1 but below 2. Thus, the odds any investment strategy has a prospective Sharpe above 2 are minute.
Just knowing this fact is very useful. I have met several hedge fund personnel, who seemingly have important jobs and large responsibilities, who want only strategies with Sharpe ratios above 2, or even above 3. This is like demanding your equity portfolio manager outperform the S&P500 by 10 percent a year. The only strategies they seed are those with backtests or testimonials that appear to have such success. Now, backtests are easy to game. If you have the data, you should know how to get the right answer, and rationalizing a successful backtest is usually rather easy. But any strategy that generates such a Sharpe ratio is invariably overfit, meaning it used accidental correlations that are not stable merely because one has to do this to generate such returns. Accepting strategies based on this standard are doomed to fail because overfit or fraudulent strategies do not work well out of sample.

Consider bonds, where there is a lot of foolish investing because the constraints are well documented, but it remains a perennial area of hope unconnected to reality. If one proposes that they have 4 percent return alpha through knowledge of credit risk within high yield corporate bonds, I am highly dubious. How can their edge be 4 percent annually, if the default rate among B-rated bonds is only 6 percent annually, implying that the expected loss rate among B-rated bonds is only 3 percent annually? If you bought only bonds that never defaulted, over the cycle you would have a 3 percent annual edge. Furthermore, transaction costs would be at least 1 percent. While a high yield edge of 2 percent can be levered, this just increases the return and standard deviation, not changing the Sharpe.

Thus, knowing some basic facts about the asset class’s properties generates a filter one can apply to potential strategies. Yet this was a crude filter, assuming that an investor can only be perfect, so the constraint is pretty generous. What about something more realistic? This is where the subtlety of having a deeper knowledge of the field really helps, because the more you know, the more you can dismiss bad ideas, and most new ideas are bad ideas.

I worked on bond models at Moody’s and therefore developed a sense as to the power of a Moody’s rating, in terms of predicting default, and also the power of a quantitative model. As mentioned, statistically, your average model that includes some measure of market leverage (market cap to debt) and equity volatility, will outperform a standard agency rating (for example, Aaa, B) when applied to nonfinancial companies. My current default model is a function of several variables, mainly a Merton distance-to-default measure, a measure of profitability, and a measure of leverage (available at defprob.com). It was fit to the data being presented here because prospectively, I want to use all the information I have. Now, comparing apples to apples and focusing on only bonds with agency ratings between B— and BBB+, we have data from 1997 through 2005, 1,892 companies and about
330 defaults. Looking at Figure 13.4, one can see that my model generates a lower default rate in the remaining pool of high yield bonds, when using the risk metric to exclude companies, based on an ordinal ranked, reflecting the greater power of my model, compared to the agency rating. In Figure 13.4, each point on the line is the default rate of the sample, annualized, if one excludes various proportions of the sample based on the default model in question. For example, the little straight dotted lines show that if you exclude the worst 20 percent of bonds rated between BBB and B, using both models, the average annual default rate over the next two years, using my Defprob metric generates a 0.49 percent default rate, whereas S&P ratings generates a higher 0.85 percent default rate.

The default rate of the included group starts at zero percent, because initially, only the very best from both models are included, and these rarely default. But then the default rate for both models rise to 1.93 percent, because when we include the entire sample, for both models, it must be identical.
Along the way, the difference in the default rate between the two default indicators maxes out at around 0.36 percent. Thus, there is about a 0.36 percent edge in my model, which was fit in this sample, and so should be somewhat of an upper bound. This is a much smaller bound to apply to a strategy, because it implies that a really smart investor in corporate high yield bonds has at best a slight 0.36 percent edge in default rates over the cycle. As recovery rates average 50 percent, this implies a 0.18 percent annual loss rate. I think my default model is very good, but like credit scoring, mainly in being easy, cheap, transparent, scalable, and as good as it gets, not because it generates preternatural default forecast ability.

I have received calls many times from people starting funds, hoping to generate a good Sharpe ratio, going long and short debt within high yield. Now, if an in-sample fit to a model generates a 0.18 percent loss difference relative to a realistic benchmark (the rating agencies), annualized, this is hardly a pillar of an investment strategy, as it is too slim an edge to really generate the kind of returns hedge fund investors are looking for. How one turns this into a 0.5+ Sharpe strategy, I would be interested in seeing, but at least at the outset I could say to the pitchman, what is your perceived long-only return advantage in long-only high-yield investing? Note that this useful information is very parochial. It involves having access to difficult-to-construct default information, and would probably never make a textbook because it is not a general idea: Corporate default models are of interest to a very small set of people. This again highlights that in practice, if you are evaluating or creating alpha, your valuable edge will be something not found in, or derived from, some fundamental financial principle, but rather, a specific knowledge in a very narrow area.

Many of the high yield arbitrage strategies I have seen are not well thought-out plans, and the players anticipated this (it was sincere delusion). In all the cases, I discovered that people pitching such performance were closet long-only strategists. That is, they would say they do capital structure arbitrage, seemingly buying senior subordinated medium-term notes, hedging with unsubordinated floating rate callable bonds in a similar sector. But in practice they merely went long bonds their buddies were selling; hedging a little, but only a little. In good times (90 percent of the time) it works great; over the cycle, it is a marginal strategy, meant for a long-only vehicle, not a hedge fund.

Another way to see the futility of using debt to create equity-like returns is to consider the average returns to AAA bonds is about 1 percent below BBB bonds. This is the credit spread puzzle most academics consider. The other puzzle, that the average return to a BBB bond portfolio has about the same return as for B bond portfolio over the cycle, before transaction costs, is usually unremarked. The return over Treasuries for BBB- to B-rated bonds
is about 2 percent annualized. Now, say you have $100 worth of B-rated bonds. You can turn them into AAA-rated bonds plus equity in various proportions given the magic of a collateralized default obligation (CDO). The much-maligned rating agencies make mistakes, but they are not stupid. They demand that the average loss rate, over a long period (about 10 years) is the same for the same rating, regardless of whether it is a tranche of the CDO, or part of the pool of securities that makes up the CDO.

The mathematics of the rating agencies demands that the expected loss for the senior, AAA-rated tranche in the CDO, have a 0.03 percent expected annual loss rate, basically zero. This is the first constraint in the exercise: How much subordination is feasible such that the senior piece has a 3-in-10,000 chance of defaulting? As bonds have only downside, this is very important, and it sets the stage for the next set of assumptions. What kind of yields, losses, and implied returns does this generate for the subordinated piece? Looking at Figure 13.5, we can see how the returns and losses must be transferred. If you have $100 worth of collateral in the underlying pool, you can get a decent estimate of the stated yield, the expected losses, and expected return for the B-rated bond, though yields vary considerably over the cycle. Assume the risk-free rate is zero to highlight the math needed to evaluate this structure. We know that the losses will annualize out at
3 percent per year for a B-rated bond portfolio, so we should subtract that from the stated yield to get the return expected. Thus, we have dollar amounts that mathematically must be allocated to the various tranches in the collateralized debt obligation that sits on top of it. Note that the AAA return for asset-backed securities such as CDOs is considerably higher than for corporate AAA bonds, and so with this we can assume that the total return, on average, is independent of the default risk (again, no risk-return relation on average; see Chapter 4).

Now, the senior piece is usually rated AAA, which has a zero expected loss rate (they can and do make mistakes, but this is the expectation). So, all the losses have to be put into the subordinated tranche. We see that given a 20 percent subordinated piece, the loss rate would be 15 percent in this case! Thus, to generate a 2 percent return for these assets, it would need a 17 percent stated yield on the subordinated piece. As defaults are highly concentrated in stressful periods such as 1970, 1990, and 2001, this stress is exacerbated on high yield portfolios because the spreads on non-defaulted bonds increase too, making the downturns much worse than the increase in default. Therefore, looking at returns on high yield bonds, one must take pains to differentiate the mean from the mode, which is one of the most common, and most costly mistake investors make. Only a fool would look at a 17 percent stated yield in debt, and think they would generate a 17 percent return, given data on the returns to high yield and distressed debt over the past 30 years.

Alas, the nature of large organizations involves a lot of fools. American Express invested in subordinated CDO tranches, backed by high yield debt (then yielding about 3 to 4 percent above Treasuries) that had yields of 15 percent in the late 1990s, and these returns were expected by the company, prospectively. In other words, they made a huge bet based on the premise that a 15 percent return in high yield was feasible alpha. They later had to write down losses of more than $1 billion, as Chief Executive Officer Kenneth Chenault noted that “it is now apparent that our analysis of the portfolio did not fully comprehend the risk underlying these structures during a period of persistently high default rates.” As average market return for B- and BBB-rated securities is similar, the market offers no method to make water into wine here. The 15 percent expected return was a clear indication that they confused a stated yield with a return; thinking they were picking either the part of the business cycle, or subset of B-rated bonds, impervious to losses, both highly dubious claims. The failure in risk estimation was not in the discount rate, merely the default rate.

In the current subprime crisis, it appears that many investment banks had large portfolios of asset-backed securities of various kinds on their books, basically involved in attempts at fixed-income arbitrage. There are
small niches within the ABS universe that offer, I think, dominating returns (for example, mezzanine tranches on unrated commercial debt, because the adjustment factor for not having an agency rating actually penalizes them for having the more powerful, if nontraditional rating model applied to them!). But most of this universe should be held in long-only accounts at mutual fund complexes and pension funds. People who try to get rich on debt don’t realize that with the slim margins in this class, alpha can generate only a certain level of outperformance, and is insufficient for being attractive within a public corporation like a bank, or a hedge fund that promises 2 and 20 fees.

That almost all of the major financial institutions were exposed to mortgages that appear to be backed by highly imprudent lending standards, highlights two effects going on that are discussed in this book. First, by benchmarking against others, these investments appeared to be prudent risk-averse portfolio tactics, to keep up with the consensus to which they were compared. Second, the nature of alpha is such that many investors make assumptions that are validated by the behavior of others. Few people actually work through the math, and check the business model on their own, and instead defer to a track record or a comparable strategy’s historical performance. This is because those in charge of the business lines tend to be leaders, meaning, they are not the smartest people, but rather, the best at making people feel appreciated, and even if they were highly detail-oriented in their youth, they have not worked through a complex problem on their own in perhaps decades. They have, for their leadership position, an essential skill, but it leads to problems like the bubbles we observe.

The financial crisis of 2008 involved a situation where many large financial institutions were unable to demonstrate to investors that they were solvent. There were no financial institutions that tried to calm investor fears by giving a complete and thorough overview of all the mortgage exposures that were of concern by investors. It appears that they were all unable to value these securities from the bottom up. That is, mortgages have various characteristics like loan-to-value, FICO score, original balances, current balances. Securities built on these mortgage pools have characteristics like overcollateralization, and one needs to add these up. Now, if you own more than a billion dollars worth of these securities, it seems reasonable to have a group that can independently value these securities. That is, based on the performance of the underlying mortgages, what the value of your claims on those pools should be. This is complicated, but there should be people working full-time on this within the institution, and you can break the problem down in such a way that your average Wall Street analyst can gain comfort your estimates are correct to within, say, 10 percent. Instead, it seems no one did this, all comforted by the fact that everyone else was
doing the same thing, and in the panic many outside investors assumed large portions of bank portfolios were worth near zero. One would think that if it were at all possible to generate information on a bank’s exposures, outsiders could have been comforted. Yet it seems the leaders of these institutions did not have the ability to generate such basic information. A smarter approach would be to work out the math, and apply it, using standard stress tests, such as what happens when collateral prices go down as much as they went up over the past two years, hardly an unreasonable assumption (after all, anything that can go up X percent can go down X percent too).

Consider, in contrast, having been presented with the opportunity to invest in convertible arbitrage in the 1990s. One could actually do the math, looking at the implied credit spreads and volatility in the convertible bonds, comparing it to their long-term options and straight debt, and see that there is extra juice there. The only way to explain the prices of convertible bonds was to assume considerably more spread on the bond, or conversely considerably undervalued options. When hedged correctly against equity movements and interest rates, one could isolate this alpha, and reap the abnormal return with a modest volatility. The difference between the convertible bond arbitrage portfolio generating a 15 percent return and the CDO tranche generating 15 percent, is that one works, given a straightforward application of reasonable assumptions (in the case of convertibles, implied volatilities on long-term options, and spreads on their straight debt). This takes a very thorough knowledge of the assets class, because as Ed Thorpe discovered, his expertise in option pricing allowed him to be comfortable with valuing convertible bonds to a degree your average investor could not be.

Someone with a strategy will not be totally forthcoming, nor should you expect them to be. The only person who is going to give you alpha is someone who has not succeeded in convincing others he has alpha. The onus always comes back to you, however, and Bernie Madoff’s case highlights that those not willing to apply due diligence are ripe for fraud. When investing in alpha, remember that your alpha should be complimentary to what you are investing in. Oil does not have alpha. It has alpha only if oil futures, say, are a tactic, within an overall strategy of finding patterns, or evaluating people who find patterns, and this should be demonstrable by disciplined self-evaluation. If you are merely taking someone’s word for it, based on the person’s current wealth and status, you are a passive investor, and should expect people to take advantage of your ignorance.

Ex-professional athletes often lose their nest eggs because the combination of wealth, high testosterone, and modest intelligence makes them sheep to be shorn. Higher levels of testosterone are associated with higher risk taking, but this does not lead them to higher average returns as implied by
the CAPM, but rather, merely implies some wily con artist gets them to invest in either Ponzi schemes, trades where the mode is presented as the mean (the mode is positive, the mean zero), or simply a low-probability investment with a large payout that is mainly a way for the promoter to try to get rich quick using other people’s money, something that blows up. Generating good returns is not merely a function of luck and risk tolerance, rather luck, effort and intelligence. If you deviate from the consensus in your portfolio and take risk, you need to understand a lot about the specific assets or strategies you are considering, because you will otherwise adversely select strategies based on your ignorance. In investing, there are usually some asset classes with alpha, but these do not have bright seals of approval, and they are hidden, in plain sight, among many other opportunities that look similar but are merely gambles in a period of fortunate returns.

The only thing harder than finding strategies with alpha, alas, is finding capital when you have alpha. A really original good idea is rarely stolen, but rather has to be crammed down people’s throats. Most proposed ideas about alpha are like most books, songs, and movies: they stink. Thus, the skepticism you face is merely an accurately calibrated Bayesian predisposition, and you have to overcome this. To the degree the idea is straightforward, the more one says, the less attractive it appears, because simple strategies are not very convincing. Further, they can listen to your pitch, say, “No thanks,” and then develop the idea internally: Why buy the cow when you can get the milk for free? An alpha discoverer, looking for an investor, faces both skepticism and duplicity.

Given all the pitfalls, it highlights the importance of having trust, which means having good contacts, and a reputation for integrity. Building these attributes is just as important as any analytical work, because ideas do not succeed on their own.

SEARCH FOR ALPHA

Alpha is about finding a comparative advantage. Something you do, relatively, better than other people, to the best that you can. One must define alpha broadly, because most of us do not have daily profit-and-loss statements like portfolio managers do. For example, some can only recognize alpha, not create it; but then, at another level, that too is alpha. A person who selects portfolio managers, or edits a journal, or publishes books, has a useful and singular skill as an evaluator of alpha. It is important to know alpha comes in many forms, because most of us do not end up in classic creative roles such as being an artist, or running your own portfolio, but
creativity is essential for almost any job, which is why one cannot automate so many jobs that, to outsiders, seem insanely dull.

This is important because self-awareness is very important in making choices that maximize your potential. As most people will not discover Sharpe 1-plus strategies, they should focus on the little edge they do have. In general, you should approach risk taking mainly in nonfinancial avenues, in your career, in strategy, not in buying risky assets, per se. The best advice in this domain was probably noted by Casey Kasem in his weekly Top 40 countdown when I was a teenager: "Keep your feet on the ground, but keep reaching for the stars."

It’s essential to have dreams, but not delusions. Dreams make us happy, and motivate us to excel. Do not forfeit your dreams merely because when you act on them, you are generally wrong; just be realistic. This means stick to things you are good at, things you like doing, because those are the things where making that extra effort is costless because it is something you like doing. This is the risk taking that leads to greater returns.

On average, risk taking loses money after transaction costs when looked at in isolation, for example, actively managed mutual funds underperform the S&ampP500. One could say the same thing for people who spend time learning to write poetry: most waste everyone’s time, mostly theirs. But the option value in risk taking is outside the act alone, so look at risk taking as a meta strategy. How do you learn you are good at post-production processing of TV shows? By becoming a cameraman while trying to progress into a job as a writer. You try a bunch of things to find that career where you are worth the most. The return of your investment is never the direct payoff of any one thing, but from the self-knowledge and connections gained by getting one’s hands dirty. Much of success is dreaming about finding gold, and then discovering you can get rich selling shovels to gold miners. There are many examples of businesses founded on unique business selling points that, with hindsight, were wrong. This is the one thing ignorant but ambitious young people have that their more knowledgeable and older colleagues are envious of. Young people have the time and energy to discover that older people do not, but this assumes one actually invests this time and energy doing things, and does not just talk about them. In searching for alpha, you often have dreams that are often ill-founded, but they can actually be beneficial, because they offset the general underappreciation of the option value of trying things and then learning an incidental skill that introduces you to new opportunities.

You have niches in the world, most where you make $100K a year, a few where you make $150K a year, the latter being the ones where you have alpha. The present value of finding that niche, discounted at 5 percent per
year, is about $1 million, so if you spend a lot of money and time trying things that do not work out, it still makes sense. You should take some risk in this dimension, rather than stick with the first opportunity offered to you, if only because it is unlikely this would have your optimal alpha. As you get older the length of your future career shortens, and your opportunity cost increases because your salary presumably increases, so one should take less risk as one gets older.

If you have a job where you perceive no alpha for yourself, now or in the future, well, hopefully you have avocations or family experiences to compensate. Such a job is the classic toil that merely pays the bills. Most people, at least initially in their careers, aspire to much more. Psychologist Martin Seligman writes that the search for meaning is a person’s greatest driver, and by this he means, knowing your signature strengths, using them in service of something larger than you, something good. Hitting a tennis ball or playing the piano, is extremely satisfying if you are good, as the process just flows and gives you continual reassurance that you are good. Such a feeling is possible for many when they make the right career choice, though clearly the feedback loop is slower. Signature strengths are a comparative advantage, where you have the most alpha. Applying these in a market economy means you are involved in a web of transactions larger than yourself. Assuming your efforts are part of a system you find morally good (as opposed to, say, developing better ways to distribute spam e-mails), maximizing your alpha should provide you with not merely a way to maximize your income, but give you the greatest satisfaction, and the most meaning, in your life.