The data in Chapter 4 highlight that higher volatility or uncertainty is generally correlated with lower returns. These are the returns unconditional on alpha. There is always the hope that one is like Warren Buffett, so expected returns might actually be positive for them. This generates a hope premium in wildly volatile assets, an expected return biased above the average return for these assets based on a delusion. Yet, no matter how risky my attempt to create new fall fashions, or design a new theory in physics, they would fail with certainty because I have no conceivable alpha in these objectives. More risk without alpha is a really bad idea, because markets are competitive and unless we have good reason to think we are above average, we are merely gambling in a casino that regularly plays and wins money on investor hopes. The risks that correlate positively with return have a unique synergy with your individual strengths, by acquiring insights that are not obvious, that one can then leverage this edge—take advantage of the risk-return positive correlation that exists, conditional upon you having some edge.

Lots of investment advice is more about objectives than tactics. That’s easy: We all want higher return and lower risk (how many times have you read the day trader rule “don’t lose money”?). And how do you estimate future returns? Presumably, first you estimate risk, and then you apply the premise that risk premia underlie all predictable returns. But there is no risk premium for most of the investment spectrum, so you can and should safely ignore this, and focus on the expected return. The expected return is generally highly case specific, and often requires tactics such as complementary market-making activity not available to the average investor.

This mistake of seeing risk as fundamentally some uber-factor that, objectively identified, generates above-average returns, is a fundamental error in finance. For example, in William Bernstein’s book *The Four Pillars of Investing*, he says, “Whether you invest in stocks, bonds, or for that matter real estate or any other kind of capital asset, you are rewarded mainly for your exposure to only one thing: risk.” The implication is to first understand
risk, which is objective because priced risk pertains to a small set of priced-risk factors, those assets or products with certain covariances with aggregate measures of societal welfare such as the overall market or inflation. Then, choose how much of such risk one wants, which implies a specific level of expected return, noting the trade-off. Everything else is window dressing: effort, skill, and wisdom.

The main problem with this view is that there are no agreed-upon measures of risk that tend to generate higher returns. Bernstein suggests investing with a small-cap and value slant because these have had higher returns in the academic literature, even though there is no one who has identified any risk measure that fundamentally explains their risk. Indeed, as Chapter 3 noted, value firms seem, if anything, less risky than growth (lower betas, less cyclical). Thus, Bernstein is essentially saying only risk matters, but then recommends investing in assets based on characteristics associated with higher-than-average returns. But here's the rub: if the excess returns of small cap and value stocks are a function of risk, one should not highlight them as attractive. Growth (antivalue) and large cap stocks, presumably, have the same reward-to-risk ratio, properly defined. But he, like everyone else, cannot fathom what that risk would be, or know anyone who cares, so he highlights value and size because he does know people care about returns. The same inconsistency is present in Dimensional Fund Advisors, set up by the kings of Risk as an explanation of everything, Eugene Fama, Rex Sinquefeld, and Kenneth French: it started with small cap funds, and branched into value funds—why target the lower-returning growth stocks? They understand people want strategies with higher returns. It's the kind of inconsistency that drives me crazy, because the risk-begets-reward pillar is put out as this wonderful theoretical principle that explains everything, yet everyone knows that no one wants to buy a stock that will prospectively earn a lower-than-average return because it has less risk, though theoretically it should happen all the time.

Another problem with this mindset is that it neglects alpha, which is really what most financial professionals are trying to generate and capture, because investors do not pay big fees for access to beta risk. The risks that pay off to an individual, however, are of a very particular type, consistent with one's particular interests and abilities. Risk is thus like education, in that more is only better after a certain point when it is highly tailored to an individual. Some people would benefit greatly from calculus, most are wasting their time; some would benefit from learning French, most would never use it. The value of education and risk involve individuals evaluating their skills and the market need for those skills, so it is the right kind. Only individuals are motivated and informed to take the right risks, and to put someone into a volatile stock, or a new franchise, is risky, but also a certain
waste if this was not the choice of the individual after looking at his various opportunities and skill set.

Successful alpha implies risk taking, but this is nothing like the risk as defined in financial models, assets with merely certain covariance characteristics. Thus, in trying to relate risk to return as a general rule, one must say something about the type of risk, which relates to the type of alpha, and this must be consistent with an individual's ability to capitalize on it.

Consider the success of John Nash. While at Princeton, Nash suggested a new physical theory to Albert Einstein concerning "gravity, friction, and radiation," and spent an hour in Einstein's office drawing equations on his blackboard. Einstein, who was then the most famous scientist alive, corrected the brash young Nash, and told him he needed to "study some more physics."¹ In contrast, without any training in economics, he discovered the concept of an equilibrium in zero-sum games that has been a workhorse for economists. What was really impressive in this result, was his application of a fixed-point theorem to this problem, as it was not obvious this type of mathematics is relevant, sort of like saying Fermat's Last Theorem can solve the traveling salesman problem in operations research. Clearly, Nash was unconventional, a risk taker who overreached on occasion. Reaching into physics was not fruitful because one needs to learn the state of the art, which in physics at this time was substantial. But game theory was really just starting, and so someone with merely a deep knowledge of math could discover a very useful result. To say Nash benefited from risk taking obscures that it was the right risk taking: the right time (early in game theory), and targeting the right subject (a problem amenable to mathematics) as defined by a person's particular strengths (Nash's excellence in math). How do we find the right risk taking for us?

**FINDING THE RIGHT ALPHA**

In 1972, Philip Anderson, who won the Nobel prize in physics for his work on superconductivity, wrote an article titled "More Is Different," and contended that particle physics, and indeed all reductionist approaches, have limited ability to explain the world. Reality has a hierarchical structure, but that does not mean one should always try to explain one layer from deeper layers.

> At each stage entirely new laws, concepts, and generalizations are necessary, requiring inspiration and creativity to just as great a degree as in the previous one. Psychology is not applied biology, nor is biology applied chemistry.
No collective organizational phenomenon, such as crystallization and magnetism, has ever been deduced from its lower-level parts. Yet there is a desire to find reductionist laws based on cascading implications, and these laws tend to be very mathematical, and also tend to be frameworks so they can encapsulate the general phenomena to be explained, leaving the specifics as an exercise for the reader. Thus, the attractiveness of Chaos theory, with a basic equation generating layer after layer of ordered complexity, general consistency, but no specific model. The richness of these models is both their attractiveness and their downfall, because at the level of practicality all predictability is lost. Emergent phenomena render reductionist views irrelevant for explaining phenomena at that level—it is unhelpful to try to understand cancer through mere chemistry, or worse, particle physics. But that does not mean that chemistry, or particle physics, is uninteresting, just, at one level, not so much. Similarly, most useful insights in finance are best described with only a level or two of greater depth than the data they are applied to. Going down to some fundamental risk explanation is not helpful, because at the broad level, it does not work, but more prosaically it pooh-poohs the importance of detailed knowledge combined with finding coalitions to successfully apply an insight.

I was once at an investors and customer conference sponsored by Northern Trust, which manages money, mainly serving wealthy clients. Most of the conference attendees were lawyers and tax accountants, the primary private wealth consultants who worked with Northern Trust. By revealed preference of customers, the most important person to speak with if you have over $5 million, is not a financial expert, but a legal and tax expert, because not optimizing over these constraints is much more important than not having the right exposure to small cap stocks. You might say that "other than tax and legal issues," the ideas from Modern Portfolio Theory are front and center, but that too is simply untrue, as you will find very few successful finance professionals who understand risk factors as described in modern financial theory.

To be a good trader, investor, or broker takes a skill set that does not necessarily require some single finance principle. To start a career in finance, and think alpha is necessarily a function of estimating returns through a market model of risk, is to constrain one’s probabilities of success enormously, because very few investors make decisions this way and more importantly, because alpha is usually found far outside this approach. Not that this is never fruitful, but of all the ways to succeed, this is one very narrow path. As risk does not beget return on average, the market in practice happily ignores those who have a narrow but deep understanding of Stochastic Discount Factors.
If your job is allocating money to portfolio managers, or as an editor at a journal, you do not create the content, but you make important decisions as to what is alpha: meta alpha. One could get into semantics, and say any edge is an alpha, broadly defined. Alpha can be intrinsic to a strategy, person, group, or brand. While related to an abnormal return, it is part of a continuum of comparative advantages relative to a benchmark, what the unimaginative, or intellectually meek person would do. We take risks when we choose majors, change careers, and of course when we write checks to invest in something. There is the way everyone else does it as you perceive it, and when you deviate from this, you take a risk. You take a risk to make your life better by finding a better way of doing something.

The key to succeeding in anything is first knowing your relative strengths, because you should gravitate toward those kinds of alphas where your skills—contacts, emotional intelligence, math, computers, and so on—have the highest relative value. But you must avoid the streetlight phenomenon, where the drunk looks for the car keys under the streetlight even though he lost them somewhere else merely because that is the only place he can see. Every so often you must evaluate the field you are in, because it is easier to succeed in a field that is growing. But there is a trade-off, because your value is a function of your ability in the field, your experience in the field, and the field’s viability itself. Thus, you may optimize in a stagnant field, given your abilities and contacts. In any case, you must be optimizing your alpha, and it helps to have some intuition of what kinds of things have worked in the past, because although past is not prologue, historical examples are instructive as to what we can expect going forward.

The list of good alpha ideas is highly parochial in practice because a good idea is an improvement on the state of the art, which is peculiar to a specific art. Someone in software development would benefit from business books by software engineers; someone in advertising will profit from that genre’s innovators. Similarly, traders would focus on technical constraints in the institutional nature of the limited number of trading platforms, and game theoretic issues in disguising intentions and inferring trade flow, are looking at finance from an almost entirely different playbook compared to a retail investor or the head of a large pension fund. One should think about finding alpha, like finding success in an ecosystem where you get to pick your species. You can be the equivalent of a large carnivore or a tiny microbe; there’s competition at every level, but you pick the domain you compete in, which require very different skills. The key is that you do not have to compete directly with everyone the way you do in school where there are a handful of subjects and sports, and everyone is graded by the same handful of teachers and coaches. In life, there are so many different occupations, you
can be happily indifferent to your relative weakness in a large set of skills. Most of the alpha in finance, something people truly appreciate, is unrelated to skills in portfolio optimization.

Like school, one needs to choose well in advance activities where one is best suited to proceed, because any field has a considerable learning curve, and it is harder to get opportunities to climb such a curve as one gets older. To create alpha requires you to take a risk, and the first risk is investing time becoming an expert on the state of the art, which may turn out to be the greatest investment you ever made, or like my years learning the clarinet, or time spent learning the proofs in Stokey and Lucas’s *Recursive Methods in Economic Dynamics*, a waste of time so complete I cannot think about it less. The costs include the opportunity cost, combined with the fixed cost one must pay in time and effort in getting the knowledge, credibility, and contacts in a new field, a process that could take a decade.

Thus, the first decision for an alpha seeker is building the largest skill set that leaves the largest set of areas to move into as one gains insight into opportunities. There is clearly some kind of hierarchy of knowledge in finance, understanding present valuation, statistics, option value, but also basics in computing so you can do things on your own, especially when you start and don’t have minions to flush out your vague premonitions. While understanding risk in the sense of diversification of idiosyncratic risk, or taking advantage of correlations, is a fundamental concept in finance, understanding risk defined as a kind of uncertainty that is priced in the market, is not fundamental, or even useful. As there are no agreed-upon risk factors that are priced, the essential knowledge is not an abstruse concept like the SDF, but simply, looking for excess returns, conventionally defined, just as the momentum, size, and value effects were discovered.

I will focus on alpha related to finance because this is mainly a book about finance, yet it is important to remember our intuition and instincts about risk taking comes from its general application to daily life, whenever we attempt to create, do, or say something novel and present it to others. By seeing famous, conspicuous examples of alpha created, or discovered, in finance one can get a better sense of what it looks like if they create or discover it, as it generally looks nothing like Jensen’s alpha, where an expected return dot is above the Security Market Line. The ideas that make you happy do not change much from century to century, but ideas that can make you rich rarely last more than 10 years. This is because a really good idea, an idea with alpha, involves a lot of institutional detail that is always changing. These details do not obscure the opportunities; they are characteristic of such opportunities. Alpha exists in many forms, each with its own set of details that makes it unique, and one must master such details if one is to truly find alpha.
ARBITRAGING PUT-CALL PARITY

One should start at the very basic alpha in finance, the arbitrage profit. This is arbitrage, seeing that you can buy a widget for $10 here, and sell it for $11 there, making an instant, riskless, profit.

Consider that in the early days of options, one could make a comfortable living merely by buying and selling options with different implied volatilities. Andy Redleaf, now CEO of the hedge fund Whitebox Capital, notes that in 1978, when he was a 20-year-old trading options on the exchange, he made a six-figure income for himself by merely arbitraging the puts and calls traded. That is, using put-call parity, one could put on a riskless trade, selling a call and buying a put at the same strike price, which creates a synthetic short forward position at the expiration date, then going long the stock (or the reverse, shorting the stock and buying a call and selling a put). The combined position generates a zero-risk portfolio. If the premium on the call is greater than that on the put, you have made money at no risk. By 1982, this game was basically over, as market makers could get financing on their stock positions and do this themselves.

The arbitrage existed because he was looking at calls, and comparing them to puts, whereas most investors would merely see that these are ways to take a leveraged bullish or bearish position, because if you know which way a stock is going, options offer the greatest payout. That is, if you gave me the Wall Street Journal from next year, as a retail investor I would maximize my wealth by taking option positions in those biggest movers with the most out-of-the-money options available. Redleaf was looking at the problem at a higher level, the way an expert chess player looks at the problem several moves ahead of the novice, and noticed a connection between puts, calls, and the stock independent of his views on the stock’s general direction, so that there was a connection between a single call, and a basket of the stock and put. It was somewhat sophisticated, but even then put-call parity was well known in standard option treatises at the time, so it is not like he had to derive this from first principles, though usually ideas like this are independently discovered by many.

CONVEXITY TRADE IN FUTURES AND SWAPS

Such instant arbitrage is relatively rare, and confined to those with special low-cost access. The following is kind of a subtle mathematical logic that is the provenance of investing quants. The arbitrage worked like this. Eurodollar futures mark your profit or loss, daily, to an account based on a
constant times \((F_t - X_t)\), where \(F_t\) is the current Eurodollar future for time \(t\), and \(X_t\) is the futures price at which you bought or sold the contract. The futures price at time \(t\) will be settled at the then current LIBOR three-month rate. The daily profit is a linear function of the current Eurodollar futures price for time \(t\). A forward is the exact same thing, only they are not marked to market daily. Thus, you take those same profits, \((F_t - X_t)\), but they are only paid at time \(t\), which may be 5 or 10 years from today. There is a real subtlety here, because when \(F_t\) is low, your discount rate of the payout, \((F_t - X_t)\), is high, and when \(F_t\) is high, your discount rate of that payout is low. This asymmetry leads to convexity in the present value of forward contracts, in contrast to the linear nature of futures. A forward rate contract is convex because its price rises more for a downward move in forward rates than its price declines for an equal upward move in forward rates.

In both markets, one is taking a bet on a forward rate, but with an interest rate swap, which is a set of forwards, cash changes hands only as each leg of the swap matures, often far in the future. In contrast, a strip of Eurodollar futures is market to market, and the profit and loss is booked daily. Like much of financial engineering, the general idea is straightforward, but the specifics depend on a lot of parochial details about instrument conventions, whether they are quoted in yield or price space, and so forth. The key driver is that the present value of gains and losses are asymmetric, in that futures rates falling are discounted at a lower rate, necessarily, than when future rates rise; as the futures prices are market to market while the swap is not, this asymmetry creates arbitrage.

Figure 11.1 shows how the profit and loss of these two contracts, forwards and futures, varies as a function of the forward rate. The convexity, or upward curvature, of the forward is the key difference with the futures profit.

Now, the curvature is very slight at the scale of daily changes, so that the difference in the effect on profits for forwards versus futures is only about 1 percent difference for changes in rates of 10 basis points. Only a true quant would have seen this, because graphically, on a daily basis, it is not obvious—you have to do the math, guided by the logic. With estimates of future variability of those rates, one can estimate the present value of this convexity on swaps versus futures.

This convexity in swaps, but not Eurodollars, means that to preclude arbitrage, the futures prices should have a higher implied forward rate to compensate for its lower convexity. If future and forward rates were equivalent, one could buy fixed-receive swaps and short Eurodollar futures, and the daily mark to market of the Eurodollars versus the present value mark to market of the swap would allow one to lock in a sure thing. The effect
added up to 15 basis points in present value if done with 5-year swaps, and 40 basis point for a 10-year swap.

I knew an executive who worked at UBS in the 1990s who said they made tens of millions of dollars on this strategy. He was surprised when I told him that the trade was outlined very clearly in Risk magazine in 1990 because he was certain UBS figured the trade out themselves after that. Indeed, a handful, but only a handful, of large institutions made a lot of money, for perhaps five years, on this trade. Its demise coincided with several published papers examining this strategy in detail around 1995, outlining very methodically how it works. One needed to be large to trade swaps of a long duration and offset this risk with futures, because this involves effectively cross-margining, where the gains and losses from one product, in this case swaps, is netted against the gains and losses of a different product, in this case futures. Individuals usually cannot receive cross margining from their broker, often by regulatory fiat, making this a game only the big guys can profit from.

Also, because the edge was so thin, large financial institutions could layer this tactic onto their normal market-making business in a way that made this trade viable when others could not. This is an important subtlety.
to many successful trading strategies, and why as a retail trader, trading like an institution or floor trader is not sophisticated, but rather quite stupid. The main job of the market maker is to make money off the flow of buys and sells he gets, while keeping the residual positions he incidentally acquires small. If he posts a bid-ask of 10 to 11, and acquires a large short position, he should adjust his bid-ask downward for two reasons. First, the market is saying people value this at less than 10, and they are therefore selling to him at 10. Second, his position now exposes him to delta risk, that is, risk in his portfolio, which is not something he really has a view on (delta risk is the risk from a change in the price of a security on a portfolio). He is busy posting bids and offers and keeping them fresh, not trying to figure out if the latest statements by the Federal Reserve are good or bad for his asset. Over the intervals he evaluates stocks, his view on fundamental value is really irrelevant, and he needs to focus on keeping his bid-ask fresh, not worrying about his long-term opinion on the securities he is trading.

However, if a large group at a higher level of aggregation monitors these positions, and decides that, say, being long IBM, or Eurodollars, is good, they might tolerate an acquisition of these positions, and not move their bid-ask so quickly as to make this portfolio go away. One can acquire positions based on market making at a negative cost, in a sense, thereby making a strategy with a slim expected return viable, but only in combination with the low, if not negative cost of its implementation. Retail traders pay the bid-ask to put on a position, whereas floor traders receive the bid-ask to put on the position—though they have to wait until the right flow comes by for this happy fortunate accident to occur. The key is that a strategy with a very small edge might be feasible for a trading operation, but infeasible for someone without the complimentary market maker function.

Another subtle statistical trade was the volatility dispersion trade. Volatility dispersion strategy involves sophisticated analytics, especially as a practical matter, but the gist is fairly straightforward. Consider that if two stocks are independent (correlation is zero), and volatilities of 100 percent, the portfolio volatility of these two stocks is about 71 percent; if the correlation is 1, the portfolio volatility is 100 percent. There is a mathematical function that relates the volatility of the options to the volatility of the index based on the covariance (look up cov(x+y), which is in an introductory statistics textbook). The arbitrage arises because index volatility has historically traded rich on occasion relative to the basket of individual stock volatilities and their correlations. Thus, the dispersion strategy allows traders to profit from price differences using index options and offsetting options on individual stocks.

The dispersion strategy typically consists of selling options on a stock index while simultaneously buying options on the component stocks, meaning
Examples of Alpha

one is short correlation, or long dispersion. The reverse dispersion strategy consists of buying options on a stock index and selling options on the component stocks. Clearly, given the bid-ask of options, and the many options one needed to trade, having direct access as an adjunct to some other function such as market making was essential at making this a real value-add.

In 2002 and 2003, as the market was crashing, then rebounding, there were many opportunities in this trade. I received several calls from headhunters around that time, desperately looking for quants to join teams that were going to implement these types of trades. One large hedge fund hired several teams to independently implement this strategy, thinking that a little internal competition might be the best way to make sure it is being done correctly. But by 2004, so many people were doing it that there was no edge in this trade, especially at large scale. The fund that hired several teams to implement this kind of arbitrage let these groups go rather quickly and quietly.

At the end of a good strategy, and sometimes the beginning, only those acting in concert with market makers—those with the lowest transaction costs—will be plying it profitably, which creates a lot of confusion, because hucksters love to sell strategies as “the same as those exploited by the big banks,” as if it is attractive merely because these players are in-the-know. It is a combination of being knowledgeable, and having the lowest cost access, that is essential for many alpha strategies. One sure sign of a strategy’s demise occurs when a sufficient number of people know about it and have forced out of the last bits of profit left in the strategy, write books about how trading “like a turtle” or some such strategy can make you rich, like it did for these successful traders or institutions. These are symptomatic of rules that used to work, but no longer do, especially without having extremely low transaction costs.

PAIRS AND MEAN REVERSION

Arbitraging assets by finding connections is one way of finding arbitrage, but another is finding patterns. That is, the sequence, 1, 1, 2, 3, 5, 8 is the start of the well-known Fibonacci sequence, which underlies the population growth of rabbits, and, for some, markets. If you know Fibonacci formula, and the data indeed follow this formula, you can predict the next number. Predicting the next price, given a sequence of prices, is not the same as arbitrage, though, because there is nothing forcing one asset to equal another at some future date, or that cash flows are always constant as market values change. You merely anticipate future changes based on patterns, and hopefully, you have discovered some tendency in the data such that you go long things that rise,
short things that fall, and on average make money in bull or bear markets because you are hedged. It requires faith that the future will be like the past.

The most famous pattern in markets is pairs trading. When I came in 2001 to Deephaven, a hedge fund in Minnesota, there were a lot of pairs traders there, and many of them had made money for years trading this strategy. I later learned that many large stat-arb (statistical arbitrage) shops on Wall Street, such as Princeton Newport, D.E. Shaw, and Morgan Stanley, all worked this basic idea, and many traders there became multimillionaires exploiting this pattern throughout the 1990s. The idea is simple. You find a pair of similar companies, such as Coca-Cola and Pepsi. You can find these using just correlations, or from fundamental analysis, looking at the industry and noting firms that have very similar market capitalization and product mixes. Now say Coke rises 5 percent, whereas Pepsi does not move for over a week. On average, if you shorted Coke, and went long Pepsi, you generally made money over the next week, as the initial move was generally overdone for the stock that jumped whereas the other half of the pair would move in sympathy, perhaps as traders recognized the fact that similar risks or fortunes were present in the pair asset.

What is really interesting about pairs is the sheer simplicity of the trade. While it started by trading pairs, so the trader would be hedged, the real edge in the trade came from the mean reversion in the big mover. Thus, the basic idea was to go short the big movers up, and go long the big movers down, and this was the basic ingredient in statistical arbitrage that was so successful in the 1990s. I knew some Ph.D.s implementing various nuances of this strategy, but generally their refinements were second order, somewhat inevitable whenever you give a bunch of smart people a lot of data and a basic model. Stat arb sounds very complicated, and one would often see computer scientists and mathematicians from the greatest schools in the world doing this, but it's a bit like hiring Eddie Van Halen to play Smoke on the Water. In their defense, people were making a lot of money, and if you are really rich, you can afford to hire overqualified people to do things just to be safe.

Andrew Lo and Craig MacKinlay wrote about this in 1988, noting that variance ratio bounds tests implied that stocks were mean-reverting at high frequencies. That is, if a stock has a pure random walk (that is, big movers did not mean revert), the ratio of its five-day return variance over its one-day return variance should be five; the ratio of the 10-day variance to the 1-day, 10, and so on. In practice, the variance from daily returns was more than proportional to the monthly variance adjusted for the number of days in a month, and so implied that stocks were not a random walk, but exhibited some mean reversion that diminished their volatility over long horizons, relative to the baseline random walk. As the essence of pairs trading, or stat
arb, is mean reversion at high frequencies (for example, weekly or daily), it was really all there in Lo and MacKinlay’s 1988 paper, meaning that they figured it out in 1986. A paper written in 1998 showed how the profitability of a simulation of a pairs strategy worked over time, and found that the profitability of the trade decreased significantly around 1990.\textsuperscript{9}

In 2007, Lo wrote about the decline of the stat-arb strategy of going long losers and short winners, and showed how it declined in profitability over time. His construction of the strategy weighted each position every day by the degree to which it was above or below the average return, so that it was on average neutral to the market. In practice, there are many different ways to implement the general pattern, say by concentrating solely on the extremums, ignoring companies that had substantitive news like actual earnings releases, looking at volume as well, and so forth. But while the nuances helped, they often hurt, and Lo’s general trend holds for the strategy. Figure 11.2 shows how profits declined as this strategy declined over the past decade.

By 2003, you see the profitability of the strategy has fallen significantly, especially compared to the early 1990s period, and Deephaven got rid of

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\caption{Daily Returns to Long Losers and Short Winners}
their pairs trading. Web sites advertising pairs arose around that time, and commercials on CNBC highlighted trading pairs as a great way to escape the rat race. This is surely a good sign that profits in a strategy are zero after modest transaction costs. I know a handful of pairs traders who are able to make a decent living doing this, yet it is not nearly as scalable and easy as it was. If I had a time machine that took me back to 1990, this is the easiest strategy for someone small to exploit.

In 2008, I ran into Andy Lo at a conference and asked if he regretted not taking advantage of this, given that he discovered it when it had at least another decade of incredible returns. He replied that he thought the real key to the strategy was implementing it at the lowest cost, so his edge was really not so great, his missed opportunity not so much. It is really an unfair question, because with hindsight there are many decisions one would do over, but I think he underestimated his alpha in this trade, because a good trade idea always needs more than just a strategy, so the strategy is not worthless merely because it is not worth so much in isolation. For example, any good idea needs capital, but clearly capital needs good ideas; so both are necessary and can haggle over the share of the proceeds. He needed someone with low-cost access to trading, but there were many people with low-cost access to trading who could have used such a strategy. If he found a match, even splitting the profits generously would have made him an immense fortune.

FINDING ALPHA

The Cowles Commission for Research in Economics was founded in 1932. Alfred Cowles, president of an investment counseling firm, initiated some inquiries into the accuracy of professional stock market forecasters over the period from 1928 to 1932. This aroused his interest in fundamental economic research, which led him to offer his financial support toward the establishment of the Cowles Commission. The first Cowles Commission product to attract widespread attention, from both businessmen and professional economists, and still one of the best known of its publications, was a paper by Cowles titled “Can Stock Market Forecasters Forecast?,” published in *Econometrica* in 1933. A three-word abstract of this paper runs as follows: “It is doubtful.” Cowles began his investigation with the suspicion that many forecasters had no real skill and were in effect simply guessing, and he set out to test this hypothesis. He charted the weekly individual stock purchase recommendations of sixteen established financial services from 1928 to 1932, and found that if an investor had followed all of them, with equal initial amounts of capital allotted to each purchase of a stock,
he would have come out making about one and a half per cent per year less than if he had invested in the stock market as a whole. He then checked the common stock investment records of 20 large fire insurance companies for the same period, and found that on the average they fell behind the market by slightly more than one per cent per year. He then charted the forecasts of stock market level made by 24 financial publications from 1928 to 1932 and found that if an investor had followed all of them, again with equal amounts of initial capital allotted to each, he would have fallen behind the market average by about 4 percent per year. The expert’s forecasts of the markets were not even better than chance.

This is the first major empirical study of the Efficient Markets Hypothesis, and has held up quite nicely. Fundamental analysis, either of stock analyst recommendations or in mutual fund management, does not outperform naïve alternative such as buying a diversified portfolio. Yet, mutual funds got away with 8.5 percent up-front commissions and 2 percent annual expense ratios until well into the 1980s. Even today, the vast majority of equity funds are actively managed (about 90 percent). Investment advisers have held themselves out for generations to be providing value through the outperformance of their picks, when everyone with data knew this is not only untrue today, it has always been untrue.

In 1940, after a horrible stretch for stocks, Wall Street veteran Fred Schwed wrote a book titled Where Are the Customer’s Yachts? The title refers to an old joke about a visitor to New York who admired the yachts that the bankers and brokers had in the harbor. Naively, he then asked where the customers’ yachts were. Naturally, there were no customers’ yachts. Schwed’s book outlines all the nefarious tendencies that bedevil investors out of their savings, mainly from overtrading based on hopes that were based on little more than possibilities. Though he describes the brokers as being sincere in their advice, it was generally unwise for the investor, and as the brokers made money regardless of the net return to investors, they had no incentives to become aware of their poor advice—it worked for them. In such a scenario, the easiest way to create alpha is not to devise a better stock-picking strategy, but to merely cut costs, because if a monkey throwing darts does as well as an active mutual fund manager, surely a mechanical monkey proxy is cheaper, and this advantage should add up over time.

By the early 1970s, the Efficient Markets Hypothesis had been formalized, which popularized the idea that it is difficult, if not impossible, to make money in financial markets. Paul Samuelson proved that unbiased expectations implied prices fluctuated randomly, which is either a tautology, or striking, depending on how geeky you are. Thus, there was a theory for why stock prices appear to follow Brownian motion. Applied to mutual
funds, it suggested that these managers were not adding value, merely adding volatility and expense. In 1973, Malkiel's *Random Walk Down Wall Street* appeared, preaching to retail investors that they should not try to outperform the market, and in 1974, Samuelson wrote an article applying these concepts to portfolio managers, arguing for passive portfolio management.10

Thus, the idea was in the air, and independently, John McQuown and David Booth at Wells Fargo, and Rex Sinquefield at American National Bank in Chicago, both established the first passive Index Funds in 1973. These were portfolios targeted at institutions. Interestingly, the Wells Fargo fund was initially an equal-weighted fund on all the stocks on the NYSE, which, given the large number of small stocks, and the fact that a price decline meant you should buy more, and at a price increase sell more, proved to be an implementation nightmare. It was replaced with a value-weighted index fund of the S&P500 in 1976, which eliminates this problem. Another misstep was clearly not targeting the retail investor early, which turned out to be where the real money was. Rex Sinquefeld started Dimensional Fund Advisors in 1981, in part to address this deficiency. Sinquefeld was also hooked into the University of Chicago, which had Eugene Fama as its head of research. As the size effect was the hot thing at that time, DFA had several small cap portfolios at the outset to take advantage of this anomaly. Unfortunately, the size effect disappeared in the 1980s, but Dimensional was able to survive this setback admirably. Thus, even a great, simple idea like an index fund, has a learning curve in practice.

In 1951, the anecdotal evidence John Bogle assembled in his Princeton University senior thesis on the then-minuscule mutual fund industry led him to write that mutual funds "can make no claim to superiority to the market averages." How joyous it must be to quote oneself from such inauspicious beginnings to explain one's success. But another driver was that in 1975, his company, Vanguard, was a shareholder-owned mutual fund group—the company was owned by the mutual fund investors—so low-cost fund administration was not taking money from owners, it was giving money to them. In contrast, the idea of an index fund would have hardly appealed to a high-cost fund complex whose very revenue depended on the conviction that active management did add value, at least, in their particular case. In his pitch to the Vanguard board for starting an index fund, he brought some of his own data on the performance of mutual fund managers, suggesting that they underperformed by about the same amount as their expenses, and some references to recent articles by Samuelson and Charles Ellis.11 So, blessed with some good intuition from Bogle, the rising popularity of the idea in the academy, timing, and good incentives from Vanguard, they had both the opportunity and the motive to create the first retail index fund, which is now the largest index fund in the world, and Vanguard, the
Examples of Alpha

second-largest fund family. By the next summer, the fund was launched with about $11 million.

At the time these funds were being introduced, the whole idea seemed stupid to most non-economists. It was dubbed "Bogle's folly," and described as unpatriotic, on the premise that any red-blooded U.S. citizen has investing alpha. Indeed, a sure sign you are taking risk is that most people, especially the experts, think you are a fool. While Samuelson sneered from his perch in his weekly Newsweek column, the hoi polloi continued investing in active managers, because index funds were a trivial portion of equity investments until the late 1980s. 12 “Sure, we considered indexing, but we rejected it—why settle for mediocrity,” said an investor in Business Week in 1984.13 A Forbes article about the same time made the standard argument against indexing:

To buy an index fund is to accept mediocrity. Such a decision is hard to justify in the stock market where several great investors—the likes of John Templeton and Warren Buffett and Philip Carret—have shown that it is possible to beat the market over three-decade stretches.14

Note that in reciting the obvious success stories, it is the same handful of names—Buffett, Graham, Lynch, Neff, Templeton—that prove the statement “The average fund manager is average before costs” is incorrect. Why not say, many people have won the lottery, so it is hard to accept that it cannot be done? The fact that some can, and they are, generally, smart people, suggests there is a chance for someone with alpha regardless of the odds. Economists may find such hope-based investing as foolish, but remember, many an economist points to an “existence theorem” to support their views on some practical matter even if the theorem, merely implies something is possible, not probable. The key is, if it is possible, there is hope.

Russ Wermers estimated that equity mutual funds outperform the market by 1.3 percent per year, although expenses and transaction costs reduce this benefit to essentially zero. His conclusion: “Funds pick stocks well enough to cover their costs.”15 But that ignores their higher annual volatility for most funds relative to broad indexes, which perhaps is not priced risk, but surely annoying. John Bogle illustrates that an index fund has a 350-basis-point advantage over the average equity mutual fund because of management expenses, brokerage costs, sales charges, and tax advantages.16 Arnott, Berkin, and Ye (2000) find that the Vanguard 500 Index Fund outperforms the average equity mutual fund and the effect is amplified when taxes are considered.17 Burton Malkiel notes that over the past 25 years,
about 70 percent of active equity managers have been outperformed by the S&P 500 Stock Index.18

Elton, Gruber, and Blake ask the relevant question: "Given that there are sufficient index funds to span most investors’ risk choices, that the index funds are available at a low cost, and that the low cost of index funds means that a combination of index funds is likely to outperform an active fund of similar risk...why select an actively managed fund?"19 Obviously, only an overconfident hope in alpha, but at least today there’s an alternative, whereas, from a practical perspective, you could not get a low-cost, diversified portfolio before 1975 without paying some delusional or duplicitous manager to take extra gambles with your money.

By year-end 2007, in the United States, assets in Exchange Traded Funds (ETFs) and index mutual funds reached more than $1.4 trillion, and these indexed products have increased more than eightfold over the past 10 years.20 Yet as large as this is, it is only 11 percent of the total assets managed by all registered investment companies. ETFs and index funds are available in most other broad asset classes but, to date, have attracted less investor interest than those tied to indexes of large-blend domestic equity.

Now, at some level this is not a big opportunity, because for investors, it merely offered one a modestly better Sharpe ratio on average (say from 0.4 to 0.5), but one without a chance for arbitrage that drives a lot of investing. Yet the aggregate savings to investors was much larger than the wealth created by the convexity trade between Eurodollar futures and swaps. Furthermore, people did get rich off this idea, such as the innovators Bogle, McQuown, and Sinquefeld, who are all fabulously wealthy as a result of their pioneering efforts in these domains. Certainly many other executives and owners who were part of these efforts made successful careers targeting this unconventional tactic. As with a lot of alpha, the big rewards are not in passive application or discovery, but active marketing, ownership, and implementation. Those who were able to parlay this into yachts, remains the brokers, which should tell you something about trying to become rich as a retail investor.

CONVERTIBLE BONDS

A convertible bond is basically a bond with a long-term call option attached.21 For decades, these bonds presented a superior return to straight bonds of similar credit quality. Consider first what a good Sharpe ratio is. The equity premium puzzle is that the apparent return over Treasuries for U.S. equities has historically been around 6 percent. When combined with
an annualized volatility of around 17 percent, this implies a Sharpe ratio of 0.35. In the early development of the CAPM, maximizing the Sharpe ratio was the underlying objective, what investors should do, and what they implicitly were doing. A Sharpe of 0.35 is anomalously high relative to alternatives, and so, represents a good benchmark of what a great Sharpe ratio is for an investor.

For Convertible Bond Arbitrage, the idea is that a hedge fund buys convertible bonds, hedges the interest rate risk with bond futures, and the equity risk through short positions on the equity aligned with the convertible bond, thereby isolating only the convertible part of the position. This kind of strategy is really only available to hedge funds, because only a hedge fund could go long and short such different products, and so hedge funds were the primary purchasers of convertible bonds. Most mutual funds are constrained to be only long, and cannot hedge their equity risk, or interest rate risk, so they were at a relative disadvantage as purchasers of this asset. The returns from January 1994 through May 2003 listed in Table 11.1 are quite impressive.

This was the return after fees, which in hedge funds averaged about 2 percent of assets, and 20 percent of profits. Often, this was after paying the portfolio manager 10 percent of the profits of his portfolio. Thus, the returns were making everyone in this space very wealthy, and the growth of an asset class is primarily driven by making not the investors rich, but the managers rich. A Sharpe above 1.0 is a very good return, especially on an asset class as scalable as convertible bonds, and so, investors had no complaints even after paying the managers a large chunk. Clearly, this strategy had a huge amount of alpha. Princeton Newport, the hedge fund operated by Beat the Dealer legend Edward Thorpe, and Citidel Investments, a fund giant built by Ken Griffith, both built a good deal of their strategy focusing on convertible bonds, and the rise in funds targeting convertible bonds rose dramatically throughout the 1990s.22

How could this be?

<table>
<thead>
<tr>
<th>TABLE 11.1</th>
<th>Convertible Bond Arbitrage: January 1994 through May 2003</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Hennessee CS/Tremont SPX</td>
</tr>
<tr>
<td>Annual Return</td>
<td>10.08%</td>
</tr>
<tr>
<td>Annual Standard Deviation</td>
<td>4.20%</td>
</tr>
<tr>
<td>Sharpe</td>
<td>1.30</td>
</tr>
</tbody>
</table>
On the supply side, issuers loved them because they exploited the fact that underpricing an option is part of a very subtle moral hazard. A company essentially gets a lower rate on its debt through the value of the option the bondholder gets, because a convert is a portfolio, and while the option is in general undervalued, it is not worth zero:

\[
\text{Straight Bond} + \text{Option} = \text{Convert Bond}
\]

The positive value of the option implies the convertible bond’s bond has a lower yield and lower price, than if they issued a straight bond without an option. A lower interest rate makes the CFO look good, because it is a directly measurable expense. Now, if the stock price subsequently falls, the bondholder will not exercise the option, and the CFO can point to how he saved the company interest expense at no explicit cost, because the option expired as worthless; if the stock price rises, equity holders are usually sufficiently happy to not worry about any dilution from the convertibles.

While a convertible bond is like a bond plus a call option on the equity, it is actually considerably more complicated. A convertible bond is usually callable, so one has to account for the situation that issuers can call bonds back as their credit rating improves, or as interest rates decline. Also, converting the bond into equity extinguishes the bond. Interest rates, credit spreads, and equity prices are not independent of each other, as for example, a much higher stock price in the future will be associated with a lower credit spread. Thus, you need a model that captures the statistical nature of these processes in a way that captures some of the correlations we know exist, but are difficult to quantify. Then, there are an infinite number of combinations of yields and implied option volatilities on the warrants that correspond to its current price, because it could have a yield of 8 percent, and an option vol of 30 percent, or a yield of 9 percent, and an option vol of 20 percent. You have two unknown inputs to the model (volatility and spread), and one observed variable (the price). As it is very difficult to reverse engineer the value implicitly assigned to the option in the convertible bond, it is rare the CFO is held accountable for selling it at too low an implied volatility or too high a credit spread, because the credit spread will necessarily be less than the spread on a straight bond, and there is no unambiguous vol that the convertible bond was sold at, only an implicit vol, and the company rarely goes out of its way to advertise this.

In 2001, I joined a hedge fund in Minneapolis, and one of my first charges was to try to make sense of the prices and volatilities in the convertible bond portfolio. I found that on average, the implied volatilities of these
convertibles was about 9 vols (for example, a 30 percent implied annualized volatility for the equity, as opposed to a 39 percent in their comparable straight options) lower than on comparable options or historical volatilities, while the spread on the bonds taking this option value into account was about 2 percent higher (that is, 200 basis points higher). A different convertible bond model, or assumptions, may change those relative numbers, but the bottom line was that convertible bonds gave an investor cheap options, and relatively higher yield, on the same underlying assets. The board and shareholders rarely had the information, nor were they interested in getting it, allowing the management to exploit this issue.

Why this could be so for the demand side was easy given the complexity of the problem. Most credit analysts do not like evaluating option values of equities. If you are a credit specialist, selling your alpha in the credit space, then doing a joint problem such as convertible bonds either meant you would be getting into an area where you did not have a lot of confidence, or sharing the profits with someone else. The same held for option experts. Convertible bonds were a hybrid product that investors generally did not appreciate being aggregated. Issuers persisted because of the perverse incentives to the CFOs and the limited information given to the Board and investors. This situation allowed the product to be underpriced for years, and people like Edward Thorpe, who was a pioneer on option pricing, had the confidence in their ability to value warrants, make some modest assumption about the credit, and diversify. It was easy money for decades.

But around 2002, many investment banks started to roll out pricing models online that made it easier to see what a great deal this was. These pricing tools basically made it easier for someone good at options, or credit, to jump into converts, deferring to the defaults for the stuff he was ignorant on (credit or options, as the case may be). The models were sufficiently well understood, that the presentation was done well, and the default assumptions of the investment bank were not bad. Furthermore, they started trading credit default swaps on these bonds, and also had instruments to strip out the volatility, or credit risk, so one could buy only the optionality on the convertible bond, or only its credit risk. As this market grew, arbitrage basically whittled the premium in convertible bonds away, because hedge funds and the investment banks, were buying the straight debt, or straight options, and selling the pure debt or pure optionality of the same issuer through a derivative on the convertible bond. Traders could avoid worrying about things they did not know, such as credit, or volatility, and take advantage of their specialty. The market disaggregated a product that was fundamentally not appreciated for its aggregation by investors.
TABLE 11.2 Convertible Bond Arbitrage: June 2004–December 2008

<table>
<thead>
<tr>
<th></th>
<th>CS/Tremont</th>
<th>Hedge Fund Research HFX</th>
<th>SPX</th>
</tr>
</thead>
<tbody>
<tr>
<td>Annual Return</td>
<td>-2.54%</td>
<td>-3.80%</td>
<td>1.14%</td>
</tr>
<tr>
<td>Annual Standard Deviation</td>
<td>8.84%</td>
<td>9.62%</td>
<td>12.94%</td>
</tr>
<tr>
<td>Sharpe</td>
<td>-0.64</td>
<td>-0.72</td>
<td>-0.15</td>
</tr>
</tbody>
</table>

Source: Credit Suisse/Tremont Convertible Bond Index, Hedge Fund Research Convertible Bond index.

This all came to a head in the summer of 2003, when the increase in entry ended this game, as it usually does, with a terrible drawdown. Since then, the Sharpe ratio has been less than zero, as shown in Table 11.2. It could be too early to call, but I think the salad days of convertible bonds are now a historical artifact, and you can see this if you try to back into volatilities and spreads given current prices. That is, actual returns since 2003 have been negative in this strategy (it should be beta neutral), and this makes sense given the new tactics used by investors, allowing entry and driving down returns. Unlike pairs trading, there is a way to see if the juice is in the trade given current parameters, and it is gone.

LONG AND SHORT EQUITY HEDGE FUNDS

Hedge funds cover investment strategies so diverse, they are more different than they are alike. Their essence is they can buy or sell anything, unlike mutual funds that are constrained to be long only, in a particular asset, often of a particular type (such as small cap growth funds). But they generally target a volatility slightly below the S&P500, about a 12 percent annualized volatility, even though leverage is a choice variable, and so could easily generate higher volatility and returns.

Hedge funds have typically received 2 percent of assets, and 20 percent of profits for their efforts. The Credit Suisse/Tremont Fund Index shows an average return of 10 percent from 1994 through August 2008, compared to the S&P’s 9 percent annualized return over that same period. The beta with the S&P was about 0.25. Now, many people look at the average expense ratio of active (that is, non-index) mutual funds, at 1.0 percent to seem cheaper. But looking deeper at the numbers, the hedge funds appear to be a much cheaper way of accessing equity alpha.
Assume that returns for equity mutual funds are generated through a combination of a passive investment in the benchmark, and then layers on an active portfolio. This generates a process

\[ r_p = w_A r_A + (1 - w_A) r_I \]

Where the weighting on the active portfolio is \( w_A \), \( r_A \) is the return on the active portfolio, \( r_I \) is the return on the index, and \( r_p \) is the return on the total portfolio. In this case, the \( R^2 \) between the mutual fund and the index would be

\[ R^2 = \frac{(1 - w_A)^2 \sigma_I^2}{\sigma_p^2} \]

Thus, one can take \( R^2 \)'s and estimate the implicit active picks by the fund manager. Applying this to 152 large cap funds, Ross Miller estimates about 15 percent of the portfolio is actively managed.23

This implies an investor can replicate the risk-and-return characteristics of the fund by placing 85 percent of her assets in an index fund that tracks the S&P500 and the remaining 15 percent in an appropriately chosen market-neutral investment. Assuming 20 basis points as the expense ratio for the passive component of a fund (about the same ratio as large index funds), the average fund expense ratio of about 120 basis points can be seen as “overcharging” investors by 100 basis points on the passive component of its portfolio. If we assess those 100 basis points against the 15 percent of the portfolio that is actively managed, we would find that annual expenses account for 6.66 percent of those funds, in addition to the 1.20 percent, for a total of 7.86 percent. That is, if you are paying for alpha, he is charging 7.86 percent of assets for those alpha picks—implying his picks have to return 7.86 percent above the benchmark to justify his fees, which is highly improbable.

A hedge fund that charges the standard annual fee of 2 percent of funds under management plus 20 percent, assuming it made about 10 percent annually, cost 4 percent per dollar invested. Given the average beta of the hedge fund universe was about 0.25, this means that only 75 percent of the equity was truly active. 4 percent divided by 0.75, gives a number of 5.3 percent for the cost of alpha dollars invested.

If you are paying for alpha, active mutual funds are an expensive way to target a dollar of capital applied directly to managerial alpha. Indeed, the average hedge fund has a positive Sharpe ratio, even after accounting for beta exposure.24 In contrast, the average fund is estimated to slightly
underperform or equally perform the index, suggesting negative alpha among those managers. Targeting equity alpha is cheaper in hedge funds, and generally alpha is positive in hedge funds, in contrast to equity funds.

Thus, with funds, innovations can arise at both ends of the superficial expense spectrum. In one, low fees with an explicit goal of matching an index. In another, high fees but highly uncorrelated with the index. Both methods are an improvement over the traditional actively managed mutual fund, and underlies the growth of index and hedge funds over the past decades.

AUTOMATING ACTIVITIES

Finding alpha in the classic sense is like picking up a $20 bill. You make money without hurting anyone. In practice, your idea hurts someone else. Building a better mousetrap puts current mousetrap makers out of business. A common method for generating alpha within institutions is figuring out a cheaper way to do something, usually by eliminating the number of people needed to get something done. In manufacturing or farming, this involves machinery; in finance, it's more likely about computers and statistics.

In banking, scale has been a large part of productivity growth. Back in the bad old days, there were laws designed to keep money in the community, so that every little hamlet was autonomous, an idyllic state for those who hate capital account deficits. The problem is that you then lose out on the free lunch of diversification. In the Great Depression, many of America's thousands of banks failed, and through the uncertainty and self-fulfilling prophecies of a bank run (and, of course other factors), many more failed. Thus, in the United States, 9,000 banks failed in the 1930s, while similarly situated Canada had zero failures. The United States had around 30,000 banks compared to Canada's 10 back then, so the key difference was the greater diversification of Canadian bank portfolios.25

The elimination of unit banking—where a bank would have one branch only—and the elimination of laws in the 1990s that prohibited interstate banking, have allowed banks to diversify their asset base, taking advantage of the one free lunch in economics.

Another major scale innovation in banking was in the credit underwriting of consumers. In the old days, if a person, not a business, wanted a loan for a car, house, or some other thing, a loan officer would size you up the way people size up potential dates: qualitative, gossipy, and incomparable. I have a bunch of old credit evaluations of consumers from the 1930s that I stumbled into at KeyCorp, historical relics that were being discarded. These are listed in Figure 11.3, where the almost illegible cursive has been written over to allow readability. As seen in Figure 11.3 they make observations
Neighbors say she is a good honest girl.

"Is a bit timid. "Takes a drink" once in a long while with friends. She is not married but has a few dates. She lives in a nice mannered & well-respected family in the neighborhood. The neighborhood is clean & decent, a bit noisy. She keeps regular hours and does not stay out late to excess."

<table>
<thead>
<tr>
<th>No.</th>
<th>DESCRIPTION</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>6'3&quot; tall &amp; Black hair</td>
<td></td>
</tr>
<tr>
<td></td>
<td>freckled face</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Teeth are far apart</td>
<td></td>
</tr>
</tbody>
</table>

I saw the following receipts:

<table>
<thead>
<tr>
<th>No.</th>
<th>DESCRIPTION</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Kitchen</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Kitchen cabinet (old)</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>Oak kitchen table</td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>Oak kitchen chairs</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Pan tryer (small)</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Oven tryer</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Oven shaft</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Oak kitchen door (new)</td>
<td></td>
</tr>
</tbody>
</table>

**FIGURE 11.3** Credit Evaluations from the 1930s

that one typically makes when gossiping with friends, and are probably illegal now:

"teeth are far apart"
"takes a drink" once in a while
Not married, but has a few dates
Neighbors say she is a good-hearted girl
Another shows how the furniture inventory of a homeowner is displayed in a list, written in longhand, with no dollar equivalents. An accompanying paragraph describes, qualitatively, the pros and cons of the creditworthiness of the borrower.

The loan officer would interview neighbors, asking about everything related to the borrower's character. They would survey the neighborhood, remarking on how ordered it was. This process would take at least a day's work. Now, on one hand, this is very detailed information, and captures information that while no longer used, is probably relevant to someone's ability and willingness to pay. But on the other hand, such information does not move up the ladder well: a headquarters can hardly compare the loan quality of one branch, or region, to another, given these reports. An outsider would have no way to validate these credits, because they are based on rather subjective assessments, and the data are not organized in a database.

In the 1960s, as people in the United States began to move about, credit cards became popular, and companies sprang up to generate an alternative: the consumer credit score. These evaluations use a very limited set of inputs: payment history, length of history, size of balances, recent credit activity. The neat thing is, they are objective, and simply require that lenders cooperate by reporting balances, inquiries, and late payments to a centralized service. They then charge other lenders who inquire about these data. The net result is a more powerful, and more important, validated, credit score. This means that credit can then be sold off to others, whereas in the old system, a pile of such qualitative assessments is very opaque to outsiders, because outsiders do not know the underwriters personally, and so cannot trust their subjective assessments. Credit score data are objectively meaningful because they are validated on a lot of data over a couple of credit cycles, something that is practically impossible for the old approach. The importance of such transparency in credit markets is highlighted in the current mortgage crisis, because the complexity of the mortgage exposure, combined with changes in underwriting standards, has made it very difficult for an outsider to know the credit risk of these products, which means that when concerns are triggered, there is no easy way to assuage panicked investors.

Experts are generally dominated by statistical algorithms given a sufficient amount of objective information. Loans, which have clear success and fail outcomes, are a perfect example of the kind of activity that is inevitably taken over by computers. The qualitative approach of the personal underwriter emphasizes explanation, narrative, and anecdotes, as opposed to the quantitative focus on prediction, models, and statistics. This would all be a matter of personal preference except that statistics dominates anecdotes for the simple reason that the bottom line is a statistic—a portfolio with lower average credit losses, and so the statistical approach is amenable to
tinkering at the margin and making improvements, while the other generates few concrete tactics that one can leave to future underwriters.

Empirical evidence in favor of quantitative models versus judgment as applied to lending comes from Libby (1975). He asked 16 loan officers from small banks and 27 loan officers from large banks to judge which 30 of 60 firms would go bankrupt within three years of the financial statements with which they were presented. The loan officers requested five financial ratios on which to base their judgments. While they were correct 74 percent of the time, this was inferior to such simple alternatives as the liabilities and assets ratio.

Outside of lending, there are many examples in which models outperformed the experts, including: evaluating graduate school applicants, future student GPA, future faculty ratings, and radiology diagnostics. Why might this be the case? That is, why might statistical models dominate judgment in prediction? Paul Meehl, in his classic 1954 book, *Clinical versus Statistical Prediction*, reviewed evidence that while humans are good at finding important variables, they are not as good at integrating such diverse information sources optimally.

As part of a study for the CIA (that CIA) Rob Johnston’s “Integrating Methodologists into Teams of Substantive Experts” noted:

*The very method by which one becomes an expert explains why experts are much better at describing, explaining, performing tasks, and problem-solving within their domains than are novices, but, with a few exceptions, are worse at forecasting than actuarial tables based on historical, statistical models.*

As opposed to quantitative models that are judged solely on their calibration and power, human analysis is also focused upon presenting a compelling explanation, and focuses more deeply on explaining individual assessments as opposed to broad statistical judgments in a statistical manner, it would be unsurprising if their judgment was not optimized to statistical objectives. Improving inductive reasoning requires continual feedback, and unfortunately in most lending institutions such feedback is anecdotal, not statistical.

Thus, today a consumer loan is more powerfully analyzed, with greater transparency, and may take only 10 minutes, based on verifying you are who you say you are, and matching you with a credit score provided by one of the three national credit bureaus, and costs the institution less than an hour’s cost of a credit analyst’s time. This is an example of the powers of scale within a field, and those on the cutting edge had alpha by way of a lower cost, and greater flexibility to sell or finance their assets because outsiders
could be confident in the credit quality. The credit underwriting is not so much more powerful—after all, interviewing your neighbors probably gives a much deeper picture of one’s credit—but rather, cheap and transparent, and harder to game.

Similarly, I was part of a team at Moody’s responding to the challenge presented by KMV, a company that sold a commercial credit model based on Merton’s model of default, which essentially looked at equity as the call option on the value of the firm, with the value of liabilities as the strike price. Unlike previous statistical models, such as Altman’s seminal credit model that was calibrated on a mere 33 defaulting and nondefaulting companies, this model was calibrated with 1,000 defaults, and it is this inevitable advantage as information is warehoused in a useful database, that dooms any human expertise. KMV insisted their models were more powerful than Agency ratings, and even though Moody’s eventually acquired KMV, they have maintained this assertion, though given the large amount of revenue from their traditional credit ratings, they downplay the relevance of this by saying that traditional ratings have a different purpose.

I led the development of the private firm model for evaluating company credit when there was no equity information. Given Moody’s brand name and existing relationships with banks, we had an advantage in creating a database needed to build, test, and validate such a model, and the Original RiskCalc™ model that I created in 2000 dominated alternatives because of this advantage. The model was robust and successful because I had seen credit models that worked while I was the head of Capital Allocations at KeyCorp. These models were actually used for making credit decisions, not merely discussed in academic publications as Altman’s original corporate credit model was. A useful model is generally too parochial for academics, who emphasize elegant, general models in academic journals. That is, previous models were too enamored with impressing the wrong people, mainly academics, and so emphasized a consistent methodological innovation such as discriminate analysis, neural nets, or ordered probit, models that could be applied, in theory, to an infinite range of things. The consumer credit way was to use inputs that had theoretical reasons to be related to default (for example, previous credit delinquencies), and transform these inputs to account for obvious nonlinearities in the distribution of these variables as well as the nonlinear effects of these variables, and then nonparametrically fit these into a multinomial model. The whole process all made sense, but very much a kluge for a particular problem, not a general result so prized by academics. RiskCalc™ is now one of Moody’s best-selling services, and I imagine it will become like a credit bureau score in the future, replacing CFAs who would write the equivalent of the 1930s consumer credit narrative, filled with data but ultimately not scalable.
A floor of CFAs and MBAs looking at financial statements of companies will, I predict, be an anachronism, just as consumer credit analysts no longer interview neighbors of consumers looking for home equity loans. Productivity is about fewer people doing more, so I suspect hard times for professional underwriters, because we simply have too many people doing work that a computer can do, more cheaply, and more accurately. The key is that the firms that switch first will have a cost advantage, and thus will grow more quickly than their competitors. This is real, financial alpha.

And the same is true for trading stocks. In the old days, you called your broker, and basically gave him the job of trading out of your position. Either he would execute a large block—often at an adverse price to compensate for the risk he generates to the floor trader—or he would trade it through the day. Now we have Value Weighted Average Price algorithms, which spread a trade into little buckets of the day. An algorithm takes a desired amount, say 10,000 shares, and slices it up into 500-share chunks, spitting out trades to buy at the bid. If it is not filled in 1 minute, or if the price move away by 3 cents, it cancels, and waits 100 seconds, but ultimately spits out market orders if it does not get filled in say, 20 minutes. It repeats this all day until the order is done, and does it on hundreds of positions. You can add parameters to speed the process up by being more aggressive; add randomization, or even game-theoretic logic, so the specialist or other computers do not infer your pattern. There is no way a human has the ability to replicate the discipline, the accuracy, and the scale and scope of this program. Invariably, he would puke out an order (sell at the bid, buy at the ask), and then move on to the next one. With algorithms, you can monitor their performance in relation to the average traded price, the open, whatever, and fine-tune your algorithm. Furthermore, letting one less institution in on your trading intentions is always a good idea, especially if you are trading for an institution (that is, big orders).

Scale also works in market making, because instead of having multiple individuals post bid and asks for a small set of stocks, having a computer monitor orders, including the limit orders unfilled on the book, you can create a much more efficient market-making algorithm than any single human could do, because it can integrate relevant information more quickly than any person—looking at patterns in the order flow, behavior of comparable assets in slightly different markets, the behavior of assets that are derivative, or merely correlated.

To give an example, centralizing the portfolios of option traders so they merely have to trade, posting up bids and offers, and not worry about their net position in the underlying (delta), saves a lot of money and allows market makers to focus on their value-add. There are several risk numbers of particular interest to option traders—delta, vega, gamma, theta, adjusting
volatility parameters—and with computers, many of these issues can be handled by someone else who is aggregating risk across many different traders. I sat next to someone who managed the risk of a group of such floor traders, and if they would start talking about the Fed, or Apple’s newest product, he used to yell over the speakerphone: “Just trade!” That is, he didn’t want them to waste time thinking about these things, as a floor trader’s opinion on the market’s direction is about as useful as a plankton’s opinion on the tide.

There is considerable belief that much of the success of Renaissance Technologies is in data mining the minutia of this information and acting as an off-floor market maker. When I was at Deephaven, which was part of the market maker NITE Financial, the process of replacing humans with software was going on, whereby a trader making $500,000 could be replaced by software and do the job better. There are always places for human judgment, but such judgment becomes much more analytical, and less of the traditional trader qualities of mere aggressiveness, and one’s Rolodex. For derivative portfolios such as options, you can add the value of consolidating the hedging activities, which greatly lowers costs as well.

The bottom line is that for jobs that have objective measures of success, such as underwriting, trading, and market making, computers enable people to create databases, then build and test models that will dominate any human proficiency. Much of finance is not about constructing investment portfolios, but trading, underwriting, and other activities. The alpha generated here is no less real than that found in traditional arbitrage as in Andy Redleaf arbitraging put-call parity in the 1970s, or in fund creation, merely because it focuses on costs and benefits, and technologies, as opposed to Sharpe ratios. The emphasis of finding a better way, given one’s knowledge of costs and statistics, is the common theme in alpha finding within finance, much more so than any conception of a priced-risk factor combined with mean-variance optimization.

CONCLUSION

Even if you are not an alpha creator, you need to understand what alpha looks like so you can sell, or manage, more efficiently. Generally, alpha is created by people with the moxie to implement an idea and a thorough knowledge of current prices, as well as the nature of the product’s complements and substitutes. The hardest part is that one usually needs two things: unusual access and an idea.

There are always opportunities in markets, and while ephemeral, they last from a year to decades. Any arbitrage takes some intelligence, but these
Examples of Alpha

ideas are pretty straightforward once you understand them. Most opportunities would have been very difficult for a retail trader, paying commissions, and the bid-ask, and having the extra hurdle of seeing price quotes with a lag to what those on the exchange see. For example, many of the successful traders outlined in Jack Schwager’s Market Wizards had floor access, allowing them to make money off the bid-ask spread and other ways (front running), that your average retail investor pays. To ignore this extra edge gives the false impression that it is feasible to become wealthy trading at home in one’s pajamas, which merely encourages excessive risk taking in financial markets. Finance is a well-trodden field with many smart people looking at the same data. The most probable avenue to generate alpha is to be on the lookout for ideas, but also to put yourself into a position so that you can implement these ideas, which implies doing something as a vocation, or not doing it at all. Investing can be fun, seeing your portfolio move up and down while watching CNBC, but as competition it is less like golf and more like boxing, where dilettantes do not get handicaps—they get beat up.