

The Paradox of Dumb Money

“As they say in poker, ‘If you’ve been in the game 30 minutes and you don’t know who the patsy is, you’re the patsy.’”

—Warren Buffett (1987)

In the summer of 1968, Ed Thorp, a young math professor at the University of California, Irvine (UCI), and author of *Beat the Market: A Scientific Stock Market System* (1967), accepted an invitation to spend the afternoon playing bridge with Warren Buffett, the not-yet-famous “value” investor. Ralph Waldo Gerard hosted the game. Gerard was an early investor in Buffett’s first venture, Buffett Partners, and the dean of the Graduate School at UCI, where Thorp taught. Buffett was liquidating the partnership, and Gerard needed a new manager for his share of the proceeds. Gerard wanted Buffett’s opinion on the young professor and the unusual “quantitative” investment strategy for which he was quietly earning a reputation among the members of the UCI community.

Gerard had invested with Buffett at the recommendation of a relative of Gerard’s who had taught Buffett at Columbia University: the great value investment philosopher, Benjamin Graham. Graham had first published the value investor’s bible, *Security Analysis*, along with David Dodd, in 1934.¹ He was considered the “Dean of Wall Street,” and regarded Buffett as his star pupil. Graham’s assessment would prove to be prescient.

By the time Thorp met Buffett in 1968, Buffett had established an exceptional investment record. He had started Buffett Partners 12 years earlier, in 1956, at the tender age of 26, with initial capital of just \$100,100. (Buffett joked that the \$100 was his contribution.) By 1968, Buffett Partners controlled \$100 million in capital, and Buffett’s share of that was \$25 million.²

For the 12 years between 1956 and 1968, Buffett had compounded the partnership's capital at 30 percent per year before his fees, which were 25 percent of the gain over 6 percent per year. Investors like Gerard had compounded at an average of 24 percent a year. Before taxes, each original dollar invested in Buffett's partnership had grown to more than \$13. Each of Buffett's own dollars, growing at the greater prefee annual rate of 30 percent became before taxes over \$23. By 1968, however, Buffett was having difficulty finding sufficiently undervalued securities for the partnership, and so had decided to wind it up. This had led Gerard to find a new manager, and Gerard hoped Thorp was the man. He wanted to know if Thorp's unusual quantitative strategy worked, and so, at Gerard's behest, Thorp found himself sitting down for a game of bridge with Buffett.

Buffett is a near world-class bridge player. Sharon Osberg, international bridge player and regular professional partner to Buffett, says, "He can play with anyone. It's because of his logic, his ability to solve problems and his concentration."³ Says Buffett, "I spend 12 hours a week—a little over 10 percent of my waking hours—playing the game. Now I am trying to figure out how to get by on less sleep in order to fit in a few more hands."⁴ Buffett presented a daunting opponent. Thorp observed of Buffett's bridge playing⁵:

Bridge players know that bridge is what mathematicians call a game of imperfect information. The bidding, which precedes the play of the cards, conveys information about the four concealed hands held by the two pairs of players that are opposing each other. Once play begins, players use information from the bidding and from the cards as they are played to deduce who holds the remaining as yet unseen cards. The stock market also is a game of imperfect information and even resembles bridge in that they both have their deceptions and swindles. Like bridge, you do better in the market if you get more information, sooner, and put it to better use. It's no surprise then that Buffett, arguably the greatest investor in history, is a bridge addict.

Thorp was no stranger to the card table either. Before he figured out how to beat the market, Thorp wrote *Beat the Dealer*, the definitive book on blackjack card counting. William Poundstone recounts the story of Thorp's foray into card counting in his book, *Fortune's Formula*.⁶ In 1958, Thorp had read an article by mathematician Roger Baldwin, who had used U.S. Army "computers"—which actually meant "adding machines" or the people who operated them—to calculate the odds of various blackjack strategies in an effort to find an optimal strategy. Over three years, he and three associates found that by using an unusual strategy they could reduce the

house edge in blackjack to 0.62 percent. Amazingly, prior to their paper, nobody, including the casinos, knew the real advantage held by the house. There were simply too many permutations in a card deck of 52 to calculate the casino's edge. "Good" players of blackjack, other writers had claimed, could get the house's edge down to 2 or 3 percent. Baldwin's strategy, by reducing the house edge to 0.62 percent, was a huge leap forward. The only problem, as far as Thorp could see, was that Baldwin's strategy still lost money. He was convinced he could do better.

Thorp's key insight was that at the time blackjack was played using only one deck and it was not shuffled between hands. In the parlance of the statistician, this meant that blackjack hands were not "independent" of each other. Information gleaned in earlier hands could be applied in subsequent hands. For example, in blackjack, aces are good for the player. If the dealer deals a hand with three aces, the player knows that only one ace remains in the deck. This information would lead the player to view the deck as being less favorable, and the player could adjust his or her betting accordingly. Thorp used MIT's mainframe computer to examine the implications of his observation and found something completely counterintuitive—the "five" cards had the most impact on the outcome of the hands remaining in the deck. Fives are bad for the player and good for the house. Thorp realized that by simply keeping track of the five cards, the player could determine the favorability or otherwise of the cards remaining in the deck. Thorp found that his improved strategy gave the player an edge of 0.13 percent. That small edge, Thorp reasoned, given enough hands, could add up to a lot of money. He published his new strategy first in a paper and then subsequently as *Beat the Dealer* in 1962, which went on to become a classic in gambling literature. The book detailed how Thorp had used his card-counting strategy for a period of several years, making \$25,000 in the process. The casinos didn't like players counting cards to gain an edge. They immediately started taking "counter-measures," including adding more decks, randomly shuffling the cards, using "mechanics" (dealers who cheated by manipulating the cards in the deck), threatening Thorp with physical harm, and then simply barring him from the casinos. By 1964, Thorp no longer found blackjack fun or profitable. He had found a new obsession, the stock market, and he was already hunting for an edge.

Thorp started working on the key element of what would become his quantitative investment strategy when he moved to UCI in 1964.⁷ There he met Sheen Kassouf, another professor at UCI, who had been working on the same problem: how to value a warrant, an unusual security that converted into stock on a certain event. They started meeting together once a week in an effort to solve the warrant valuation conundrum. Thorp found the answer in an unlikely place. In a collection of essays called *The Random*

Character of Stock Market Prices (1964), Thorp read the English translation of a French dissertation written in 1900 by a student at the University of Paris, Louis Bachelier. Bachelier's dissertation unlocked the secret to valuing warrants: the so-called "random walk" theory. As the name suggests, the "random walk" holds that the movements made by security prices are random. While it might seem paradoxical, the random nature of the moves makes it possible to *probabilistically* determine the future price of the security.

The implications of the random walk theory are profound, and they weren't lost on Thorp. He saw that he could apply the theory to handicap the value of the warrant. Where the warrant's price differed from Thorp's probabilistic valuation, Thorp recognized that an opportunity existed for him to trade the warrant and the underlying stock and to profit from the differential. While any given warrant might expire worthless, given a large enough portfolio of warrants Thorp was likely to make money. These two insights—a probabilistic approach to valuation and the construction of portfolios large enough to capture the probabilities—formed the bulwark of Thorp's "scientific stock market system," one of the most consistently profitable trading strategies ever developed. In 1965, Thorp wrote in a letter to a friend about his strategy⁸:

I have finally hit pay dirt with the stock market. I have constructed a complete mathematical model for a small section (epsilon times "infinity" isn't so small, though) of the stock market. I can prove from the model that the expected return is 33 percent per annum, and that the empirical assumptions of the model can be varied within wide limits (well beyond those dictated by skepticism) without affecting this figure much. Past records corroborate the 33 percent figure. It assumes I revise my portfolio once a year. With continuous attention to the portfolio the rate of return appears to exceed 50 percent gross per year. But I haven't finished with the details of that, so I can only be sure of the lower rate at present. A major portion of my modest resources has been invested for several months. We once "set" as a tentative first goal the doubling of capital every two years. It isn't far away now.

As he had with his blackjack betting system, Thorp was again seeking to steadily exploit a small edge—epsilon times "infinity"—to beat the market.

Thorp put the strategy to work in his hedge fund, Princeton-Newport Partners, which went on to become one of the most successful ever formed. For the 20 years from its inception in 1969, the fund compounded at

15.1 percent annually after fees. By the time it was wound up, Princeton-Newport was managing over \$270 million. Each dollar invested in the fund in 1969 had grown to \$14.78. By way of comparison, the Standard & Poor's (S&P) 500 averaged 8.8 percent annually over the same period, which means that Princeton-Newport outperformed the market by more than 6 percent per year. But that's only half the story. The fund was much less volatile than the market itself. In fact, Princeton-Newport never had a down year or down *quarter*. Thorp closed Princeton-Newport in 1988 following an investigation by Rudy Giuliani into stock parking on behalf of Drexel Burnham Lambert in which Thorp was not accused of any wrongdoing.

Unable to stay away, Thorp relaunched in August 1994 as Ridgeline Partners. From the get-go Ridgeline outperformed Princeton-Newport, averaging 18 percent per year after fees. In 1998, Thorp reported that since the inception of Princeton-Newport in 1969 he had returned 20 percent per year for nearly 30 years, with a standard deviation of just 6 percent⁹:

To help persuade you that this may not be luck, I estimate that ... I have made \$80 billion worth of purchases and sales ("action," in casino language) for my investors. This breaks down to something like one and a quarter million individual "bets" averaging \$65,000 each, with on average hundreds of "positions" in place at any one time. Over all, it would seem to be a moderately "long run" with a high probability that the excess performance is more than chance.

As Buffett and Thorp sat down for the 1968 game of bridge, it appeared that a deep philosophical chasm existed between each man's investment strategies. Buffett, the value investor, used fundamental analysis on individual securities to carefully calculate their "intrinsic value," and find those trading at a market price well below that intrinsic value. Thorp, the quantitative investor, valued securities on a probabilistic basis and relied on the statistical phenomenon known as "the law of large numbers"—the law states that the more observations we make, the closer our sample will be to the population, and hence greater the certainty of our prediction—to construct portfolios of securities that would, in aggregate, outperform the market. There were other apparently irreconcilable differences. In his 1992 Berkshire Hathaway, Inc. Chairman's Letter,¹⁰ Buffett said of value investing:

The investment shown by the discounted-flows-of-cash calculation to be the cheapest is the one that the investor should purchase—irrespective of whether the business grows or doesn't, displays volatility or smoothness in its earnings, or carries a high price or low in relation to its current earnings and book value.

Thorp had a different view of value investing, spelled out in *Beat the Market*¹¹:

My attraction to fundamental analysis weakened further as practical difficulties appeared. It is almost impossible to estimate earnings for more than a year or two in the future. And this was not the least difficulty. After purchasing an undervalued stock it is essential that others make similar calculations so that they will either purchase or wish to purchase it, driving its price higher. Many “undervalued” stocks remain bargains for years, frustrating an owner who may have made a correct and ingenious calculation of the future prospects.

Buffett spoke in his 1987 Shareholder Letter¹² about the use of computer programs in the investment process:

In my opinion, investment success will not be produced by arcane formulae, computer programs or signals flashed by the price behavior of stocks and markets. Rather an investor will succeed by coupling good business judgment with an ability to insulate his thoughts and behavior from the super-contagious emotions that swirl about the marketplace.

Thorp countered in the introduction to *Beat the Market*¹³:

We have used mathematics, economics, and electronic computers to prove and perfect our theory. After reading dozens of books, investigating advisory services and mutual funds, and trying and rejecting scores of systems, we believe that ours is the first scientifically proven method for consistent stock market profits.

While the philosophical differences between Thorp and Buffett were vast, over a game of bridge they were able to find common ground chatting about their shared interests in statistics and finance. For his part, Thorp was thoroughly charmed by Buffett, writing later that Buffett was a “high speed talker with a Nebraska twang and a steady flow of jokes, anecdotes and clever sayings.”¹⁴ He also observed that Buffett had a “remarkable facility for remembering and using numerical information, plus an adeptness in mental calculation.” At the end of the evening, Thorp told his wife that he thought Buffett would one day be the richest man in America. Buffett’s subsequent trajectory through life is well chronicled, and Thorp’s prediction

has been true, or within spitting distance, since the 1990s. Buffett's opinion on Thorp is unfortunately lost in the sands of time. We can, however, guess that it was favorable. Gerard, who had made a fortune with Buffett, went on to invest with Thorp. As we have seen, it turned out to be another great investment for him.

At first blush, each man's strategy seems diametrically opposed to the other, and irretrievably so. They agreed, however, on one very important point: both believed it was possible to outperform the stock market, a belief that flew in the face of the efficient market hypothesis. While it is true that Thorp's strategy was grounded in the random walk, a key component of the efficient market hypothesis, he disagreed with the efficient market believers that it necessarily implied that markets were efficient. Indeed, Thorp went so far as to call his book *Beat the Market*. Buffett also thought the efficient market hypothesis was nonsense, writing in his 1988 Shareholder Letter¹⁵:

This doctrine [the efficient market hypothesis] became highly fashionable—indeed, almost holy scripture in academic circles during the 1970s. Essentially, it said that analyzing stocks was useless because all public information about them was appropriately reflected in their prices. In other words, the market always knew everything. As a corollary, the professors who taught EMT said that someone throwing darts at the stock tables could select a stock portfolio having prospects just as good as one selected by the brightest, most hard-working security analyst. Amazingly, EMT was embraced not only by academics, but also by many investment professionals and corporate managers as well. Observing correctly that the market was frequently efficient, they went on to conclude incorrectly that it was always efficient. The difference between these propositions is night and day.

On this most important point, Buffett and Thorp agreed: the market was beatable, if you held an edge.

VALUE STRATEGIES BEAT THE MARKET

[It] is extraordinary to me that the idea of buying dollar bills for 40 cents takes immediately to people or it doesn't take at all. It's like an inoculation. If it doesn't grab a person right away, I find that you can talk to him for years and show him records, and it

doesn't make any difference. They just don't seem able to grasp the concept, simple as it is.

—Warren Buffett, “The Superinvestors of Graham-and-Doddsville”¹⁶

Corporate gold dollars are now available in quantity at 50 cents and less—but they do have strings attached.

—Benjamin Graham, “Should Rich but Losing Corporations Be Liquidated?”¹⁷

It is difficult to overstate Benjamin Graham's impact on Wall Street. He arrived there in 1914 fresh from Columbia College, where he just had turned down offers to undertake doctorates in the philosophy, mathematics, and English departments. He was employed on Wall Street as a “statistician” (as analysts were then known) and observed in this role that the “mass of information” available from the data services like Moody's and Standard Statistics was “largely going to waste in the area of common-stock analysis.” Graham found Wall Street “virgin territory for examination by a genuine, penetrating analysis of security values.”¹⁸

Graham wasn't exaggerating about the lack of genuine analysis on Wall Street. At the time, stock market statisticians had a deservedly poor reputation. A 1932 paper by Alfred Cowles III had asked, “Can stock market forecasters forecast?” and concluded that they could not. With the aid of an IBM punch card machine, Cowles examined the investment performance of 16 statistical services, 25 insurance companies, 24 forecasting letters, and the Dow Theory editorials of William Peter Hamilton over the period from December 1903 to December 1929. Only a handful beat the market. Worse, Cowles concluded of the performances of those few who had beaten the market that their results were “little, if any, better than what might be expected to result from pure chance.”¹⁹

Graham took it upon himself to form a rigorous analytical framework for the scrutiny of securities. In 1927, he started teaching his philosophy at Columbia in a night class called “Security Analysis.” By 1934, Graham, with the assistance of David Dodd, a student who had taken his first night class in 1927 and was by 1934 a Columbia Business School professor, converted his lectures into *Security Analysis*, his magnum opus.

Graham and Dodd's 1934 publication of *Security Analysis* laid out the first well-reasoned and comprehensive approach to analyzing securities. As each new edition was published, and with the subsequent publication of *The Intelligent Investor* in 1949,²⁰ Graham refined his approach, but the philosophy remained the same: equity securities should be regarded as a part share in a business. An investor should thoroughly analyze a security's

financial statements to determine a conservative valuation for the security. If the price of the security is available in the market at a sufficient discount to the rough valuation to provide a margin of safety, the security could be purchased. This was “value” investing. More than any other book, *Security Analysis* ushered in the era of the professional financial analyst. But does it work? And how can we know?

The arguments for value investing fall into two categories: logical and empirical. The logical argument is that value investing seeks to exchange one sum of value (money) for a greater sum of value (the “intrinsic value” of the security), which Buffett more pithily states as “price is what you pay; value is what you get.”²¹ Value investors seek to pay less than the security’s value. They realize the profit when the price reverts to the value, but the gain is made at the time of purchase because the purchaser has exchanged a smaller store of value for a greater one. Implicit in this assertion is the concept that price and value are distinct. There are many examples of stocks trading at a discount to intrinsic value, but the most transparent case is in a liquidation scenario. In the 1934 edition of *Security Analysis*, Graham argued that the phenomenon of a stock selling persistently below its liquidation value was “fundamentally illogical.” In Graham’s opinion, it meant that the stock is too cheap. In a liquidation, an investor can identify a transparent difference between market value and intrinsic value. After all other liabilities have been met, common stockholders are the residual claimants to the company’s assets. As Seth Klarman, legendary chairman of the Baupost Group, elegantly demonstrated in his hugely popular out-of-print 1991 book *Margin of Safety*²²:

A liquidation is, in a sense, one of the few interfaces where the essence of the stock market is revealed. Are stocks pieces of paper to be endlessly traded back and forth, or are they proportional interests in underlying businesses? A liquidation settles this debate, distributing to owners of pieces of paper the actual cash proceeds resulting from the sale of corporate assets to the highest bidder. A liquidation thereby acts as a tether to reality for the stock market, forcing either undervalued or overvalued share prices to move into line with actual underlying value.

To say that price and value are distinct in theory is not to say that we can profit from this distinction in practice. The problem is that in the real world we cannot observe intrinsic value. Rather we must estimate it through some proxy, a model populated with imperfect, backward-looking information, and must make certain assumptions about the future. Change the assumptions, and we change our estimate of “intrinsic value.” Klarman

discusses the use of the “net current asset value” or “net-net working capital” model to calculate liquidation value²³:

In approximating the liquidation value of a company, some value investors, emulating Benjamin Graham, calculate “net-net working capital” as a shortcut. Net working capital consists of current assets (cash, marketable securities, receivables, and inventories) less current liabilities (accounts, notes, and taxes payable within one year). Net-net working capital is defined as net working capital minus all long-term liabilities. Even when a company has little ongoing business value, investors who buy at a price below net-net working capital are protected by the approximate liquidation value of current assets alone.

All well and good, but let’s not forget that this assessment must be made with imperfect information. There are a number of assumptions embedded in the model, which amply demonstrates why the calculation is often difficult²⁴:

As long as working capital is not overstated and operations are not rapidly consuming cash, a company could liquidate its assets, extinguish all liabilities, and still distribute proceeds in excess of the market price to investors. Ongoing business losses can, however, quickly erode net-net working capital. Investors must therefore always consider the state of a company’s current operations before buying. Investors should also consider any off-balance sheet or contingent liabilities that might be incurred in the course of an actual liquidation, such as plant closing and environmental laws.

Critics of this approach—typically adherents to the *efficient market theory*—focus on the deficiency of the information available to investors. They argue that price and value cannot be distinct in practice because all information about a security’s value is immediately incorporated into the price. Any new information that might affect the value of a security is immediately reflected in its price by arbitrageurs trading away the differential. It is therefore not possible to profit from the difference. This argument reminds us of the old joke about the two professors of finance who while walking one day spot a 10-dollar note lying on the ground. One professor turns to the other and says, “Is that a 10-dollar note lying on the ground?” The other says, “Impossible. If that were a 10-dollar note, someone would have picked it up already.”

The other argument in favor of value investing is empirical. Numerous studies demonstrate that a variety of price ratios find stocks that outperform the broader market. In Chapters 7 and 8, we examine in detail the performance of various value metrics. Figure 1.1 sets out a brief graphical overview of the performance of the cheapest stocks according to common fundamental price ratios, such as the price-to-earnings (P/E) ratio, the price-to-book (P/B) ratio, and the EBITDA enterprise multiple (total enterprise value divided by earnings before interest, taxes, depreciation, and amortization, or TEV/EBITDA).

As Figure 1.1 illustrates, value investing according to simple fundamental price ratios has cumulatively beaten the S&P 500 over almost 50 years.

Table 1.1 shows some additional performance metrics for the price ratios. The numbers illustrate that value strategies have been very successful (Chapter 7 has a detailed discussion of our method of our investment simulation procedures).

The counterargument to the empirical outperformance of value stocks is that these stocks are inherently more risky. In this instance, risk is defined as the additional volatility of the value stocks. Prolific finance researchers and founders of modern quantitative asset management analysis Eugene Fama and Ken French made this argument most forcefully in their 1992 paper, “The Cross-Section of Expected Stock Returns.” Behavioral finance researchers Joseph Lakonishok, Andrei Shleifer, and Robert Vishny argue

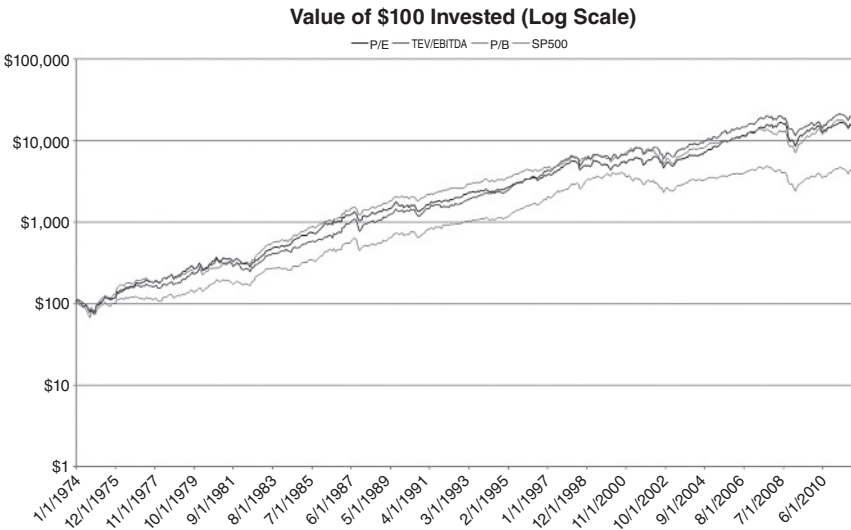


FIGURE 1.1 Cumulative Returns to Common Price Ratios

TABLE 1.1 Long-Term Performance of Common Price Ratios (1964 to 2011)

	P/E	Enterprise Multiple	P/B	S&P 500 TR
Compound Annual Growth Rate (CAGR)	12.44%	13.72%	13.11%	9.52%
Standard Deviation	17.62%	17.25%	17.39%	15.19%
Downside Deviation	12.17%	11.49%	11.12%	10.66%
Sharpe Ratio	0.46	0.53	0.50	0.33
Sortino Ratio	0.68	0.82	0.80	0.50
Worst Drawdown	-49.01%	-43.45%	-49.20%	-50.21%
Worst Month Return	-22.02%	-18.66%	-22.37%	-21.58%
Best Month Return	25.75%	16.95%	28.59%	16.81%
Profitable Months	60.42%	62.85%	61.63%	60.94%

in their 1994 paper, “Contrarian Investment, Extrapolation, and Risk,”²⁵ that value strategies produce better returns, not because they are fundamentally riskier, but because they are contrarian to the “naïve” strategies followed by other investors. Naïve investors extrapolate poor earnings performance too far into the future, assume a downward trend in stock prices will persist or simply overreact to bad news, leading them to oversell stocks to the point that they are undervalued. Contrarian investors bet against these naïve strategies, investing disproportionately in underpriced stocks and, consequently, beating the market. It might be more accurate to say that individual value stocks *appear* to be more risky to the naïve investor, but are, in the aggregate, no more risky than other stocks. We’re not going to linger on the arguments. Instead, we’ll give the last word to Buffett, who said in 1985²⁶:

Most institutional investors in the early 1970s, on the other hand, regarded business value as of only minor relevance when they were deciding the prices at which they would buy or sell. This now seems hard to believe. However, these institutions were then under the spell of academics at prestigious business schools who were preaching a newly-fashioned theory: the stock market was totally efficient, and therefore calculations of business value—and even thought, itself—were of no importance in investment activities. (We are enormously indebted to those academics: what could be

more advantageous in an intellectual contest—whether it be bridge, chess, or stock selection than to have opponents who have been taught that thinking is a waste of energy?)

Graham's Simple Quantitative Value Strategy

Security Analysis in 1934 was a weighty and ambitious tome focused on the analysis of individual securities. Graham and Dodd wrote in the preface to the original edition²⁷:

The scope of the work is wider than its title may suggest. It deals not only with methods of analyzing individual issues, but also with the establishment of general principles of selection and protection of security holdings.

...

[We] have stressed the technique of discovering bargain issues beyond its relative importance in the entire field of investment, because in this activity the talents peculiar to the securities analyst find perhaps their most fruitful expression.

Some 40 years after the publication of *Security Analysis*, Graham modified his approach in an important way. When asked in one of his last interviews whether he still selected stocks by carefully studying individual issues, Graham responded²⁸:

I am no longer an advocate of elaborate techniques of security analysis in order to find superior value opportunities. This was a rewarding activity, say, 40 years ago, when our textbook “Graham and Dodd” was first published; but the situation has changed a great deal since then. In the old days any well-trained security analyst could do a good professional job of selecting undervalued issues through detailed studies; but in the light of the enormous amount of research now being carried on, I doubt whether in most cases such extensive efforts will generate sufficiently superior selections to justify their cost. To that very limited extent I’m on the side of the “efficient market” school of thought now generally accepted by the professors.

Instead, Graham promoted a highly simplified approach that relied for its results on the performance of the portfolio as a whole rather than on the selection of individual issues. Graham believed that such an approach

“[combined] the three virtues of sound logic, simplicity of application, and an extraordinarily good performance record.”

Graham said of his simplified value investment strategy²⁹:

What's needed is, first, a definite rule for purchasing which indicates a priori that you're acquiring stocks for less than they're worth. Second, you have to operate with a large enough number of stocks to make the approach effective. And finally you need a very definite guideline for selling.

Graham proposed two broad approaches, the first of which he had discussed in some detail in the original edition of *Security Analysis*—“net current asset value”³⁰:

My first, more limited, technique confines itself to the purchase of common stocks at less than their working-capital value, or net-current-asset value, giving no weight to the plant and other fixed assets, and deducting all liabilities in full from the current assets. We used this approach extensively in managing investment funds, and over a 30-odd year period we must have earned an average of some 20 per cent per year from this source. For a while, however, after the mid-1950's, this brand of buying opportunity became very scarce because of the pervasive bull market. But it has returned in quantity since the 1973-74 decline. In January 1976 we counted over 300 such issues in the Standard & Poor's Stock Guide—about 10 per cent of the total. I consider it a foolproof method of systematic investment—once again, not on the basis of individual results but in terms of the expectable group outcome.

While this strategy was “almost unfailingly dependable and satisfactory,” it was “severely limited in its application” because the stocks were too small and infrequently available. Graham had a second strategy with an application much wider than the first. Based on his own research over a 50-year period, Graham believed that a “portfolio put together using such an approach would have gained twice as much as the Dow Jones Industrial Average over the long run,” or about 15 percent a year or better.

So what did Graham believe was the simplest way to select value stocks? He recommended that an investor create a portfolio of a minimum of 30 stocks meeting specific price-to-earnings criteria (below 10) and specific debt-to-equity criteria (below 50 percent) to give the “best odds statistically,” and then hold those stocks until they had returned 50 percent, or, if a stock hadn't met that return objective by the “end of the second calendar year from

the time of purchase, sell it regardless of price.” Graham said that his research suggested that this formula returned approximately 15 percent per year over the preceding 50 years. He cautioned, however, that an investor should not expect 15 percent every year. The minimum period of time to determine the likely performance of the strategy was five years.

Graham’s simple strategy sounds almost too good to be true. Sure, this approach worked in the 50 years prior to 1976, but how has it performed in the age of the personal computer and the Internet, where computing power is a commodity, and access to comprehensive financial information is as close as the browser? We decided to find out. Like Graham, we used a price-to-earnings ratio cutoff of 10, and we included only stocks with a debt-to-equity ratio of less than 50 percent. We also apply his trading rules, selling a stock if it returned 50 percent or had been held in the portfolio for two years.

Figure 1.2 shows the cumulative performance of Graham’s simple value strategy plotted against the performance of the S&P 500 for the period 1976 to 2011. Amazingly, Graham’s simple value strategy has continued to outperform.

Table 1.2 presents the results from our study of the simple Graham value strategy. Graham’s strategy turns \$100 invested on January 1, 1976, into \$36,354 by December 31, 2011, which represents an average yearly compound rate of return of 17.80 percent—outperforming even

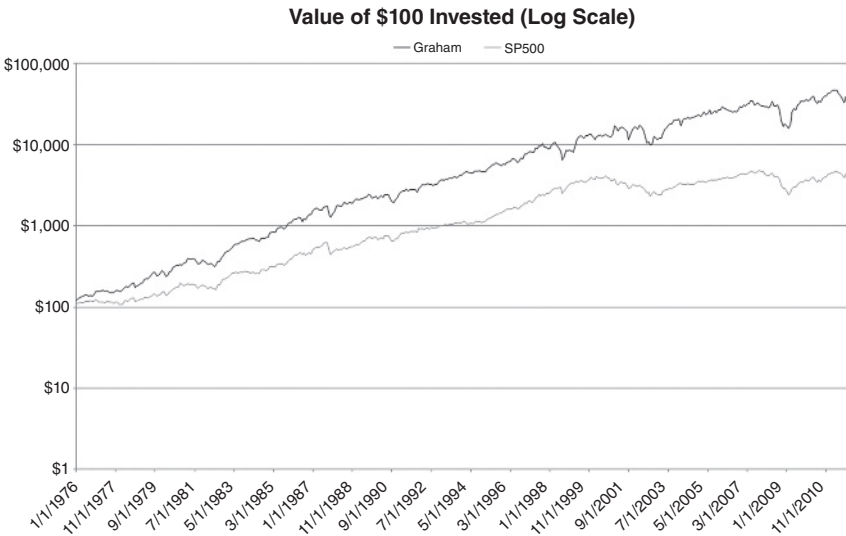


FIGURE 1.2 Graham Simple Value Strategy Performance Chart (1976 to 2011)

TABLE 1.2 Performance of Graham's Simple Quantitative Value Strategy (1976 to 2011)

	Graham	S&P 500 TR
CAGR	17.80%	11.05%
Standard Deviation	23.92%	15.40%
Downside Deviation	16.26%	11.15%
Sharpe Ratio	0.59	0.42
Sortino Ratio (MAR = 5%)	0.88	0.60
Worst Drawdown	-54.61%	-50.21%
Worst Month Return	-28.84%	-21.58%
Best Month Return	40.79%	13.52%
Profitable Months	59.95%	61.57%
Rolling 5-Year Win	—	90.35%
Rolling 10-Year Win	—	95.53%

Graham's estimate of approximately 15 percent per year. This compares favorably with the performance of the S&P 500 over the same period, which would have turned \$100 invested on January 1, 1976, into \$4,351 by December 31, 2011, an average yearly compound rate of return of 11.05 percent. The performance of the Graham strategy is attended by very high volatility, 23.92 percent versus 15.40 percent for the total return on the S&P 500. The strategy would also have required a cast-iron gut because only a few stocks qualified at any given time, and the back-test assumed that we invested all our capital in those stocks. The Graham portfolio averaged 21 positions for the full period, but Figure 1.3 illustrates that the portfolio was frequently heavily concentrated in only very few stocks, and was fully invested in only one security in 2004. In practice, portfolio risk considerations would prevent us from investing "all in" on one stock.

Table 1.2 sets out the performance statistics for Graham's simple quantitative strategy over the period from 1976 to 2011.

Graham said that the minimum period to determine the likely performance of his strategy was five years. Table 1.2 highlights that Graham's simple strategy beats the S&P 500 90.35 percent of rolling 5-year periods, and 95.53 percent of rolling 10-year periods. Figures 1.4 (a) and (b) show

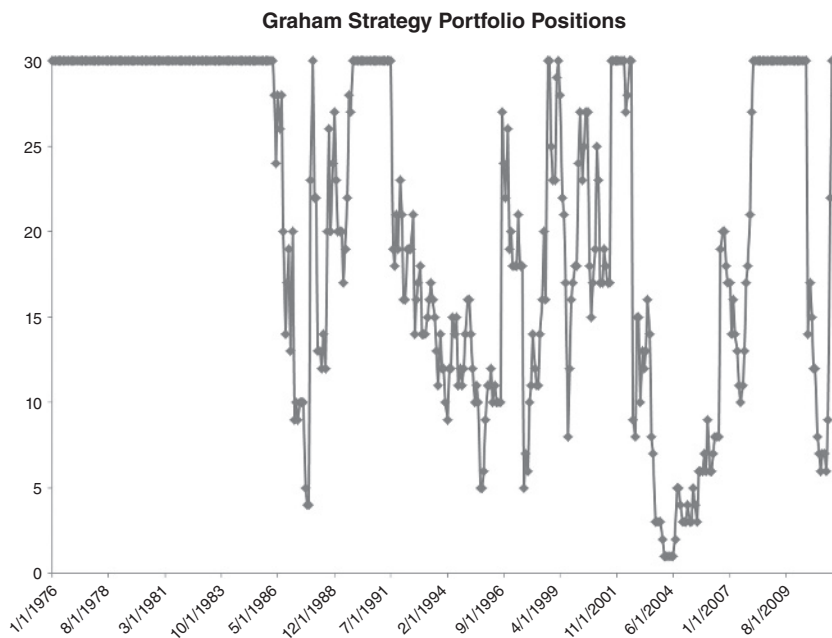


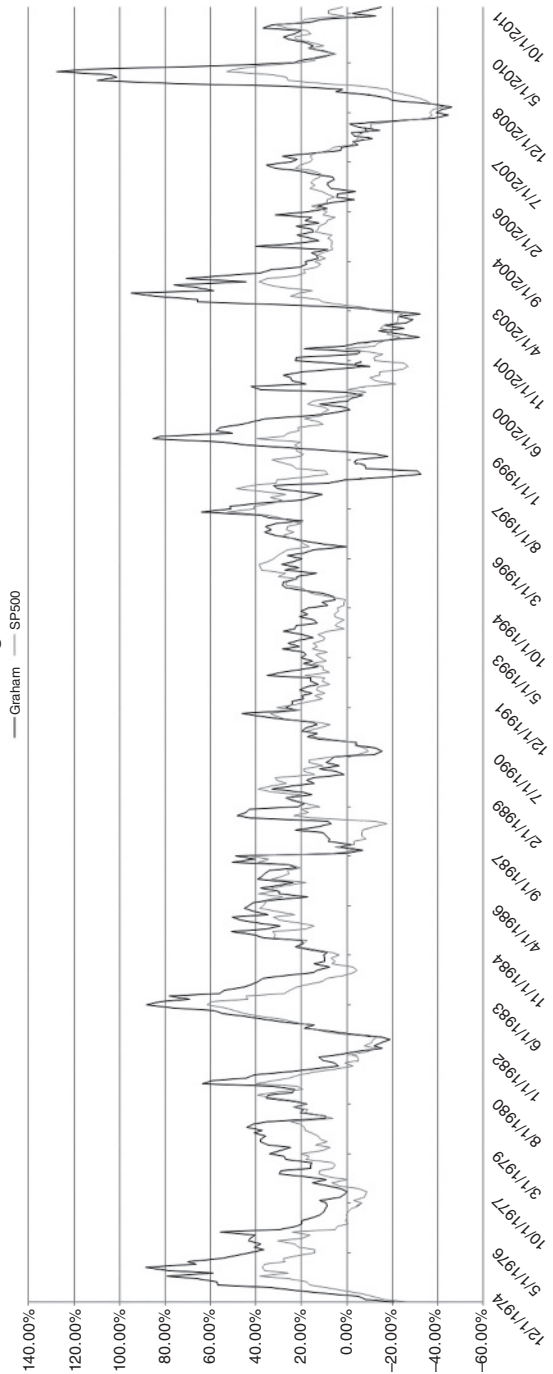
FIGURE 1.3 Graham Strategy Portfolio Holdings over Time (1976 to 2011)

the rolling 1-, 5-, and 10-year returns for the simple Graham strategy for the period 1976 to 2011. As the figures illustrate, Graham's simple value strategy has underperformed in several periods; however, over long periods of time, it has proven to perform exceptionally well and in accordance with Graham's prediction.

The evidence suggests that Graham's simplified approach to value investment continues to outperform the market. It's useful to consider why. At a superficial level, it's clear that some proxy for price—like a P/E ratio below 10—combined with some proxy for quality—like a debt-to-equity ratio below 50 percent—is predictive of future returns. But is something else at work here that might provide us with a deeper understanding of the reasons for the strategy's success? Is there some other reason for its outperformance beyond simple awareness of the strategy? We think so.

Graham's simple value strategy has concrete rules that have been applied consistently in our study. Even through the years when the strategy underperformed the market, and even though it forced us to put all our capital into one stock in 2004, our study assumed that we continued to apply it, regardless of how discouraged or scared we might have felt had we actually used it during the periods when it underperformed the market. Is

1-Year Rolling CAGRs



(a)

FIGURE 1.4(a) One-Year Rolling Period Performance Statistics: Graham Strategy (1976 to 2011)

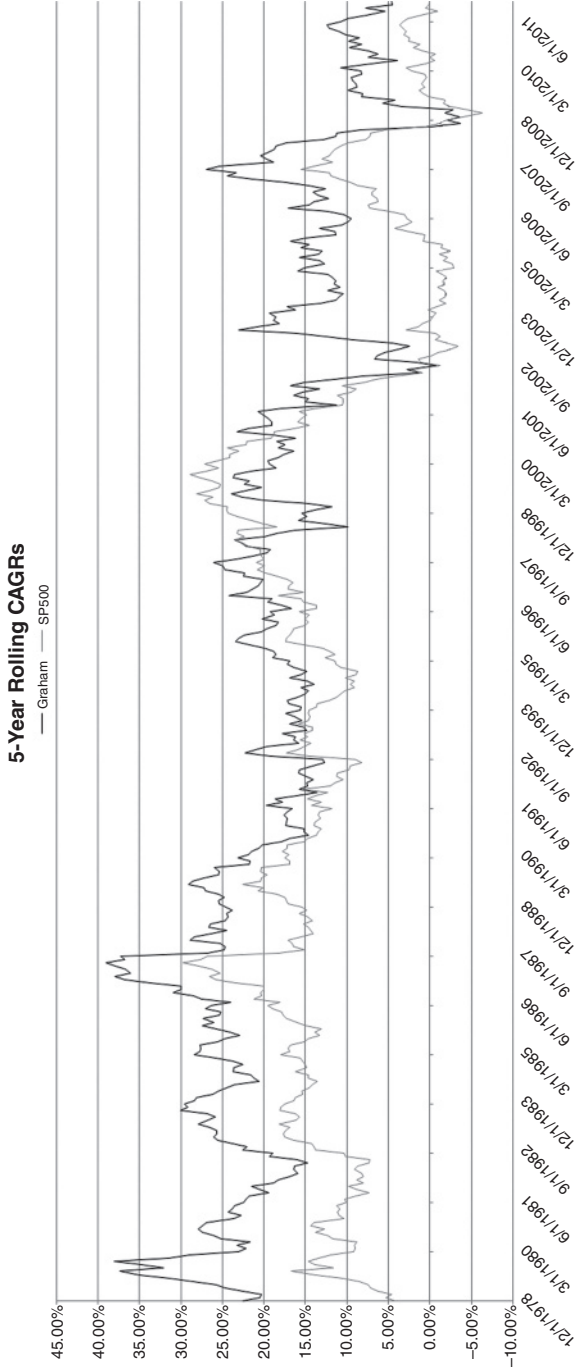


FIGURE 1.4(b) Five-Year Rolling Period Performance Statistics: Graham Strategy (1976 to 2011)

10-Year Rolling CAGRs

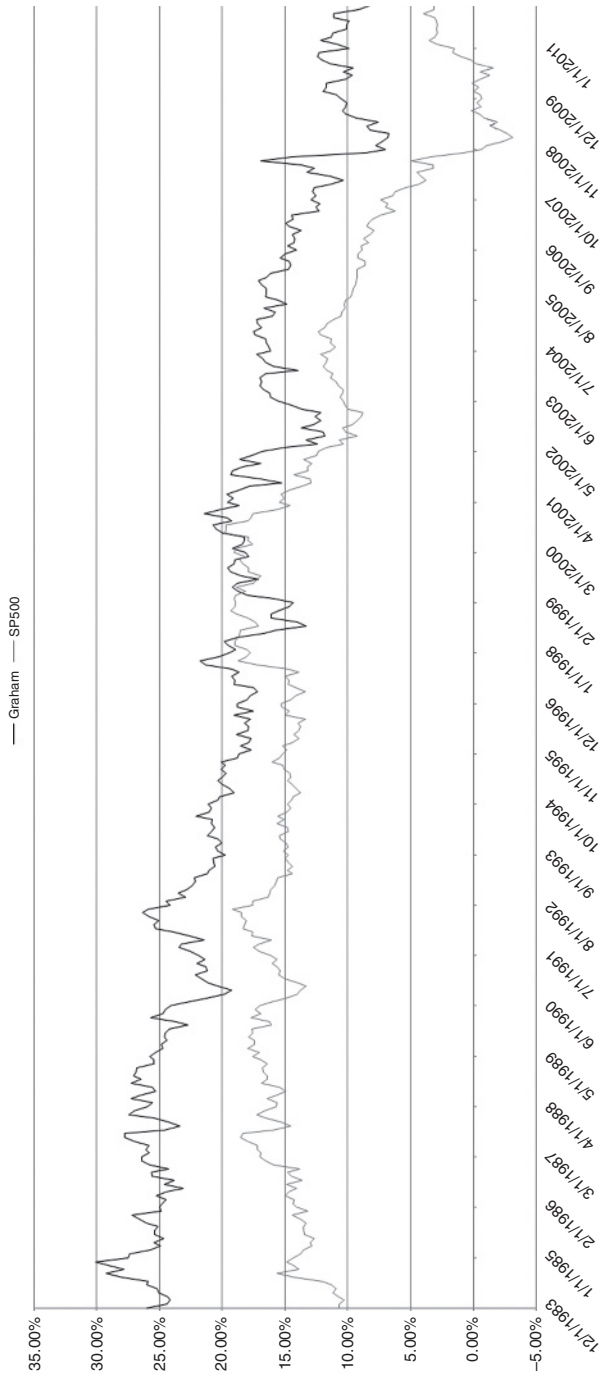


FIGURE 1.4(c) Ten-Year Rolling Period Performance Statistics: Graham Strategy (1976 to 2011)

it possible that the very consistency of the strategy is an important reason for its success? We believe so. A value investment strategy might provide an edge, but some other element is required to fully exploit that advantage. Warren Buffett and Charlie Munger believe that the missing ingredient is *temperament*. Says Buffett, “Success in investing doesn’t correlate with IQ once you’re above the level of 125. Once you have ordinary intelligence, what you need is the temperament to control the urges that get other people into trouble in investing.”³¹

HOW QUANTITATIVE INVESTING PROTECTS AGAINST BEHAVIORAL ERRORS

In the decade to December 31, 2009, the *Wall Street Journal* reported that the best-performed U.S. diversified stock mutual fund according to fund-tracker *Morningstar* was Ken Heebner’s *CGM Focus Fund*. Over the decade, the fund had gained 18.2 percent annually, beating its closest rival by 3.4 percent per year, which is exceptional. The typical investor in Heebner’s fund, however, *lost* 11 percent annually. Investor returns, also known as “dollar-weighted returns,” take into account the capital flowing into and out of the fund as investors buy and sell. The investor returns were lower than the fund’s total returns because investors bought into the fund after it had a strong run and then sold as it hit bottom. Heebner’s fund surged 80 percent in 2007, and then investors poured in \$2.6 billion. The following year, the fund sunk 48 percent, and investors yanked out more than \$750 million. Said Heebner³²:

A huge amount of money came in right when the performance of the fund was at a peak. I don’t know what to say about that. We don’t have any control over what investors do.

This behavior caused the investor returns in Heebner’s fund to be among the worst of any fund tracked by *Morningstar*. Amazingly, this means that the worst investor returns were found in the decade’s best-performing fund. We are each our own worst enemy.

Reason Is the Slave of the Passions

Behavioral finance researchers have found that investors behave in a predictably irrational manner. The reason? Humans are flawed decision makers. Sure, at our best we’re capable of amazing things like logic, humor, deduction, abstract reasoning, and imagination. But our brains were adapted

for life in the wild, where split-second decision making meant the difference between life and death. We developed mental shortcuts—called heuristics—that enable us to identify a snake and jump away before we are conscious of the snake’s presence.³³ When we realize moments later that the “snake” was in fact a stick, we are a victim of the heuristic that avoids snakelike objects. These heuristics—useful as they are for survival—give us a number of *cognitive biases* that impede us in our efforts to make rational or optimal decisions.

Cognitive biases impact every aspect of our lives, but, from an investor’s perspective, there are several that are particularly pernicious. The first is *overconfidence*, which leads us to put more weight in our own judgment than is objectively warranted. For example, if we are given a test and after taking it asked to determine the number of questions that we got right, we tend to overrate how well we performed. This is not a matter of simply incorrectly guessing our performance on the test because the errors all tend to be in one direction—we reliably overestimate how well we perform. Further, the more difficult the questions, and the less familiar we are with the content, the *more* we tend overestimate how well we performed. The two pioneers of the field of behavioral finance, Daniel Kahneman and Amos Tversky, suggest that our overconfidence may stem from two other biases, *self-attribution bias* and *hindsight bias*.³⁴ Self-attribution bias refers to our propensity to ascribe our successes to our skill, while blaming our failures on bad luck, rather than a lack of skill. For example, the stocks we buy that go up show our great stock picking skills, while those we buy that go down do so because of some outside factor, like Congress changing the law or the Federal Reserve increasing interest rates. If we do it often enough, we are led to the conclusion that we are skillful, which is as pleasant as it is wrong. Hindsight bias is the propensity to believe, after an event has occurred, that we predicted it before it happened. If, after watching some unlikely event unfold, you’ve ever said, “I knew that would happen,” when your reason for saying so was just some gut-feeling, you were subject to hindsight bias. The problem with hindsight bias is that if we think we predicted the past better than we actually did, we tend to believe that we can predict the future better than we actually can.

A related bias is *neglect of the base case*. The bias manifests when we try to answer probabilistic questions like, “What is the probability that object A originates from class B?” or “What is the probability that process A will generate outcome B?” The neglect-of-the-base-case bias is caused by a heuristic called *representativeness*. It is called the representativeness heuristic because we answer the questions by determining how much A represents—or resembles—B, rather than determining the likelihood of A given B.

Kahneman and Tversky give the classic example in their 1974 paper “Judgment under Uncertainty: Heuristics and Biases”³⁵:

Steve is very shy and withdrawn, invariably helpful, with little interest in people, or in the world of reality. A meek and tidy soul, he has need for order and structure and a passion for detail. How do people assess the probability that Steve is engaged in a particular occupation from a list of possibilities (for example, farmer, salesman, airline pilot, librarian, or physician)?

Kahneman and Tversky find that we guess that Steve is a librarian by assessing the degree to which the description of Steve is similar to the stereotype of a librarian. We should instead focus on the base rate. In Steve’s case, the fact that there are many more farmers than librarians in the population should lead us to guess that Steve is a farmer. We evaluate probability by representativeness and we ignore base rates.

There are many other cognitive biases. For example, the *availability* bias leads us to weigh more heavily information that can be easily brought to mind. We are influenced by vivid stories in the media about shark attacks and plane crashes when determining the likelihood of such an event’s occurring to us, and so we overestimate the likelihood of a shark attack or a plane crash, when driving in a car is a more dangerous pastime. An example of the bias in the context of stock markets is the drop in airline stocks beyond any reasonable estimate of the ongoing risk following high-profile plane crashes. *Anchoring* and *adjustment* biases describe our tendency to rely too heavily, or “anchor,” on one piece of information when making decisions. For example, if we buy a stock at a given price and it falls, we tend to anchor on the purchase price when determining the right price at which to sell. We want to “break even,” and hold on to the stock hoping to do so, ignoring new information. Our starting point influences us too much, so we don’t adjust sufficiently to account for new information, and as a result, our actions are biased toward the starting point.

We so regularly distort what we see, interpret illogically, and make poor judgments that our errors in reasoning become predictable. We are, as Dan Ariely puts it, “predictably irrational.”³⁶ Systematic behavioral biases create opportunities for investors who can find a way to control their innate weaknesses. For example, many researchers have found that most investors avoid “value” stocks—stocks that trade at a discount to book value, and instead buy “glamour” or “growth” stocks—stocks that trade at a premium to their book value. Why? We like the vivid story of the glamour stock, hear stories about our friends getting rich after buying them, and ignore the base rate returns for stocks that trade at high P/B value multiples. We happily

buy high-tech companies, and we lose money. We don't like boring stocks in label manufacturing businesses; our friends would laugh if we told them we owned them. Instead, we ignore the base rate returns for stocks with low P/B value multiples, even though those stocks tend to go up.

The reliance on heuristics and prevalence of biases is not restricted to laymen. Experts are also subject to the same biases when reasoning intuitively. In his book, *Expert Political Judgment*,³⁶ Philip Tetlock discusses his extensive study of people who make prediction their business—the experts. Tetlock's conclusion is that experts suffer from the same behavioral biases as the laymen. Tetlock's study fits within a much larger body of research that has consistently found that experts are as unreliable as the rest of us. A large number of studies have examined the records of experts against simple statistical model, and, in almost all cases, concluded that experts either underperform the models or can do no better. It's a compelling argument against human intuition and for the statistical approach, whether it's practiced by experts or nonexperts.

Even Experts Make Behavioral Errors

In many disciplines, simple quantitative models outperform the intuition of the best experts. The simple quantitative models continue to outperform the judgments of the best experts, *even when those experts are given the benefit of the outputs from the simple quantitative model*. James Montier, an expert in behavioral investing, discusses this phenomenon in his book, *Behavioral Investing: A Practitioners Guide to Applying Behavioral Finance*.³⁸ The first example he cites, which he describes as a classic in the field, and which succinctly demonstrates the two important elements of his thesis, is the diagnosis of patients as either neurotic or psychotic. The distinction is as follows: a psychotic patient “has lost touch with the external world,” while a neurotic patient “is in touch with the external world but suffering from internal emotional distress, which may be immobilizing.” According to Montier, the standard test to distinguish between neurosis and psychosis is the Minnesota Multiphasic Personality Inventory (MMPI).

In 1968, Lewis Goldberg, now a professor of psychology at the University of Oregon, analyzed more than 1,000 patients' MMPI test responses and final diagnoses as neurotic or psychotic. He used the data to develop a simple model to predict the final diagnosis based on the MMPI test response. Goldberg found that his model applied out-of-sample accurately predicted the final diagnosis approximately 70 percent of the time. He then gave MMPI scores to experienced and inexperienced clinical psychologists and asked them to diagnose the patient. Goldberg found that his simple model outperformed even the most experienced psychologists. He ran the

study again, this time providing the clinical psychologists with the simple model's prediction. Goldberg was shocked. Even when the psychologists were provided with the results of the model, they continued to underperform the simple model. While the performance of the psychologists improved from their first attempt without the benefit of the model, they still didn't perform as well the model did by itself. Montier draws an interesting conclusion from the results of the study: "[As] much as we all like to think we can add something to the quant model output, the truth is that very often quant models represent a ceiling in performance (from which we detract) rather than a floor (to which we can add)."³⁹

In his 2007 book *Super Crunchers*,⁴⁰ Ian Ayres discusses a myriad of other fields in which simple models prevail over experts, often in areas that would not appear to be friendly to a quantitative analysis. One such example is a statistical algorithm for predicting the outcome of Supreme Court decisions. The outcome of a Supreme Court hearing does not appear to be a subject matter that would be easy to reduce to a quantitative model because the language of law is language, and it's rarely plain. Ayres discusses a study by Andrew Martin and Kevin Quinn, "Competing Approaches to Predicting Supreme Court Decision Making," in which they found that just a few variables concerning the politics of a case predict how the U.S. Supreme Court justices will vote. Martin and Quinn analyzed data from 628 cases decided by the Supreme Court justices sitting at the time. Martin and Quinn considered six factors, including such unrelated matters as the circuit court of origin and the political ideology of the lower court's ruling, from which they developed simple models that predicted the votes of the individual justices. For example, the model predicted that if the lower court decision were "liberal," Justice Sandra Day O'Connor would vote to reverse it. If, however, the decision were "conservative" and came from the 2nd, 3rd, or Washington, D.C., circuit courts or the Federal circuit, she would vote to affirm.

Ayres writes that Ted Ruger, a law professor at the University of Pennsylvania, approached Martin and Quinn at a seminar and suggested that they test the accuracy of the simple model against a group of legal experts. The men decided to run a horse race. On one horse was Martin and Quinn's simple model, and on the other, 83 legal experts, law professors and legal practitioners, who would each assist in their own particular areas of expertise. The race was run over the Supreme Court's 2002 term. Who would most accurately predict the votes of the individual justices for every case that was argued? As you might expect by now, Martin and Quinn's simple model won, beating out the legal experts. The model predicted 75 percent of the court's decisions correctly, while the legal experts collectively could manage only 59 percent accuracy. Ayres writes that the model was most useful when predicting the crucial swing votes of Justices O'Connor

and Kennedy. The model predicted O'Connor's vote correctly 70 percent of the time, while the experts' success rate was only 61 percent.⁴¹ How can it be that simple models perform better than experienced clinical psychologists or renowned legal experts with access to detailed information about the cases? Are these results just flukes? No. In fact, the MMPI and Supreme Court decision examples are not even rare. There are an overwhelming number of studies and meta-analyses—studies of studies—that corroborate this phenomenon. In his book, Montier provides a diverse range of studies comparing statistical models and experts, ranging from the detection of brain damage, the interview process to admit students to university, the likelihood of a criminal to reoffend, the selection of “good” and “bad” vintages of Bordeaux wine, and the buying decisions of purchasing managers.

Value Investors Have Cognitive Biases, Too

Graham recognized early on that successful investing required emotional discipline. He wrote in the introduction to *The Intelligent Investor*⁴²:

Our main objective will be to guide the reader against the areas of possible substantial error and to develop policies with which he will be comfortable. We shall say quite a bit about the psychology of investors. For indeed, the investor's chief problem—and even his worst enemy—is likely to be himself. (“The fault, dear investor, is not in our stars—and not in our stocks—but in ourselves. ...”) This has proved the more true over recent decades as it has become more necessary for conservative investors to acquire common stocks and thus to expose themselves, willy-nilly, to the excitement and the temptations of the stock market. By arguments, examples, and exhortation, we hope to aid our readers to establish the proper mental and emotional attitudes toward their investment decisions. We have seen much more money made and kept by “ordinary people” who were temperamentally well suited for the investment process than by those who lacked this quality, even though they had an extensive knowledge of finance, accounting, and stockmarket lore.

As we have seen in other disciplines, the problem is that simply exhorting investors to “establish the proper mental and emotional attitudes toward their investment decisions” is not enough. Graham seems to nod to this when he says that “‘ordinary people’... temperamentally well suited for the investment process” will make more money than those who have “extensive knowledge of finance, accounting, and stockmarket lore.” The problem is behavioral rather than rational. We can understand the issue on

an intellectual level, and still fall victim to it because our emotions let us down. Seth Klarman acknowledged as much when he said⁴³:

So if the entire country became securities analysts, memorized Benjamin Graham's Intelligent Investor and regularly attended Warren Buffett's annual shareholder meetings, most people would, nevertheless, find themselves irresistibly drawn to hot initial public offerings, momentum strategies and investment fads. People would still find it tempting to day-trade and perform technical analysis of stock charts. A country of security analysts would still overreact. In short, even the best-trained investors would make the same mistakes that investors have been making forever, and for the same immutable reason—that they cannot help it.

If mere awareness that our judgments are biased does little to correct the errors we make, how then can we protect against these errors?

Nassim Taleb, author of *Foiled by Randomness*⁴⁴ and who calls himself a “literary essayist and mathematical trader,” argues that we should not even attempt to correct our behavioral flaws, but should instead seek to “go around” our emotions:

We are faulty and there is no need to bother trying to correct our flaws. We are so defective and so mismatched to our environment that we can just work around these flaws. I am convinced of that after spending almost all my adult and professional years in a fierce fight between my brain (not Fooled by Randomness) and my emotions (completely Fooled by Randomness) in which the only success I've had is in going around my emotions rather than rationalizing them. Perhaps ridding ourselves of our humanity is not in the works; we need wily tricks, not some grandiose moralizing help. As an empiricist (actually a skeptical empiricist) I despise the moralizers beyond anything on this planet: I wonder why they blindly believe in ineffectual methods. Delivering advice assumes that our cognitive apparatus rather than our emotional machinery exerts some meaningful control over our actions. We will see how modern behavioral science shows this to be completely untrue.

Research seems to support Taleb's method—tricking ourselves into doing the right thing—works better than simply trying to do the right thing (or flagellating ourselves if we don't).⁴⁵ Montier says, “Even once we are aware of our biases, we must recognize that knowledge does not equal behavior. The solution lies in designing and adopting an investment process that is at

least partially robust to behavioral decision-making errors.”⁴⁶ The advantage of the quantitative method is that it starts with the idea that most of us are temperamentally unsuited to investment, and then seeks to protect against those potential errors. If we acknowledge this flaw from the outset, we can build a process to force or trick us into exhibiting the correct behaviors. Given the diversity of fields in which quantitative models outperform experts, it would be remarkable if we did not observe the phenomenon in value investment. Yet within the world of value investing the quantitative approach continues to be uncommon. Where it does exist, says Montier, the practitioners tend to be “rocket scientist uber-geeks.” Why isn’t quantitative value investing more common? According to Montier, the most likely answer is that old cognitive bias overconfidence. We think we know better than simple models, which have a known error rate, but prefer our own judgment, which has an unknown error rate:

The most common response to these findings is to argue that surely a fund manager should be able to use quant as an input, with the flexibility to override the model when required. However, as mentioned above, the evidence suggests that quant models tend to act as a ceiling rather than a floor for our behaviour. Additionally there is plenty of evidence to suggest that we tend to overweight our own opinions and experiences against statistical evidence.

Our cognitive biases are most pronounced when we reason intuitively, so the more we rely on statistical evidence and limit our discretion, the fewer errors we should make. This is a powerful argument for a quantitative approach to value investment. As Buffett says, “Paradoxically, when ‘dumb’ money acknowledges its limitations, it ceases to be dumb.”⁴⁷

THE POWER OF QUANTITATIVE VALUE INVESTING

Charlie Munger, vice chairman to Buffett’s chairman of Berkshire Hathaway, Inc., says that playing poker in the Army and as a young lawyer made him a better investor. “What you have to learn is to fold early when the odds are against you,” says Munger, “or if you have a big edge, back it heavily because you don’t get a big edge often.”⁴⁸ Good poker players know that exploiting their edge leads over time to a reliable return, which can be expressed as an hourly rate: “big blinds per hour” (the “big blind” is the minimum bet in a hand of poker. By calculating their edge in terms of big blinds, good poker players can calculate the likely hourly rate available to them in a game by multiplying their edge by the minimum bet). For poor

poker players, the hourly rate is negative. It is amazing that in a game where luck plays such a huge role, the relative skill of a player can be quantified into an hourly rate. This not to say that good poker players expect to win every hand, every hour, or even every time they sit down to play. They know that over short periods of time luck is more important than skill. As David Einhorn, founder of Greenlight Capital and outstanding value investor, says⁴⁹:

People ask me “Is poker luck?” and “Is investing luck?” The answer is, not at all. But sample sizes matter. On any given day a good investor or a good poker player can lose money. Any stock investment can turn out to be a loser no matter how large the edge appears. Same for a poker hand. One poker tournament isn’t very different from a coin-flipping contest and neither is six months of investment results. On that basis luck plays a role. But over time—over thousands of hands against a variety of players and over hundreds of investments in a variety of market environments—skill wins out.

The law of large numbers rears its head. As the number of hands played increases, skill wins out. Given a large enough sample size, a player’s skill determines the player’s return. Investing is no different. Investors who want to outperform the market need an edge, and a value investing philosophy provides that edge. The difficulty for many investors will be in exploiting it.

The power of quantitative investing is in its relentless exploitation of edges. The objective nature of the quantitative process acts both as a shield and a sword. As a shield, it serves to protect us from our own cognitive biases. We can also use it as a sword to exploit behavioral errors made by others. It can give us the confidence to sit down at the poker table and know we’re not the patsy.

This book seeks to take the best aspects from quantitative investment and value investment and to apply them to stock selection and portfolio construction. Such an approach has several important advantages over pure quantitative investment, or pure value investment. We call our approach *Quantitative Value Investing*. This book describes our philosophy and sets out to describe the state-of-the-art in quantitative value investment techniques.

We seek to marry Ed Thorp’s quantitative approach to Warren Buffett’s value investment philosophy. We focus on the key to both investment styles, which is a valuation of the target security based on imperfect information, and the consistent exploitation of the differential between the valuation and the pricing available in the market. Buffett seeks to determine the value of an equity security through careful fundamental analysis, relying on his vast

experience and superior intellect. Thorp also processed information to generate valuations, but focused on probability and statistical theory to dictate his decisions.

Our connection of the quantitative process with a value-investing philosophy is not without antecedents. The first is, of course, Graham, the man who stands astride the entire value-investing edifice. The second is Joel Greenblatt, Graham's heir in the application of systematic methods to value investment. Greenblatt has recently defined a quantitative value strategy he calls the *Magic Formula*. The Magic Formula follows the same broad principles as Graham's simple model, but diverges from Graham's strategy by exchanging for Graham's absolute price measures a ranking system that seeks those stocks with the best combination of price and quality more akin to Buffett's value investing philosophy. We examine the Magic Formula in detail in the next chapter.

We believe Greenblatt's Magic Formula is an elegant step in the right direction, but we want to take the study of quantitative value to its logical conclusion. For the remainder of this book, we apply the quantitative process to our strict value investment philosophy. We exhaustively examine the state-of-the-art in quantitative value investment techniques. Then we test the research to find the best metrics for uncovering value: the cheapest price, the highest quality, and those stocks signaling that they are likely to quickly close the gap between price and value. Finally, we combine those metrics into a single method for finding high-performance value investment opportunities.

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