3

Theory and Practice

“We were like kids in a candy store,” Eugene Fama told Peter Bernstein (1992, p. 107). The young finance academics in the United States in the 1960s had ideas: the theories outlined in chapter 2. They had tools: at major research universities such as Fama’s (Chicago), access to powerful digital computers was becoming more readily available. And they had data—soon, lots of data.

In 1959, a vice-president of the stockbroker Merrill Lynch, Pierce, Fenner & Smith phoned James H. Lorie, a professor in the University of Chicago’s Graduate School of Business, to ask “whether anyone knew how well people were doing in the stock market relative to other investments” (Kun 1995, p. 1). It was a question that was still not easy to answer at the end of the 1950s. As any entrepreneurial academic would, Lorie parlayed it into a research grant.

Merrill Lynch’s $50,000 grant was the initial foundation of Chicago’s Center for Research in Security Prices (CRSP). After four years and a further $200,000, Lorie, Lawrence Fisher (CRSP’s associate director), and their staff had created the first CRSP Master File. It contained the monthly closing prices of all the common stocks in the New York Stock Exchange for the 35 years from January 1926, together with associated information such as dividends (Lorie 1965).

CRSP’s tapes—soon joined by other data sources, notably the “Compustat” tapes of accounting data sold by Standard & Poor’s—gave U.S. finance academics from the mid 1960s an advantage over their predecessors: easy access to massive volumes of data in a format that facilitated analysis. Even at the start of the 1960s, researchers such as the Chicago Ph.D. student Arnold B. Moore were still having to construct stock-price series by hand from runs of the *Wall Street Journal* (Moore 1962, p. 47). Once the CRSP tapes became available, that tedious effort was no longer needed.

A New Specialty

Another difference between the situation of the younger generation of finance academics in the United States in the 1960s and that of their predecessors such as Bachelier can be summarized in one word: institutionalization. "The new finance men," as their most persistent critic, David Durand, called them, were not simply individual scholars working predominantly on their own.[1] Academics of the stature of Merton Miller, Paul Samuelson, and Eugene Fama attracted Ph.D. students. Three of Miller and Fama's students—Michael Jensen, Myron Scholes, and Richard Roll—played significant parts in the developments discussed in this book, as did Samuelson's student Robert C. Merton.

Soon the students of the first generation of "new" finance scholars had their own Ph.D. students. A distinct academic field, not just a school of research, was created. A typical indicator of the coming into being of a new specialty is the setting up of a journal dedicated specifically to it. The *Journal of Financial Economics* began publication in 1974.

Another indicator of the successful emergence of a new specialty is incorporation into teaching curricula and textbooks. As the developments described in chapter 2 were consolidated and extended, they entered the curricula of the leading business schools in the United States. In the mid 1960s, for example, Eugene Fama and Merton Miller began to collaborate in the teaching of finance at the University of Chicago's Graduate School of Business, a collaboration that led to their 1972 textbook *The Theory of Finance*. "To make the essential theoretical framework of the subject stand out sharply," wrote Fama and Miller, "we have pruned away virtually all institutional and descriptive material." (1972, p. vii)

The intellectual world of Arthur Stone Dewing had been swept away. Standard aspects of "managerial finance" such as "cash flow forecasting, cash budgeting . . . [and] credit management" were set aside by Fama and Miller because work on them was "ad hoc" and "largely unrelated" to the emerging theory of finance (Fama and Miller 1972, p. vii). Replacing those topics was a broadly unified theoretical structure encompassing the topics of chapter 2 of this book: the Modigliani-Miller propositions, the work of Markowitz and Sharpe, and the efficient-market hypothesis. In the next two decades, *The Theory of Finance* was joined by many other textbooks building on the same core material. By the end of the 1990s, if one walked into almost any large university bookshop in Western Europe or in the United States, one could find shelves of textbooks whose contents had their roots in the finance scholarship described in chapter 2 and in option theory.

From the mid 1970s on, significant clusters of analytical, economics-based research in finance were to be found in most major American research universities: contributions from outside the United States were, in general, slower to appear. In the 1960s and the early 1970s, however, two schools dominated: the University of Chicago (where work in finance was led by Lorie, Miller, and Fama), [2] and the Massachusetts Institute of Technology (where Samuelson was joined in 1960 by Modigliani, first for a visitorship and then permanently).

The Chicago and MIT groups differed in approach: as was discussed in chapter 1, Miller and his Chicago colleagues shared Friedman's view that the realisticness of assumptions was irrelevant, while Samuelson regarded that attitude as cavalier. In the background also lay political differences. Chicago was overwhelmingly "free market" in its politics. That was most famously so in the writings of Milton Friedman, but Miller also developed a highly visible, activist commitment to a free-market approach. Samuelson and Modigliani were more skeptical of the virtues of unfettered markets and more favorable to government action.

The political and methodological differences between the leaders of the Chicago and MIT groups were less marked among the more junior faculty and did not prevent practical collaboration. The finance groups at the two universities exchanged ideas, people, and mutual assistance. For example, MIT was more prominent in option theory: Scholes and Merton were employed there, and although Black did not have an academic job until 1971, he was a regular participant in MIT's Tuesday evening finance workshops (Merton and Scholes 1995, p. 1359). However, Scholes had come to MIT from Chicago in 1968, and Black's first university post was at Chicago (in 1975, he returned to Boston to take up a professorship in MIT's Sloan School of Management). After the initial rejection of the 1973 paper in which Black and Scholes laid out their analysis, Fama and Miller intervened to secure its publication in the prestigious *Journal of Political Economy*, which was edited in Chicago.[3]

Even at Chicago and MIT, not everyone welcomed the new financial economics wholeheartedly. Friedman's reservations about Markowitz's thesis were described in chapter 2. David Durand of MIT extended his criticism of Modigliani, Miller, and Markowitz into an overall attack on the "new finance men" for having "lost virtually all contact with terra firma." "On the whole," he wrote, "they seem to be more interested in demonstrating their mathematical prowess than in solving genuine problems; often they seem to be playing mathematical games." (Durand 1968, p. 848)

Nor were the financial markets a safe choice of substantive topic for an ambitious young academic economist, at least up to the mid 1970s. Prais's objection to Kendall—that his conclusions might be right, but that in focusing on stocks and similarly traded commodities he was looking at markets that were not of great economic interest—seems to have been widely shared. Hayne Leland recalls that when he started to work on finance in the mid 1960s many economists did not regard such work highly, and he did not achieve tenure in his initial post at Stanford University.[4]

Economics departments were usually in the prestigious faculties of arts and sciences, while finance was often seen as an appropriate topic for universities' vocationally oriented business schools, which were gaining academic status only slowly. The older descriptive, institutional, "unscientific" approach taken in those business schools to the study of finance may have left its mark on economists' attitudes. The economist Stephen Ross, who began research in finance while an assistant professor at the University of Pennsylvania at the start of the 1970s, recalls being warned that "finance is to economics as osteopathy is to medicine" (Ross interview).[5]

Nevertheless, in the 1960s and the 1970s the new financial economics gradually became a recognized, reasonably high-status, enduring part of the academic landscape, one that could, and did, successfully reproduce itself and grow. Economists might continue to harbor doubts about the value of the research being done, but business schools, not economics departments, were the main institutional base of the new specialty. Within those schools, the mathematical modeling and computerized statistical testing that the "new finance men" employed were unquestionably state-of-the-art.

The business schools of the United States were changing fast in the 1960s, as Richard Whitley (1986a,b) emphasizes. The attempt in the 1950s by Herbert Simon and his colleagues at Carnegie Tech to shift education for business from a vocational to a "science-based" approach was a harbinger of a wider transformation. In 1959, an influential report for the Ford Foundation titled *Higher Education for Business* noted the pedestrian, descriptive courses, the academic mediocrity, and the absence of a culture of research at many American business schools. Business, the authors of the report commented, "finds itself at the foot of the academic table, uncomfortably nudging those two other stepchildren, Education and Agriculture" (Gordon and Howell 1959, p. 4).

In response to the perception that they were not rigorous enough, American business schools sought to "academicize" themselves, a goal whose achievement was assisted by the availability of funds for the task from the Ford Foundation and by the general expansion in higher education. Academicization proceeded rapidly in the 1960s and the 1970s. In 1977, Paul Cootner, who had published the canonical collection of papers on the random-walk thesis referred to in chapter 2, noted in a talk to the Western Finance Association that "virtually all aspects of modern business studies have shared a rise in academic prestige" (Cootner 1977, p. 553).

In a context of self-conscious "academicization," the new financial economists—who worked on a traditional business school topic, but in a sophisticated, mathematical, academic, discipline-based way—were attractive targets for recruitment. In consequence, the new generation of finance academics could find jobs outside the Chicago-MIT axis. Miller and Fama's student Michael Jensen, for example, was hired by the College of Business at the University of Rochester.

Not achieving tenure at Stanford was only a temporary setback for Hayne Leland, who moved across the Bay to the Graduate School of Business Administration at the University of California at Berkeley. Sharpe received an offer from the business school at the University of Washington in Seattle—which was, as he describes it, in "transition from a traditional, nonrigorous, institutionally oriented program to [a] rigorous discipline-based academic school"—that was good enough for him to view it as superior to an approach from Chicago.[6]

Promising career opportunities in serious, academically oriented business schools were opening up for the "kids in a candy store." They had done more than bring new, more mathematical methods to bear on the study of finance. They had also brought about a subtle shift in attention, one well captured by Cootner's 1977 talk. Finance had "outpace[d] most of its sister fields" of business studies, said Cootner. "It would be nice," he continued,

> if we could regard this as a mere reflection of the innate brilliance and superior intellect that naturally attaches to members of our profession, but I think that any objective historian would find little support for any such proposition. While we have found ourselves on the road to *academic* prosperity, I suspect that innate ability has played about as much a role as it has played in the recent *monetary* prosperity we see among coffee farmers and Arabian princes. No aspersion is cast on my brilliant colleagues when I argue that if they had invested the same effort on marketing theory or organizational behavior, they would have produced less striking results.[7]

Success had come, said Cootner, because finance academics had been able to look not directly at the firm but at the market. "The areas within finance that have progressed more slowly are either those internal to the firm and most immune to market constraint, or those in which financial institutions' very *raison d'etre* [*sic*] arises from the imperfection of markets." (Cootner 1977, pp. 554)

The focus as well as the methodology of finance scholarship had shifted: from the corporation, as in Dewing's classic focus on the *Financial Policy of Corporations* (Dewing 1953), to the rational investor, the market, and the way in which "an individual's optimal behavior is strongly constrained by the competitive efforts of others" (Cootner 1977, p. 553). Take the Modigliani-Miller propositions, discussed in chapter 2. They concerned matters about which firms made decisions (capital structure and dividend policy); however, instead of looking inside the firm for the determinants of these decisions, Modigliani and Miller looked at the firm from the outside: from the viewpoint of investors and the financial markets.

The shift of attention from corporation to market was in part a matter of finding a focus that was tractable mathematically. It was also a shift encouraged by the application to finance of orthodox microeconomic ways of thinking: recall Sharpe's testimony, quoted in chapter 2, that he "asked the question microeconomists are trained to ask. If everyone [that is, all investors] were to behave optimally . . . what prices will securities command once the capital market has reached *equilibrium*." The shift was most likely also helped—for example, in the attractiveness to students of the research topics involved—by the gradual recovery in prestige of the stock market in the United States from a nadir that lasted from 1929 to the 1940s.

The Wall Street crash, the Great Depression, and World War II had left U.S. financial markets focused largely on bonds, especially government war debt.[8] However, as investors became more confident that the postwar prosperity would continue, stocks began to seem attractive. Stock prices rose markedly in the 1950s, and in February 1966 the Dow Jones industrial average reached, albeit briefly, the unprecedented level of 1,000 (Brooks 1973, pp. 102–103). The "blue chip" corporations that traditionally made up the Dow had to jostle for attention with alluring high-technology corporations such as Xerox, Polaroid, Litton Industries, Ling-Temco-Vought, and—perhaps most strikingly of all—H. Ross Perot's Electronic Data Systems. Investment, Wall Street, the financial markets—in the 1960s, all these were once again interesting, even exciting.

"Saddam Hussein Addressing the B'nai B'rith"

In the period in which modern financial economics emerged, the financial markets in the United States were changing in their structure as well as reviving economically. The new generation of investors often chose to entrust their money to the managers of the fast-growing, seductively advertised mutual funds, rather than themselves directly buying stocks and other securities. With other investment intermediaries such as bank trust departments, pension funds, and insurance companies also expanding, the proportion of stocks held by "institutional" investors grew, for example increasing between 1969 and 1978 from 34.2 percent of holdings of stocks traded on the New York Stock Exchange to 43.4 percent (Whitley 1986a, p. 165).

With the finance sector of the U.S. economy growing, and with more complex tasks also being performed in the treasurers' departments of non-financial corporations, there was a rapid increase in the demand for graduates or holders of Masters of Business Administration (MBA) degrees with training in finance. This in its turn meant more jobs for those with Ph.D.s in finance teaching those students. Whitley (1986a,b) rightly emphasizes that this was an important component of the institutionalization of financial economics in American business schools.

The quantitative, analytical skills that financial economists taught their students were certainly likely to be of practical benefit in their future employment. However, the relatively smooth institutionalization of finance theory within academia contrasts with an often quite different reaction to it outside universities. As financial-market practitioners gradually became aware of the views and the techniques that the theorists were developing, their response was frequently one of hostility, on occasion extreme hostility.

The overall causes of practitioner ire were, above all, the random-walk hypothesis and the efficient-market hypothesis. They were a direct challenge to the two dominant schools of thought among investment professionals: "chartism" (or "technical analysis") and "fundamentals analysis." Chartism was in a sense a by-product of the rapid development in the late nineteenth century of the technology for recording and disseminating securities prices, notably the stock "ticker," in which prices were recorded on exchanges' floors, transmitted telegraphically, and printed out on paper tape in brokers' offices. With prices available in close to real time, it became possible to construct charts of hour-by-hour fluctuations. In some offices, clerks stood beside the ticker, directly recording changing prices on graphs (Preda, forthcoming).

As the name suggests, "chartists" specialized in the analysis of graphs of price changes, discerning patterns in them that they believed had predictive value. The stock ticker and the chart "disentangled" financial markets from the messy contingency of stock exchange floors, making them abstract and visible (Preda, forthcoming). Chartists argued that a diligent student of price graphs, deploying the techniques of chartism, could detect trends not just in retrospect but as—or indeed before—they happened.

Richard Demille Wyckoff and Roger Ward Babson began to popularize the techniques of chartist forecasting in the first decade of the twentieth century (Preda 2004b). They often cited as the inspiration of chartism Charles H. Dow (1851–1902), co-founder with Edward D. Jones of the Dow-Jones average and the *Wall Street Journal* (although Preda suggests that Dow contributed only general ideas, not specific techniques).[9]

Chartism never achieved institutionalization in academia, but it became a lasting component of how many financial practitioners think about markets. It offered a vernacular theory of markets (one rooted not in economics but in speculations about investor psychology and perhaps even in the sociology of “herd behavior”) and a way of making sense of markets that was, and is, attractive (Preda 2004b). Much mass-media presentation of markets—with its citation of “trends,” “reversals,” “corrections,” “resistance levels,” and so, and with its fascination with salient round-number index levels such as a Dow Jones level of 10,000—is in a sense a diluted form of chartism. Even in Chicago’s derivatives markets, heavily influenced as they are by finance theory, I encountered chartists.[10]

Random-walk theory and efficient-market theory challenged the chartist worldview by suggesting that the patterns that the chartists believed they saw in their graphs were being read by them into what were actually random movements. For example, the University of Chicago Business School statistician Harry V. Roberts used a table of random numbers to simulate a year in a market, and found that the resultant graph (figure 3.1) contained the most famous of all chartist patterns, the “head-and-shoulders”: a “peak” with two lower peaks on either side of it, regarded by many chartists as the unequivocal signal of the start of a prolonged decline. “Probably all the classical patterns of technical analysis [chartism],” Roberts asserted, “can be generated artificially by a suitable roulette wheel or random-number table.” (1959, p. 4)

Efficient-market theory thus saw chartism as delusional pseudoscience. It was slightly more charitable to the chartists’ traditional opponents, fundamentals analysts. The “fundamentals” they studied were not stock-price fluctuations—overattention to which they despised—but the health of and prospects for a corporation’s business, the relationship of that business to underlying conditions in the economy, and the details of a corporation’s balance sheet, income statements, and cash flow. Fundamentals analysts prided themselves on being able to discover companies that the market was undervaluing. Fundamentalism’s most influential proponent was Benjamin Graham (1894–1976), stock analyst, investor, author, visiting professor at Columbia University, and teacher and employer of a young man from Omaha who was to become America’s most famous investor: Warren Buffett.

The successive editions of *Security Analysis*, written by Graham and his Columbia colleague David L. Dodd, taught that stocks and other securities had an “intrinsic value,” a value “which is justified by the facts, e.g., the assets, earnings, dividends, definitive prospects, as distinct, let us say, from market

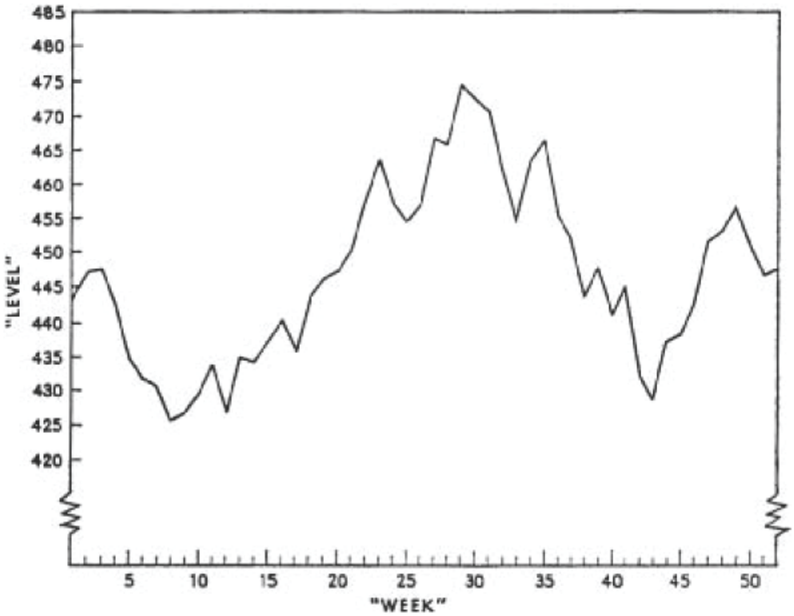

Figure 3.1
Harry Roberts's randomly generated stock market price levels. Source: Roberts 1959, p. 5. Courtesy of Blackwell Publishing Ltd.

quotations established by artificial manipulation or distorted by psychological excesses" (Graham and Dodd 1940, pp. 20–21). Graham and Dodd admitted that there was no simple, rote way of determining what that intrinsic value was, but they believed that careful analysis could reveal cases in which a market price differed from any plausible estimate of intrinsic value.

Efficient-market theorists agreed with fundamentals analysts that securities had an intrinsic value that was rooted in the reality of the economic situations of the corporations that issued them. However, efficient-market theory posited that market prices were the best available estimators of that intrinsic value: that was part of the meaning of the statement that "prices always 'fully reflect' available information" (Fama 1970, p. 383). Corporate fundamentals mattered, but, precisely because there were many skilled and intelligent analysts scrutinizing their implications for stock prices, such implications would already have been incorporated into prices. In consequence, if markets were efficient, fundamentals analysts were wasting their time looking for cases in which intrinsic value differed knowably from market price.

Was the practical investment success of some fundamentals analysts, notably of the enormously successful Warren Buffett, evidence against efficient-market theory? Buffett's investment record certainly commanded respect: Buffett told his biographer, Roger Lowenstein, that Paul Samuelson had hedged his intellectual bets by making a large investment in Buffett's holding company, Berkshire Hathaway (Lowenstein 1995, p. 311). In 1984, Columbia University celebrated the fiftieth anniversary of the original publication of Graham and Dodd's *Security Analysis* by staging a debate between Buffet and the efficient-market theorist Michael Jensen. Jensen argued that such success as had been enjoyed by followers of Graham and Dodd, such as Buffet, might be sheer chance: "If I survey a field of untalented analysts all of whom are doing nothing but flipping coins, I expect to see some who have tossed two heads in a row and even some who have tossed ten heads in a row." (quoted on p. 317 of Lowenstein 1995)

Replying to Jensen, Buffet did not dismiss the latter's unflattering analogy out of hand. If all the inhabitants of the United States were to begin a coin-tossing game in which those who called wrongly, even once, were thrown out of the game, after twenty rounds there would still remain some 215 players who had called successfully twenty times in a row. It would be easy to imagine that this successful few had superior predictive skills.

As Buffett put it, however, "some business school professor will probably be rude enough to bring up the fact that if 225 million orangutans had engaged in a similar exercise, the results would be much the same" (quoted by Lowenstein 1995, p. 317). Ultimately, though, Buffett rejected the hypothesis that his success and that of similar fundamentals analysts was attributable to chance: in his view, too many of the successful orangutans "came from the 'same zoo,'" fundamentals analysis in the style of Graham and Dodd, for their success to be explicable as mere random good fortune (ibid., pp. 317–318).

Efficient-market theorists were, in general, prepared to concede that it was possible that some analysts might have systematically superior skills in identifying investment opportunities. They denied, however, that such skills were widespread. Their most damning piece of evidence in this respect was an analysis by Michael Jensen of the performance of mutual funds from 1945 to 1964. These funds were an increasingly popular way of investing in the stock market. Investors bought units or shares in the fund, and its managers invested the capital thus raised. The funds charged substantial fees for making it possible for investors indirectly to own a well-diversified stock portfolio and for the apparent privilege of professional management of that portfolio.

Diversification was indeed valuable, Jensen concluded, but there was no evidence that fund managers had systematic predictive skills. Even after the funds' large sales commissions were removed from the analysis, investing in a mutual fund was typically less rewarding than simply buying and holding a "market portfolio" in the form of all the stocks in the Standard and Poor's 500 index: "The evidence on mutual fund performance . . . indicates not only that these . . . mutual funds were *on average* not able to predict security prices well enough to outperform a buy-the-market-and-hold policy, but also that there is very little evidence that any *individual* fund was able to do significantly better than that which we expected from mere random chance."[11]

There was a sense in which Jensen's result was theory-laden: the Capital Asset Pricing Model was used to eliminate the effects of differences in performance resulting from different levels of beta. (See chapter 2.) Even that, though, was scant comfort to the traditional "stock-picking" investment manager: without the correction, mutual funds would have underperformed by an even greater margin.[12] The underperformance, Jensen suggested, "may very well be due to the generation of too many expenses in unsuccessful forecasting attempts" (1968, p. 394).

Results such as Jensen's point to a dilemma central to practitioners' responses to new financial economics. There was a significant movement among investment advisers and securities analysts toward professionalization. The idea that such advisers and analysts should gain certification after being examined on their knowledge of finance had been proposed for some time (the fundamentals analyst Benjamin Graham was a particular advocate of it), and in 1963 the Institute of Chartered Financial Analysts took in its first chartered member, soon to be followed by many thousands more (Jacobson 1997, pp. 72–73).

The existence of an increasingly authoritative theory of finance might be thought helpful in gaining professional status. The trouble, though, was that the theory suggested that much of the apparent expertise of members of the putative profession was either spurious (as in the case of chartists) or of little or no direct practical benefit (as in the case of most fundamentals analysts).

What, for example, was the point of certifying that someone had mastered the skills necessary for security analysis in the style of Graham and Dodd if the results of such analysis did not, in general, improve investment decisions? James Lorie, director of the University of Chicago's Center for Research in Security Prices, told a reporter for the magazine *Institutional Investor* that money managers should "give up conventional security analysis. Its occasional triumphs are offset by its occasional disasters and on the average nothing valuable is produced" (Welles 1971, p. 58). David Goodstein, an investment manager who was a convert to the new ideas, put the same point more bluntly: "A lot of people are simply going to be put out of business. I mean, what are they really doing? What value are they adding to the process? What passes for security analysis today, in my opinion, is 150,000 percent bullshit." (ibid., p. 24)

To practitioners, finance theory—especially random-walk theory and efficient-market theory—appeared to be claiming that "the value of investment advice is zero," as one such advisor, Pierre Rinfret of Rinfret-Boston Associates, told a 1968 conference of 1,500 money managers. To Rinfret, the theory that led to this conclusion was fundamentally flawed: ". . . random-walk theory is irrelevant, it is impractical, it is logically inconsistent, it is conceptually weak, it is limited in scope, and it is technically deficient." Finance academics seemed to be saying, in an analogy drawn by Gilbert E. Kaplan, who chaired the conference, that one might just as well select stocks by throwing darts at the financial pages of a newspaper as take professional advice.[13]

Financial economists would not have accepted that the dartboard analogy was entirely fair—a portfolio selected by dart-throwing might, for example, not be diversified well enough to be optimal or near-optimal—but it was a simple, memorable image that endured and seems to have captured for many investment managers what they objected to in efficient-market theory. In 1988, the *Wall Street Journal* began regular contests in which the performance of a small number of stocks selected by investment managers was compared to a similar number of stocks selected by throwing darts. The managers indeed tended to outperform the darts, although the consistency with which they did so was less than fully reassuring. Managers' choices outperformed those of the darts in 83 of the 135 contests from 1990 to 2001, but the darts did better than the apparent experts 52 times (Jasen 2001).

If finance academics were believed to be saying that "the value of investment advice is zero," it is no wonder that, as the economist Burton Malkiel of Princeton University put it, proponents of efficient-market theory were "greeted in some Wall Street quarters with as much enthusiasm as Saddam Hussein addressing a meeting of the B'nai B'rith" (Malkiel 1996, p. 159). The investment adviser Peter Bernstein "found the new theories emerging from the universities during the 1950s and the 1960s alien and unappealing, as did most other practitioners. What the scholars were saying seemed . . . to demean my profession as I was practicing it." (Bernstein 1992, p. 13)

James Vertin of the Wells Fargo Bank, a leading member of the Financial Analysts Federation, wrote: "You just don't win friends . . . by appearing to tell sincere, dedicated, intelligent people that they are useless dolts who could and should be replaced by computers. . . . Rightly or wrong, most practitioners feel themselves to be objects of academic ridicule, and most feel bound to resist this assault." (Vertin 1974, p. 11)

Measuring Investment Performance

There were also more specific reasons for practitioners' hostility to techniques based on finance theory. Take the Capital Asset Pricing Model, for example. It had aspects that were broadly compatible with how practitioners thought about stocks. When the computers and data sets of the 1960s and the 1970s were used to calculate betas, it usually turned out that low-beta stocks were those that practitioners regarded as stable, "defensive" investments, whereas a high beta generally indicated a riskier "growth" stock. So far so good, from a traditional practitioner's viewpoint. The CAPM could, however, also be turned into a disciplinary device, subjecting investment managers' results to mathematical scrutiny.

Two of the CAPM's developers, Jack Treynor and William Sharpe, began to employ it to analyze investment performance (Treynor 1965; Sharpe 1966). Treynor found that some apparently stellar performances were the result simply of constructing risk-laden, high-beta portfolios. When stocks were doing well, as they were in the mid 1960s, such a portfolio would indeed enjoy returns superior to those of the overall market. If, however, there were a downturn, a high-beta portfolio would be expected to incur greater than average losses.

Treynor presented his results to a group of investment professionals and trustees of university endowment funds. "I was very pleased with myself" for the analysis, says Treynor, but "I looked around that room and all I saw was angry faces." Shortly afterward, on a quiet highway in the late evening, another driver attempted to force Treynor's automobile off the road (Treynor interview).

There is no evidence of a connection between the incident and Treynor's efforts at systematic performance measurement, but that the possibility of a connection struck him indicates how controversial the practical applications of finance theory were. When Nicholas Molodovsky, editor of the *Financial Analysts Journal*, died in 1969, Treynor succeeded him and, like Molodovsky, he sought to bring theorists' work to the attention of the journal's predominantly practitioner readership. The latter did not all warm to Treynor's efforts. "The increasingly angry reaction by security analyst readers to the often quite difficult and theoretical articles . . . led to a serious 'identity crisis' at the magazine," and in 1976 Treynor was nearly ousted as editor by the directors of the Financial Analysts Federation (Welles 1977, p. 40).

When Treynor stepped down as editor of the *Financial Analysts Journal*, in 1981, his last editorial was pointed. The journal, he wrote, has "enemies" because "it encourages investors to distinguish between good research and bad research." The former often led to the efficient-market conclusion that "securities are correctly priced, with nothing to be gained by buying or selling." However, no transactions meant no income for brokers or market makers, so "a few greedy exceptions to the generally high-minded people in the securities industry consider they have a vested interest in bad research"—research that claimed to identify what were in reality non-existent opportunities for profit (Treynor 1981).[14] The *Financial Analysts Journal*, said Treynor, could be seen as "one small, unimportant front in a much larger war."

The divide over finance theory was, however, never a simple war of academics versus practitioners. Rejection of finance theory by the latter was never universal. For example, Vertin and Bernstein, practitioners whose initial reactions were hostile, changed their views, the latter becoming first a supporter and then the historian of the new ideas. The U.S. financial markets are, if nothing else, places of entrepreneurship, and so it is not surprising that some practitioners began to see ways of making money out of finance theory.

The kind of performance analysis with which Treynor and Sharpe had experimented could, for example, be offered as a commercial product. What was probably the first such service was offered by John O'Brien. After graduating in economics from MIT in 1958, O'Brien did military service at an Air Force base just north of Santa Monica that was frequently used as a research site by the Rand Corporation. In 1962, O'Brien joined a Rand spinoff, the Planning Research Corporation, and then moved to a spinoff of the latter where his job was to "try to break into the finance industry." He searched the finance literature, came across Sharpe's work, sought personal tutoring from him, and designed a system based on the Capital Asset Pricing Model that would, for example, allow pension fund treasurers to discover whether the performance of the investment managers they employed was as good as it should be, given the betas of their portfolios (O'Brien interview).

The idea of systematic evaluation of investment performance by using the Capital Asset Pricing Model was slow to take off. At first O'Brien could find only one client: the investment committee of the Aerospace Corporation, made up as it was of quantitatively minded "rocket scientists" (O'Brien interview). Another firm that began to offer a similar service, Becker Securities, even suspected that some of its former investment manager clients were diverting business away from it in retaliation for its move into performance measurement (Welles 1977, p. 41).

As the 1960s ended, however, the stock boom of the decade's middle years began to evaporate. The Dow Jones industrial average fell 15 percent in 1969, and continued to slide in the early months of 1970. By May, it was 36 percent below the level of December 1968. The favored "growth" stocks of the 1960s—major components of the high-beta portfolios whose risk Treynor had diagnosed—suffered far worse, many falling by 80 percent or more. The initial public offering of Electronic Data Systems in September 1968 had been one of the decade's stock-market highlights, but in a single day (April 22, 1970) its stock lost one-third of its value.[15]

The reviving fortunes and renewed glamor of the stock market in the 1950s and the 1960s may have increased its attractiveness as an academic research topic. The sharp reversal of those fortunes at the start of the 1970s seems greatly to have increased practitioner interest in the practical results of this research. After what had happened, for institutional investors to know the beta values of their portfolios began to seem sensible. Well-established firms began to move into the field, realizing that betas could, literally, be sold.

The databases and the computer power required to calculate betas were, at the start of the 1970s, still far from universally available, so there was a commercial opportunity for those with the resources to produce "beta books" (lists of the beta values of different stocks) and to provide performance measurement services. Much of the early initiative came from smaller firms, notably Becker Securities and James A. Oliphant & Co.[16] However, Merrill Lynch, a major presence on Wall Street and the initial funder of the Center for Research in Security Prices, also launched a performance measurement service and a beta service (Welles 1971).

Indeed, beta came to enjoy quite a vogue in the 1970s. Not to know what it meant—or, at least, not to appear to know—started to mark one out as unsophisticated. Pension fund treasurers were increasingly spreading their money across several different investment management firms. In that kind of competitive situation, as a pension consultant told the magazine *Institutional Investor*, "when the treasurer asks you how you calculate beta, you better damn well have a nice smooth answer ready" (Welles 1971, p. 22).

It is difficult to determine just how much practical use was made of the beta books of the early 1970s and of the increasingly elaborate performance analysis systems that were marketed later in the 1970s. Leading providers of such systems included BARRA (set up by Barr Rosenberg, a Berkeley professor who combined quantitative skill with a flair that turned his colorful countercultural lifestyle into a surprisingly effective marketing resource[17]) and Wilshire Associates (a consulting firm, based in Santa Monica, that developed out of John O'Brien's consultancy, O'Brien Associates[18]).

"An awful lot of this material [from firms such as BARRA and Wilshire] is coming in, is sitting on people's desks, is getting talked about in meetings," the consultant Gary Bergstrom told *Institutional Investor* in 1978. "But the number of people who are actually using the new investment technology to develop investment strategies and manage money is still limited to a handful." (Welles

1978, p. 62) The 1970s, with their oil shocks and apparently out-of-control inflation, were difficult, tumultuous years in the financial markets. Harold Arbit of the American National Bank (Rosenberg's initial source of finance) complained: "A lot of people who are signing up with Barr [Rosenberg] are just glomming onto him as a security blanket without understanding him." (ibid., p. 66) "More and more managers," said Bergstrom, "are hiring quantitative guys to make pitches to their clients. Everybody is trying to look *au courant*." (ibid., p. 62) That did at least mean that there were jobs and consultancies for those versed in quantitative approaches to finance, and that finance theory's ideas were becoming known. Furthermore, there were at least some in the investment management industry whose engagement with finance theory was deeper than this.

Index Funds

Particularly important among those who drew actively on finance theory was the Wells Fargo Bank, based in San Francisco. The vice president in charge of the bank's Management Sciences Department was John A. McQuown. After getting a degree in engineering and then serving in the Navy, McQuown studied at the Harvard Business School from 1959 to 1961. Harvard had yet to engage fully with the emerging new approaches to finance. The teaching of the subject was, "in retrospect, pathetic. It was institutional. It was . . . the story was an institutional story. There wasn't any theory." (McQuown interview)

McQuown went on to work on Wall Street, honing his mathematical skills by taking postgraduate courses at New York University's Courant Institute, one of the world's leading centers of mathematical research. He learned of the way finance scholarship was developing from Chicago friends such as James Lorie and Eugene Fama. After he was hired by Wells Fargo in 1964, McQuown started to build links to financial economics.

McQuown brought in, as consultants to Wells Fargo, Fischer Black, Myron Scholes, William Sharpe and other financial economists, and sponsored an important series of conferences of the new field. In 1968—long before such hirings were popular—McQuown recruited as a Wells Fargo employee Oldrich Vasicek, who was fresh from a Ph.D. in probability theory from Prague's Charles University and a refugee from the Soviet invasion of Czechoslovakia.[19]

By far the most important innovation to come out of Wells Fargo was the index fund. Both Michael Jensen's work and the findings of performance measurement firms such as Becker (Ehrbar 1976) suggested that active, stock-picking investment managers did not outperform stock-market indices systematically. Indeed, once such managers' high costs were taken into account they typically did worse than those indices.

So why not turn Jensen's comparator—buying and holding every stock in an index—into an investment strategy? If conventional investment analysis had to be "throw[n] away" (McQuown interview) because markets were efficient, then why not simply invest in a portfolio that encompassed the market, for example by including every stock in the S&P 500 index in proportion to its market value?

This idea of an "index fund" met with considerable hostility from securities analysts who believed they could discern stocks' inherent worth and thus distinguish good from bad investments. "They though we were crazy," says Oldrich Vasicek. "They said 'You want to buy all the dogs. . . . You just want to buy whatever garbage happens to be traded?' " (Vasicek interview) A crucial spur, however, came from outside, indirectly from the University of Chicago. Keith Schwayder, son of the owner of the luggage manufacturer Samsonite, had just completed a degree at the university's Graduate School of Business.

In his courses at Chicago, Schwayder had "heard about all this beta stuff and went back to work for Dad" (Fouse interview). He discovered that the firm's pension fund "was invested in a mixed bag of mutual funds. To someone who had sat at the feet of Lorie, Fama, and Miller, this was heresy. He began by asking around to see if anyone, anywhere, was managing money in the 'theoretically proper' manner in which he had been schooled." (Bernstein 1992, p. 247) William Sharpe put him in touch with Wells Fargo (Sharpe interview), and in 1971 Samsonite's pension fund commissioned the bank to create an index fund in which to invest some of its capital.[20]

It helped that Wells Fargo, despite bearing a historically famous name, did not have a large base of clients for actively managed, stock-picking funds. The case for an index fund was that such active management was useless or worse. "It's hard to tell your clients that the world is flat [meaning that your managers can successfully pick good stocks] and then spring a completely different universe on them," points out William L. Fouse, then at Wells Fargo, who had previously tried and failed to persuade colleagues at the Mellon Bank in Pittsburgh to launch an index fund (Fouse interview).

Two other initial implementers of index funds in the United States were also relative outsiders. One was the Chicago-based American National Bank; the other was Batterymarch Financial Management, set up in Boston in 1969 by Dean LeBaron, a mutual fund manager who had become interested in finance theory (LeBaron interview).[21] At the American National Bank, the main proponent of index funds was Rex A. Sinquefield, who had become a proponent of efficient-market theory while studying for an MBA at the University of Chicago. "I remember the first class with Merton Miller," says Sinquefield, "and he talked about the notion of market efficiency . . . and I remember thinking, 'This has got to be true. This is order in the universe, and it's not plausible that it is not true.'" (Sinquefield interview)

Those who believed themselves to be skilled stock pickers, able to identify investment opportunities that the other participants in the market had not seen, often despised index funds. One such firm modified the classic Uncle Sam recruiting poster so that the caption read "Indexing is un-American." Soon a copy of the poster was "nailed behind [the] trading-room doors of practically every money manager in the country, replacing Marilyn Monroe" (Fouse interview).

Opposition from stock pickers did not, however, stem the flow toward indexation. During the 1970s, more and more pension funds began placing at least some of their investments under "passive" (that is, index fund) management, and soon index funds also began to be sold direct to the general public. Crucial recruits to indexing from the world of pensions were American Telephone and Telegraph, which had what was then the largest of all private pension funds, and the local operating company New York Telephone. By June 1976, index funds were "an idea whose time is coming," according to a prominent article in *Fortune* (Ehrbar 1976). Samsonite's initial $6 million in 1971 grew to around $9 billion in U.S. index funds by 1980, $47 billion in 1985, $135 billion in 1988, and $475 billion in 1996.[22]

Some of the overt resistance to indexing by active, stock-picking managers turned to covert concession. If, as was increasingly the case, a manager's performance was judged relative to an index such as the S&P 500, then there was some safety in selecting a portfolio that closely resembled the makeup of the index. Of course, doing so meant little or no chance of a dramatic overperformance. However, it also greatly lessened the chances of a career-killing relative underperformance: if one's portfolio did badly, those of other managers would most likely be doing badly too, so the fault would be seen to lie with the market, not the manager.

The pension fund and endowment trustees who employed investment managers also had to worry whether, with efficient-market theory becoming academic orthodoxy and beginning to influence regulators, they might be held to have behaved imprudently if they allowed stock portfolios to diverge too much from coverage of the overall market. A particular spur in this respect was the 1974 Employee Retirement Income Security Act (Whitley 1986a, p. 169). Section 404 of the act laid down the "prudent man" test: "A fiduciary shall . . . discharge his duties with the care, skill, prudence and diligence . . . that a prudent man . . . would use in the conduct of an enterprise of like character." (quoted by Brown 1977, p. 37) Following modern portfolio theory could be a defense against the charge of imprudence, while diverging too radically from its precepts might even leave one open to such a charge.

Increasingly, those who appeared to be active, risk-taking, stock-picking managers (and who charged the corresponding high fees) were in fact "closet indexers." Becker Securities regularly tracked the beta values of a sample of apparently actively managed portfolios relative to the S&P 500 index. A portfolio that tracked the index exactly would have a beta of precisely 1.0. From 1967 to 1971, the median beta was 1.09, indicating the taking on, on average, of somewhat more than simply overall market risk. By the end of 1974, the median beta was down to 1.07, and at the end of 1976 it was a mere 1.02 (Welles 1977, p. 51).

Among the consequences of the growth of index funds and of covert index tracking was that the Capital Asset Pricing Model's prediction that all investors would hold the same portfolio of risky assets gradually became less false than it had been when the model was formulated in the early 1960s. By 1990, for example, index funds made up around 30 percent of institutional holdings of stock in the United States (Jahnke and Skelton 1990, p. 6), with an unknown but probably substantial further proportion covertly indexed.

The growing sense that the findings of financial economics implied that one should simply "buy and hold the market" was possibly one reason why economists found the CAPM's "egregious" implication that there was only one optimal portfolio, the entire market, less shocking than Sharpe had feared, helping give him the confidence to abandon his earlier, strained alternative of multiple optimal portfolios. In that respect at least, the emergence of indexing meant that the world of investment practice came closer to that posited by finance theory.

Because a significant body of practitioner opinion came gradually to embrace at least some of the conclusions of financial economics, it is tempting to tell the latter's story within the familiar frame of scientific "discovery," practitioner resistance, and then eventual acceptance. Such a framing, however, would fail to capture the historical process involved in several respects. Most importantly, it would be misleading to present the development of financial economics as simply the discovery of what was out there all along, waiting to be discovered.

As the new financial scholarship emerged, theory was often in advance of empirical work. Markowitz's portfolio selection was prescriptive, not descriptive: it told rational investors what to do, rather than seeking to portray what they actually did. In the case of the Capital Asset Pricing Model, conceptual development preceded any attempt to test the model empirically. The random-walk hypothesis had some more directly empirical roots, but it was certainly not a simple empirical "fact."

Bringing finance theory into confrontation with reality turned out to be a complex matter. The results of empirical testing were often equivocal, and argument broke out over whether the Capital Asset Pricing Model could be tested at all. The efficient-market hypothesis seemed empirically the sturdiest of finance theory's central propositions, but it too began to encounter anomalies. "Reality" was not a stable backdrop against which testing could take place: finance theory's effects on its object of study were growing. Nor were the mathematical foundations of finance theory secure: as early as the 1960s they met with radical challenge. All these are the topics of chapter 4.

An Engine, Not a Camera

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